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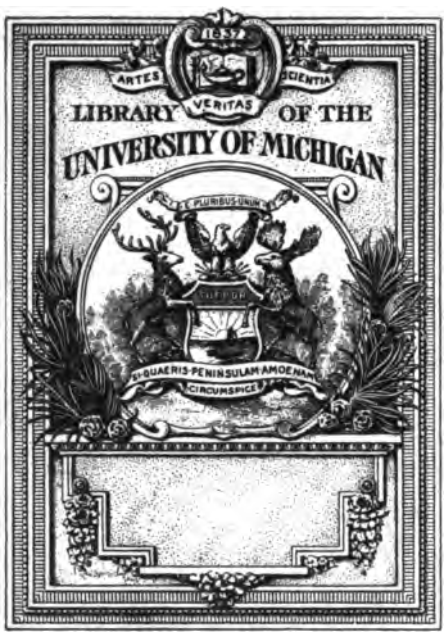
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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

JAMES BRAITHWAITE, M. D., LOND.

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CONTENTS OF VOL. LII.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE		AUTHOR.	PAGE
1	Treatment of Zymotic Diseases by the Alkaline Sulphites	*Dr. De 'Ricci*	17
2	The use of the Hypophosphites of Lime, Soda, and Potash in Remittent Fever, &c.	*Dr. H. S. Purdon*	20
3	On Contagion	*Dr. Lionel S. Beale*	22
4	On Ozone as a Disinfectant	*Dr. T. Moffatt*	28
5	What is Scarlet Fever?	*Dr. William Addison*	29
6	On the Treatment of Scarlet Fever	*J. L. Milton, Esq.*	34
7	On the Treatment of Gout	*Dr. H. Bence Jones*	35
8	On the Germs of Rheumatic Fever	*Dr. Horace Dobell*	40
9	On the Blister Treatment of Acute Rheumatism	*Dr. Herbert Davies*	42

DISEASES OF THE NERVOUS SYSTEM.

10	Cases of Reflex Paralysis, with Observations	*Dr. Alexander Robertson*	43
11	On the Treatment of Certain Forms of Paralysis by Galvanization and Faradization	*Dr. Julius Althaus*	46
12	On Progressive Locomotor Ataxy	*Dr. C. B. Radcliffe*	51
13	Cases of Pain of the Spine	*Dr. Henry Dick*	56
14	Case of Epilepsy with Insanity, cured chiefly by means of Ice to the Spine	*Dr. John Chapman*	57
15	Croton Oil in Neuralgic and Rheumatic Affections	*Dr. S. C. Sewell*	59
16	Neuralgia and Hypophosphites	*Dr. T. Hayes Jackson*	62
17	Obstinate Chorea Treated with the Calabar Bean —Encouraging Result—Paralysis Agitans Treated by the same	*Dr. John W. Ogle*	62
18	On a New Remedial Agent in the Treatment of Insanity and other Diseases	*Dr. S. Newington*	63
19	On the Treatment of Insomnia	*Dr. W. A. Hammond*	64
20	On the Hypodermic Injection of Morphia	*H. W. Freeman, Esq.*	65
21	On the Progress of the Hypodermic Treatment of Disease	*Charles Hunter, Esq.*	69
22	Danger of Subcutaneous Injections	*Prof. Nassbaum*	75

ORGANS OF CIRCULATION.

23	Cases Illustrating the Action of Digitalis on Enfeebled Hearts	*Dr. Gull*	75
24	On the Action of Digitalis in Disease of the Heart	*Dr. William Murray*	80
25	On Digitalis as a Cardiac Tonic	*Dr. Walter J. Henry*	80

ORGANS OF RESPIRATION.

26	On the Inhalation of Atomised Fluids	*Dr. Beigel*	82
27	A Suggestion in Aid of Laryngoscopy	*Dr. George Johnson*	85

ORGANS OF DIGESTION.

ARTICLE		AUTHOR.	PAGE
28	On Flatulence and its Successful Treatment by a Novel Use of Charcoal	*Dr. Arthur Leared*	86
29	On the Assimilation of Sugar by the Liver	*Dr. W. F. Pavy*	90
30	On a Case of Tapeworm (Tænia Solium) Treated Successfully with the Oil of Male-Fern	*Dr. John Constable*	93
31	On Azoturia	*Dr. E. H. Sieveking*	94

URINARY ORGANS.

32	On the Pathology of Diabetes	*Dr. George Harley*	97

SURGERY.

DISEASES OF BONES AND JOINTS, &c.

33	On Certain Errors in the Treatment of Joint-Diseases	*R. Barwell, Esq.*	100
34	On Amputation by Single Flap	*Henry D. Carden, Esq.*	103
35	On Amputation	*Prof. Fergusson*	106
36	On Amputation of the Thigh	*Do.*	110
37	On Fractures	*Dr. William Lyon*	111
38	On Operations on the Jaws	*Prof. Fergusson*	112
39	On Trephining for Compression of the Brain from Fracture of the Skull, with Remarks	*Bamlet T. Switzer, Esq.*	119
40	On Acupressure	*Dr. William Pirrie*	119
41	Cases in which, after Amputation, Acupressure was Employed for the Arrest of Arterial Hemorrhage in the Aberdeen Royal Infirmary	*Dr. William Keith*	121
42	On the Employment of Acupressure as a Hæmostatic Agent	*Dr. P. Heron Watson*	127
43	On Acupressure for Controlling Hemorrhage	*Prof. R. Macnamara*	133

ORGANS OF CIRCULATION.

44	Aneurism of the Popliteal Artery Cured by Digital Pressure in Twenty-six Hours and Forty-five Minutes	*Dr. George H. Porter*	135
45	On a New Mode of Tying Arteries, &c.	*Fred. Churchill, Esq.*	136

ALIMENTARY CANAL.

46	Radical Cure of Inguinal Hernia	*Dr. J. Fayrer*	137
47	On the Complication of Internal Hemorrhoids and their Treatment	*W. Allingham, Esq.*	140
48	On the Alleged Dangers of the Ligature in the Operation for Hemorrhoids; and the Comparative Merits of the Treatment by the Clamp and by the Ligature	*James R. Lane, Esq.*	141
49	On Polypoid Growths in the Rectum, and their Occasional Association with Anal Fissure	*Do.*	146
50	Persulphate of Iron in Hemorrhoids	—— *Cartwright, Esq.*	148

ORGANS OF URINE AND GENERATION.

51	On Lithotrity	*Prof. Fergusson*	148
52	On Lithotomy in the Adult	*Do.*	151
53	On the Treatment of Stricture	*Dr. Wm. Rutherford*	155

DISEASES OF THE SKIN.

ARTICLE		AUTHOR.	PAGE
54	On the Treatment of Prurigo	*J. L. Milton, Esq.*	157
55	On the Treatment of Infantile Strophulus, Prurigo, &c.	*Dr. Martin and M. Hardy*	159
56	On the Treatment of Alopecia	*J. L. Milton, Esq.*	159
57	On the Treatment of Tinea	*Do.*	160
58	On Eczema and its Treatment	*Do.*	162
59	On the Treatment of Psoriasis and Lepra	*Dr. T. M'Call Anderson*	170
60	On the Treatment of Acne	*J. L. Milton, Esq.*	174
61	Recent Researches on Scabies	*M. Gudden*	178
62	Large and Painful Carbuncle, Treated Successfully by Subcutaneous Section	*Christopher Heath, Esq.*	184
63	On a Cure of Ulceration of the Tongue which had Existed for Twenty Years	*Dr. Arthur Leared*	185
64	Solution of Venice Turpentine as a Dressing for Wounds	*M. Kerner*	186
65	On Ill-Smelling Feet	*Prof. Hebra*	186

DISEASES OF THE EYE AND EAR.

66	On Catarrhal Ophthalmia	*Henry Hancock, Esq.*	187
67	On the Yellow Amorphous Oxide of Mercury and its Application in Conjunctivitis and Corneitis Phlyctenulosa	*Dr. Pagenstecher*	189
68	Vascular Corneæ and Granular Lids of Nine Years' Standing—Cure by Purulent Inoculation after Peritomy	*J. Hutchinson, Esq.*	194
69	On an Improved Speculum for the Eye	*J. Z. Laurence, Esq.*	194
70	A Simple Mode of Examining the Fundus Oculi	*Dr. A. M. Roseburgh*	195
71	On Cataract	*Dr. A. Von Graefe*	195
72	On the Removal of Cataract by the Scoop Method, or the Method by Traction	*George Critchett, Esq.*	197
73	On the Extraction of Cataract by a Traction-Instrument, with Iridectomy; with remarks on Capsular Obstructions, and their Treatment	*Wm. Bowman, Esq.*	204
74	Electro-Therapeutical and Physiological Researches on Paralysis of the Ocular Muscles	*Dr. Moritz Benedikt*	210

SYPHILITIC DISEASES.

75	On Syphilization	*Prof. Boeck*	212

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

76	On the Facilitation of the First Stage of Labour	*Dr. Andrew Inglis*	221
77	The Active Constituents of Ergot	—— *Wenzell, Esq.*	225
78	On the Use of Ergot in Cases of Tedious Labour from Inefficient Action of the Uterus	*Dr. D. Dyce Brown*	225
79	On the Induction of Premature Labour in a Case of Deformed Pelvis	*Dr. Grasly Hewitt*	227
80	On Abortion in the Early Months of Pregnancy	*Dr. Charles Bell*	228
81	Case of Rupture of the Uterus; Abdominal Section; Subsequent Pregnancies	*Dr. Samuel S. Dyer*	229
82	On the Management of the Placenta in Natural Labour	*Dr. James Paterson*	231
83	On Electro-Magnetism in Post-Partum Hemorrhage	*D. W. Parsons, Esq.*	232
84	Remarkable Case of Deformity	*James Paget, Esq.*	233

ARTICLE		AUTHOR.	PAGE
85	On Hysterotomy	*T. Spencer Wells, Esq.*	234
86	On Puerperal Fever	*Dr. T. Snow Beck*	235
87	On Puerperal Fever	*Dr. Robert Barnes*	237
88	On Puerperal Tetanus	*Dr. G. H. Philipson*	239
89	Case of Puerperal Convulsions Successfully Treated by the Inhalation of Chloroform	*Dr. James Cuthill*	241
90	Case of Puerperal Tetanus following Abortion and Plugging of the Vagina	*Joseph Blackshaw, Esq.*	242
91	On Dysmenorrhœa	*Dr. Kidd*	243
92	On the Dysmenorrhœa, Metrorrhagia, Ovaritis, and Sterility associated with a Peculiar Form of the Cervix Uteri, and the Treatment by Division	*Dr. Robert Barnes*	248
93	On the Surgical Treatment of Painful Menstruation	*Dr. J. Henry Bennet*	251
94	On the Operation for the Relief of a Lacerated Perineum and Sphincter Ani, &c., with some of its Complications	*Thomas Bryant, Esq.*	253
95	Two Cases of Operation for the Cure of Laceration of the Perineum	*James Lane, Esq.*	254
96	On Vesico-Vaginal and Recto-Vaginal Fistula	*Thomas Bryant, Esq.*	256
97	A Radical Operation for Procidentia Uteri	*Dr. T. Addis Emmet*	257
98	On a Case of Ovariotomy in which the Cyst was in a State of Gangrene	*Thomas Keith, Esq.*	263

CHOLERA.

99	On Cholera: its Pathology and Treatment				265
	Do.	Do.	Do.	*Dr. G. Johnson*	265
	Do.	Do.	Do.	*Dr. G. Wallis*	269
	Do.	Do.	Do.	*Dr. W. G. Maxwell*	270
	Do.	Do.	Do.	*Dr. Massy*	270
	Do.	Do.	Do.	*Dr. C. W. Bell*	271
	Do.	Do.	Do.	*Dr. J. Hill*	274
	Do.	Do.	Do.	*Dr. P. Brady*	275
	Do.	Do.	Do.	*Dr. G. Plimmer*	275
	Do.	Do.	Do.	*Dr. Steadman*	276
	Do.	Do.	Do.	*Dr. Davies*	277
	Do.	Do.	Do.	*Dr. Bullar*	278

SYNOPSIS 280
INDEX 295

PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—TREATMENT OF ZYMOTIC DISEASES BY THE ALKALINE SULPHITES.

By Dr. De 'Ricci, Dublin.

[The arrest of zymotic diseases is at the present moment a most interesting question. How many diseases which attack us are owing to some mysterious fermentation of poisonous materials in the blood we are ignorant of! but we suspect that this is a more fertile source of disease than we have hitherto supposed. If we could stay all forms of catalytic action in the system we might arrest some of our most fatal diseases. We therefore direct particular attention to the two following papers by Dr. De 'Ricci and Dr. Purdon.]

That sulphurous acid had the power of checking fermentation, was a chemical fact that had been known for many years, and in wine-making countries had from time immemorial been employed to stop vinous fermentation when it had reached the required point; but that sulphurous acid possessed the power of stopping other catalytic action besides vinous fermentation, was not so well known, and the great fact that the alkaline and earthy sulphites possessed, in common with the acid, this same important quality was altogether unknown, until discovered and proved by Professor Polli of Milan, to whom the entire merit of the discovery is due. When the humoral theory of disease was in a way revived, and when the truth of a zymotic or catalytic origin of many diseases was all but universally admitted, I presume it occurred to many others besides Professor Polli and myself, that if a substance could be discovered which could neutralize the zymotic principle—the ferment—without, at the same time, being injurious to life, a remedy would have been discovered for all zymotic diseases, and as this class comprehends almost, if not all, the most fatal diseases to which man is liable, the value of such a discovery would have been incalculable. I believe, that in the alkaline sulphites we possess such a remedy, and to my friend Professor Polli is due the whole merit of the discovery. How the idea of employing sulphites instead of sulphurous acid, which cannot be administered, never occurred previously cannot be well explained, although, Claude Bernard's assertion that it was impossible to neutralize zymotic principles in the living frame without at the same time destroying its vitality, may have contributed somewhat to deter physiologists from trying experiments in that direction.*

Zymotic diseases are supposed to arise and to be propagated by a specific poison, which has the property of reproducing itself. A striking example of a zymotic disease we possess in small-pox; if a fraction of a drop of variolous infection is introduced into the circulation of an individual not proof

* Claude Bernard says, "Fermentation may arise in the blood, and give rise to poisonous principles, which may, in their turn, produce certain grave accidents in the living frame. * * * We cannot neutralize these ferments in the living organism—*it is impossible*; because, to effect such a purpose, it would be necessary to interfere with the character of the blood to such a degree that it would no longer be capable of sustaining life."

against such infection, that fraction of a drop will in the course of a few days multiply itself millions of times. This increase is supposed to be the result of an action resembling fermentation; and though this fermentation be somewhat different from that which operates in a brewer's vat, it is yet very analogous to it, and as it turns out, is subject in many instances to the same laws. It had been known for centuries that fermentation cannot continue in the presence of sulphurous acid; but it is a discovery of the last few years that the sulphites possess the same properties as the acid itself, and this fact has been established in many ways. Two animals of equal kind, size, and condition, were fed alike for a few days, with the exception that to *one* of them was administered a certain amount of a sulphite, until the urine passed off gave evident proof of the presence of the salt in the circulation. Both animals were then killed and exposed to the same conditions of heat, moisture, &c., &c. It being summer when these experiments were carried out, and the weather being warm, putrefactive fermentation very soon set in, but only in the animal to whom no sulphites had been administered; the *sulphurized* animal (if I may be permitted to coin a new name), on the contrary, still remaining fresh for many days after the other one had long passed into an advanced stage of decomposition. Other animals, after having been saturated with sulphites, were killed, and every portion of their body, solids and fluids, gave on examination unquestionable proofs of the presence of the sulphites employed. These experiments seemed to prove that when the sulphites, whether alkaline or earthy matters not, are introduced into the stomach of a living animal, they may be absorbed and circulated *as such* throughout the organism. This fact now established, the following deduction was unavoidable—that as no fermentation can take place in the presence of a sulphite, and as sulphites can be administered with impunity to living animals, and are absorbed *as sulphites* into the circulation, in a disease depending on a fermenting or zymotic action, we could arrest such action by saturating the system with one of these sulphites. Thus far this matter had been reasoned out well and satisfactorily, but facts were required to establish it, and to obtain these the following experiments were instituted:—Two dogs of same size and weight, and in good health, were fed perfectly alike for four or five days, excepting that to one was administered daily a certain quantity of bisulphite of soda. Having been thus prepared, some very fetid pus (about one drachm) obtained from an ill-conditioned abscess was then injected with great care into the femoral veins of each dog, and the results watched; immediately after the operation both dogs seemed stupefied; they laid down, refused all food, and remained prostrated for twenty-four hours. Both, however, seemed a good deal better on the following day, when a fresh injection of the same pus and to the same amount was again practised, both dogs being still fed alike, with the exception of the one to whom bisulphite of soda still continued to be administered in full doses. The effect of the second injection was most interesting. Both dogs were affected alike; both were seized with stupor; in both the pulse was rapid and feeble, while the respiration was greatly accelerated. Both dogs refused their food; both lay down in a state of stupor, and when made to rise and walk, tottered and reeled across the room. One dog, however, continued to receive daily a certain amount of sulphite of soda, and in four days was so far recovered as to be able to eat again his food with relish, while the wound in his femoral vein was healing rapidly. The other dog fared differently; he was fed equally, but he got no sulphites either before or after the operation, and the result was that he daily became worse; the wound in the thigh became gangrenous; the limb swelled up, and ten days after the second injection, the dog died with all the symptoms of typhus, the other dog being about and well. Upwards of three hundred dogs have been experimented on by Professor Polli and myself in this manner. We have varied the infecting substances by employing pus of different kinds—sanious matter, from ill-conditioned phagedenic and sloughing sores; we have employed defibrinated blood that had purposely been exposed for months to the air and become putrid; we have employed the discharge from the nares of glandered horses

—and we have always obtained the same result. I do not mean by this to assert that in every case the sulphurized animal has recovered, while the other has perished; but in the great majority of cases those animals to whom the sulphites had been freely and largely administered did well, while the corresponding animal in almost every experiment perished. These are facts which any one can try for himself; and now, what has been the result of these facts when applied to the treatment of disease in the human subject? Just one year ago I published the details of a most interesting case of septicæmia treated solely and entirely with bisulphite of soda, where death seemed to be imminent and the patient given over, and where the infection had clearly resulted from repeatedly kissing, for several days in succession, the dead body of a beloved friend far advanced in a state of decomposition. My friend Dr. B. M'Dowall, professor of anatomy in our university, has on several occasions tested the value of these sulphites in the treatment of scarlatina and small-pox, and always with satisfactory results.

My friend Dr. William Waters, of Carbury, treats all his cases of scarlatina with bisulphite of soda or magnesia, and not only administers it during the disease, but also as a prophylactic, with the greatest success, and last year fully established the safety of the remedy by administering it as a prophylactic to an infant three months old. Scarlatina of a very malignant type had broken out in the family of a gentleman in his neighbourhood; the eldest boy died of it in three days. Dr. Waters administered the sulphites to all the inmates of the house, including the infant. Some of the family escaped the disease altogether, and those who did take it had it in the mildest possible form. The first case in which Dr. M'Dowall tried this remedy was a most unfair one, it might be thought, but fortunately it turned out successfully. It was a case of confluent small-pox, which from collateral symptoms seemed to offer little if any hope of recovery; it was a case, in fact, in which nothing could be done, according to previous ideas. Sulphite of soda was prescribed in scruple doses every third hour, and to the surprise of the doctor and the whole class, the patient not only recovered, but recovered without a scar. Dr. Cummins, of Cork, has published in the "Dublin Quarterly Journal," vol. 77, a very interesting paper on the treatment of scarlatina by alkaline sulphites which he employed also as a prophylactic, and always with decided success.

I might add a great deal more, and cite cases of the most rebellious fevers treated at Tunis and other parts of the northern coast of Africa—fevers which, for intensity and malignancy, were scarcely if at all inferior to the Egyptian plague. These fevers have of late been all treated with bisulphite of soda and magnesia, and the success has been so great, the recoveries so marked, that the present sovereign of Tunis has publicly recognized Professor Polli's discovery by a handsome present and the decoration of an order of merit.

I might tell of the great success which has attended the local application of the sulphites in solution to putrid and phagedenic ulcerations, but shall content myself with quoting a short but most important letter which I received last spring from Dr. J. H. Purdon, of Belfast:—"Though personally a stranger, I cannot refrain from doing that which I am sure will give you pleasure, viz., informing you of the success of a remedy you have introduced to our notice, the bisulphite of soda. The cases in which it has been tried by us have all been puerperal, and in all, so far as I have been able to ascertain, it has proved successful. The first case I tried it in was with a primipara. The labour had been complicated with placenta prævia and consequent hemorrhage. On the third day she had rigors, which were quickly followed by rapid pulse, great prostration, profuse perspiration, fœtid diarrhœa, and facies hippocratica. Opium and mercury were freely tried, when, finding after five days that the patient was gradually sinking, I determined in consultation with my brother, whose patient she was, to administer the bisulphite of soda in scruple doses every four hours. From the third dose there was progressive amendment, all the bad symptoms subsiding. After a few more doses the lochia returned, and in a fortnight she was perfectly con-

valescent. The next case in which this remedy was tried was also a lady, but under the care of another physician justly in extensive practice. It had been a 'face to pubis' case, very tedious, and finally requiring the aid of forceps. On the third day symptoms of uterine disease set in, for which mercury, opium, and quinine were freely administered, and the gums became rapidly affected. Notwithstanding all this heroic treatment, matters had reached by the ninth day a most alarming height. The patient complained of but little pain, but she was greatly prostrated, and the pulse was 140, and very feeble. Bisulphite of soda was administered as in the previous case, together with opium, and quinine, first every four hours, and then every six hours. The following morning the pulse was at 130; in the evening of same day it had further come down to 106. The second morning it was at 96; the third morning 86. The bisulphite was persisted in all through, and the case ended in perfect recovery.

"A few weeks after I saw another patient, in whom the usual symptoms of puerperal fever of the typhoid type had set in on the fifth or sixth day. It was determined to lose no time with other remedies, but to administer the bisulphite at once in scruple doses every three hours. The pulse at this time was 140; by the next morning it had fallen to 120, when it steadily declined, and in a short week she was perfectly convalescent. Since these cases, my brother has had two more cases in the same locality as the first, in both of whom symptoms of the same character showed themselves. In both the same remedy was adopted, and in both the same successful result was obtained. I have heard of other successful cases in this neighbourhood amongst my professional friends, but can give you no details. I may add, however, that my brother and I have used it successfully both in anthrax and boils, which latter are very continuous in our jail, and we have found it to succeed admirably in 'putting a stop' to their recurrence."

I now conclude. I think I can safely assert, in the first place, that I have proved this remedy, the bisulphite of soda, to be perfectly *harmless*. It is not—like strichnine, arsenic, iodine, opium, or mercury—an instrument with double edge, cutting equally for good or for evil, according to the skill or the incapacity of the physician. It is perfectly harmless, for I have taken it myself, in *larger doses* than are required for therapeutic purposes, with perfect impunity. The physician, therefore, who wishes to try it, can do so freely, without fear of risk. Physiology leads to believe that all zymotic diseases depend on a fermenting or catalytic principle in the blood. Chemistry teaches us that in the presence of sulphurous acid and the sulphites no catalytic action can take place, and practical experience confirms the teachings of science, by showing us palpably that zymotic diseases, even in their most virulent forms, become completely neutralized by this remedy. Who, then, shall not try it, and bring it to the only test which can decide of its value—clinical experience? I stand at present nearly, if not entirely, alone in these countries as an advocate of this remedy; but if I am not mistaken, the discovery of the use of the alkaline sulphites in the treatment of zymotic diseases will one day rank with the greatest discoveries in medicine.—*Glasgow Medical Journal, Oct.*, 1865, *p.* 297.

2.—THE USE OF THE HYPOPHOSPHITES OF LIME, SODA, AND POTASH IN REMITTENT FEVER, &c.

By Dr. H. S. Purdon, Belfast.

[This paper corroborates the views of Dr. De 'Ricci, as seen in the previous paper. Dr. Francis Churchill, of New York, some time ago pointed out the value of the hypophosphites of lime, soda, and potash in phthisis, but we do not think that his practice has been followed up in this country. These medicines we think are more likely to be useful in all kinds of diseases caused by fermentation of poisons in the blood, such as typhus, typhoid, scarlatina, small-pox, &c., &c., and probably in cholera, and even in the cattle plague now raging in this country. The experiments of Dr. De 'Ricci, as shown in

the previous paper, prove that they have a power of arresting this action of fermentation similar to sulphurous acid in vinous fermentation.

A hypophosphite is very soluble in the animal fluids, and is readily admitted into the circulation. Sugar also occasions them to be more easily assimilated: these facts have been proved by the observations of Mr. Taylor.

As regards the actions of hypophosphites on the system, Dr. Churchill considers that "they increase the nervous force, and are the most powerful hæmatogens; possessing all the therapeutic properties of phosphorus." They also seem to increase the appetite, check any tendency to diarrhœa, and occasion cheerfulness in children who have long been fretful and peevish.

In remittent fever occurring in children, great benefit is derived from their use, even in cases which seem at first sight to be hopeless; and where we have the tongue loaded with a creamy exudation, dark coloured circles beneath the eyes, aphthæ in the mouth, distended abdomen, offensive diarrhœa, &c., the hypophosphites act like a charm, when other remedies have failed.

In giving the hypophosphites in the disease known as tabes mesenterica, rapid improvement does not take place; but it is nevertheless sure although slow. As regards their action in this disease, they appear to have the power of dissolving the tuberculous matter deposited in the abdominal viscera, possibly by causing disintegration of the fibrine.

It will be necessary, before using the hypophosphites, to improve the character of the biliary secretion; but in cases of simple gastric irritation I would have no hesitation in commencing their use at once. It is a curious fact, that children who are fretful, restless, and a torment to all within hearing distance, quiet down after a few doses have been administered.

The following formula is suitable for an infant, the dose being increased according to age:

℞. Hypophos. sodæ gr. vi.; hypophos. calcis, hypophos. potass. ana gr. iv.; glycerinæ ʒii.; aquæ ʒi. M.
Forty drops, three times a day, in a little water.

The accompanying cases have been selected for the purpose of illustrating the therapeutical effects of the hypophosphites, after failure of the usual remedies, in some of the diseases occurring in infants and children.

Sugar may be combined with the hypophosphites, as it fulfils a twofold object, viz.:—disguising the taste, and rendering the hypophosphites more soluble, and for this purpose I think that the sugar of milk of the British Pharmacopœia is the most suitable.

Case I.—*Remittent Fever.*—M. F., aged eighteen months. Tongue covered with a creamy exudation, dark-coloured circles beneath the eyes; aphthæ in the mouth; abdomen hot and distended; diarrhœa with stools of a most offensive odour; extremely fretful and peevish. Alteratives were tried, but without any improvement; but after recourse was had to the hypophosphites, in a few days marked symptoms of improvement presented themselves.

II.—*Remittent Fever.*—M. J., aged five years, a very delicate boy with a pale countenance. For some time had been very restless at night, with continual picking at the nose. These symptoms gradually merged into remittent fever; tongue of a strawberry appearance, stools very offensive, occasional convulsive seizures, and considerable thirst. The hypophosphites in this case were commenced at once, and in a fortnight the child was convalescent.

III.—*Remittent Fever.*—Wm. W., aged two years; symptoms—abdomen tympanitic, alvine evacuations dark-coloured and offensive, thirst, vomiting, countenance stupid and heavy-looking, pupils dilated, period of remission lasting about one hour. Treatment—he first had powders containing pulv. cretæ, sodæ siccat., and hydrargyri c. cretæ; but these not producing the desired effect, hypophosphites were commenced. The diet during the disease consisted of milk and lime water, with the occasional use of arrowroot.

IV.—*Tabes Mesenterica.*—M. S., aged five years, began to "waste away" after an attack of measles. She presented all the appearances of a strumous diathesis; thorax on being examined was found healthy, and there was no cough. The abdomen felt rather hard, stools of a whitish colour; tongue clean for a few days, would then become foul, and remain so for three or four days; appetite fair, and her hair comes away in the comb. This child was put upon the use of the hypophosphites, and in about three weeks' time was convalescent. One rather curious fact about this case is, that although the hypophosphites effected a cure of the tabes mesenterica, still they possessed no specific action over the formation of tubercle in other organs, as this patient died in about sixteen months from phthisis.

V.—*Gastric Irritation.*—Mr. J——'s child, aged eight months. Continual vomiting of a sour curdled-looking matter; nothing would lie on the stomach; very feverish and fretful. The gums were lanced: and for the vomiting the child got powders containing bismuth, calomel, and compound powder of chalk; but as the child gradually became worse although various means of treatment were tried, the hypophosphites were commenced, and the child is at present lively and healthy.

VI.—*Chronic Gastric Irritation in the Adult.*—Mrs. F. had for some time complained of irritation and pain in the stomach, increased on pressure; very restless at night, bowels irregular, tongue red and raw-looking, great thirst. The usual remedies, such as bismuth, various preparations of magnesia, &c., were tried without avail. Considering it a good case to try the hypophosphites, they were administered with the best results.

As it is impossible to cut short a disease such as remittent fever, the Latin proverb, *medicus curat, natura sanat morbum*, strikes us with full force, directing the physician to assist nature with as little fatigue to the patient as possible; the hypophosphites seem to fulfil all these indications, in causing sleep where slumber has been lost or disturbed, in relieving thirst, cleansing the tongue, increasing the appetite, correcting any intestinal disorder which exists, and this in a much shorter time than can be obtained by other remedies.—*Glasgow Medical Journal, October*, 1865, *p.* 319.

3.—ON CONTAGION.

By Dr. Lionel S. Beale, F. R. S., Professor of Physiology and of General and Morbid Anatomy in King's College, London; Physician to King's College Hospital.

Although the subject of *contagion* may seem to have little connection with inflammation, I find that the consideration of the properties of living pus leads me into the discussion of the nature of contagious diseases, and I shall therefore venture to offer some remarks upon this most interesting subject.

Of the Vitality of Pus.—The vitality of the germinal matter of pus is exceedingly great. Pus multiplies very rapidly, and certain forms may be carried from one living organism to another without being destroyed—without losing their vitality. It is most interesting to notice that the faster the pus grows the more varied the conditions under which its life may still be carried on. Certain conditions which would have destroyed the life of the germinal matter of an epithelial cell may not destroy that of a pus corpuscle; the pus corpuscle may indeed flourish under conditions which would render the existence of the germinal matter of a normal cell quite impossible; and this power of existing under various conditions seems to increase as the multiplication of the mass of the germinal matter of the pus corpuscle proceeds. The capacity for living and growing under a variety of different conditions may proceed to such an extent that germinal matter originating in the organism of an animal may grow and multiply in that of man, and *vice versâ*. And we know that certain contagious diseases originating in animals may pass to man, and it is not improbable that some of those emanating from the human subject may be communicated to animals.

As I have stated before, this power of living is so remarkable that the pus

corpuscles, or portions of the pus corpuscles, may be transmitted through the air without loss of vitality. It is in this way may be explained, I think, certain phenomena known in connection with the propagation of purulent ophthalmia and other diseases in which pus is carried from one person to another. The pus corpuscles produced on the surface of the conjunctiva, or, rather, the minute offsets from them, may be transported through the air, or by clothes, &c., and, coming into actual contact with a conjunctiva properly prepared by morbid processes for the supply of nutrient matter to these corpuscles, they grow and multiply. In this way pus corpuscles, or portions of pus corpuscles, perhaps not the fifty thousandth or the one hundred thousandth part of an inch in diameter, may be transferred from one organism to another. Allied facts may be observed with reference to the propagation of gonorrhœal pus, and I venture to infer that the poison of syphilis is really germinal matter, which passes in a living state from the infected to the sound organism, and grows and multiplies in it. Many of the highly important facts recently discovered by Prof. Boeck support this view.

Certain living particles which have directly descended from the living or germinal matter of the tissue or fluids of an unhealthy organism may be transmitted in a living state to a healthy organism, and, being there supplied with proper pabulum, grow and give rise to the production of new particles like themselves; and although the nutrient material may be somewhat different, yet they grow and multiply, as they are capable of living under a variety of different conditions. The action of vaccine lymph is, I think, due to the growth and multiplication in the vaccinated person of living particles which existed in the lymph taken from the vaccine vesicle. Nor is there good reason for denying the possibility of such living particles retaining their life when perfectly dried, since we have the striking fact that many of the lower organisms retain their life although desiccation has been carried to a very considerable extent. We know that complete desiccation cannot occur without producing the death of any living particles; but comparative desiccation may actually occur, and the small amount of living matter remaining may be sufficient to induce the changes with which we are familiar.

The Nature of the Poison of Contagious Fevers.—I think the above genera views may be extended to fevers, which, I believe, are due to the passage from one body to another of living particles. These living particles consist of germinal matter, and have descended from the germinal matter of organism itself. Many have attributed diseases of this class to the introduction into the body of a very low form of vegetable organism. But it is very doubtful if the growth and multiplication of vegetable germs can proceed in the circulating fluids of a living body without causing death in a very short time; for the conditions favourable to their existence and multiplication are incompatible with the life of the germinal matter of the higher tissues. The germination and multiplication of very low vegetable or animal organisms in man and the higher animals are an indication not only that the death of the tissue has taken place, but that it is passing into a state of decomposition. It is true that bacteria have been detected in the blood of patients during life, but hitherto only in cases shortly before death, at a time when the blood in which they grew and multiplied was no longer fit for nourishing the tissues, and was itself passing into decomposition. Millions of bacteria exist in the softened outer portions of the fibrinous clot of an aneurismal sac, and, therefore, in such close proximity to the blood, that it is almost certain that, from time to time, some pass into the current of the circulation; but if this were the case they would be destroyed or else so altered that they would cease to multiply. For, before multiplication could take place, changes must have occurred in the composition of the blood which would render it incapable of supporting the life of its owner.

Many of these contagious maladies have been regarded as different species of "foul air" diseases. They are commonly termed *zymotic* or *fermenting diseases*; but no one has shown that these poisons originate in air, or that they really are of vegetable origin. Nor is it probable that they are a species of animal—a sort of parasite, as some have supposed. Nor, there is good

reason to believe, would such poisons have ever arisen if man had known the exact conditions necessary to ensure a healthy state of existence. Man has produced these pests himself by establishing, without due care, artificial conditions of existence in which over-crowding, dirt, and terribly diminished supply of air play no unimportant part. Nor let us conclude too hastily that these are *necessary* visitations. The results of scientific observation and practical experience tend to show that such scourges are preventible. Not only so, but there is much to convince us that many of these diseases are not only fostered and propagated, but actually caused, in the first instance, by ignorance, obstinacy, and indolence in domestic management.

The above considerations seem to me to lead to the conclusion that the *materies morbi* concerned in the propagation of various contagious diseases is not a low form of animal or vegetable organism, but germinal matter which was originally derived from that which exists normally in the higher organisms. The living germinal matter forming the *materies morbi* of contagious diseases bears somewhat the same relation to the normal germinal matter of the blood, lymph, or tissues, that a pus corpuscle does. Like pus, it results from the too rapid growth of germinal matter. It is much to be regretted that the question cannot be definitely put to the test of experiment, but it will be admitted by all that the conditions which are favourable to the propagation of these contagious diseases are the very conditions which we should imagine would be favourable to the maintenance of the vitality of such masses of living matter as are supposed to be the active agents. If it were possible to produce this class of diseases in frogs or newts, I would almost undertake to make out the nature of the morbific agent as well as the changes which take place during the course of the disease; but as our observations are necessarily limited to man and the higher animals, in which minute investigation is remarkably difficult, much that I shall say will be but conjecture, although my conclusions are supported by many considerations.

It is not to be supposed that any one unacquainted with physiology would believe that a terrible and fatal malady could arise from the introduction into the body of a minute germ weighing less than the $\frac{1}{100000}$th of a grain; but to those familiar with the *vital* phenomena many facts will at once occur which afford considerable support to such a doctrine. The multiplication of bacteria and low animal and vegetable organisms, so very minute and transparent, that they are scarcely visible under a power magnifying 3000 diameters,—the subdivision of pus and mucus corpuscles which may be *seen* under the microscope,—not to mention the wonderful powers residing in the infinitesimal amount of matter constituting the spermatozoon, are facts which must at once destroy the validity of objections grounded upon the minuteness of the particles of living matter supposed to be the active agents.

The origin, transmission, and effects of the different contagious poisons which affect man and animals will, no doubt indeed, be regarded as *mysterious* and *inexplicable* by those who support the dogma, that living things, like lifeless ones, are influenced by physics and chemistry only. But those who study the phenomena of living beings from a more general point of view will see that these phenomena fall into the same category as many other actions and changes peculiar to living beings. By attentively studying effects we may reasonably hope to learn something of the nature of the forces concerned in their production; but if we adopt the arbitrary dictum that all the phenomena of living things are due to physical and chemical changes only, we shall not be likely to progress very far in such an inquiry as the present one.

The vitality of the Poison of Contagious Diseases.—The fact of the poison of the contagious diseases retaining its activity for some time after it has been detached from the organism in which it was produced is not, as would at first, perhaps, appear, opposed to the view that this poison is living germinal matter directly descended from normal germinal matter; for certain kinds even of normal germinal matter retain their vitality for some time after they have been removed from the organism in which they were produced. I have

demonstrated that the cells of the liver and other epithelial cells retain their vitality after they have been transferred to a glass slide, and a piece of skin and some other tissues may be detached, and their temperature very much reduced, without destroying their life; for if they be replaced in their proper position they reabsorb nutrient material and again become an integral part of the organism from which they had been removed; proving that the masses of germinal matter retained their vitality during the whole time they were separated—or they may even be removed from one organism and made to grow upon another organism of the same kind. As is well known, parts of the body may be exposed to extreme cold for a considerable time, so that sensation is destroyed and the circulation stagnates, without the death of the germinal matter of the tissues taking place; and as is well known, the death of many textures of the body does not occur until many hours after the cessation of the heart's action and respiration.

But there is still a stronger argument. We have the positive fact that pus will live for a considerable time in urine, and therefore we are quite justified in assuming that other kinds of germinal matter allied to pus, and capable of living under a still greater variety of conditions than pus, would retain their vitality in urine, fæces, and other excretions, as numerous facts establish is the case with regard to some forms of contagious matter. As we have positive evidence that minute particles of pus may pass through the air, or remain on sponges, clothes, &c., for many hours without their vitality being destroyed, is it not reasonable to assume that the living matter of contagious diseases, supposing it to be allied to pus, should retain its vitality under the same or under yet more adverse conditions? Does not this view receive confirmation from our experience with regard to the conditions which are favourable to the propagation of contagious fevers? A warm, moist atmosphere, small close rooms, with curtains, carpets, and plenty of clothes, rags, &c., so arranged as to cause air to be pent up in confined spaces with very slow interchange,—are the circumstances which favour the spread of contagion; and a disposition to prevent currents of air from finding their way into every part or into any part of an apartment is a mental characteristic of many of those who are too often sufferers from the worst forms of contagious diseases; while has not experience taught us that the converse of all this almost certainly prevents the spread of contagion, destroys the active material, or renders its assaults perfectly harmless? And would not free change of air, washing, frequently shaking of clothes, extreme cold, dryness, or a very high temperature be likely to destroy the vitality of such particles of living matter as I suppose to be the active agents in the propagation of contagious diseases?

It is not probable that the particles of germinal matter grow and multiply when removed from the living body. Multiplication could not take place as they are passing through the air; nor is it likely that their process could occur in particles attached to dry clothes. But the question whether such particles may grow and multiply in moist substances, as in excrementitious matters, remains open. It would be most interesting if pus or any allied form of germinal matter could be caused to multiply artificially, and the fact would doubtless excite much highly valuable speculation; but at present the experiments instituted for the purpose of determining this question have not afforded satisfactory results. No doubt multitudes of the germs of many contagious diseases are very frequently coming in contact with us; but, instead of multiplying at the expense of our nutrient juices, they die and become harmless. But, as is well known, there are conditions of the system which are favourable to the growth and multiplication of such disease-producing germs, and it is for us to ascertain exactly what these conditions are. There is much more hope that we may learn how men may be enabled to resist contagion or be protected from its influence, than that we shall ever be able either to extirpate the contagious poison, or even to confine it within a certain area. Moreover, the circumstances adverted to in this paper render it almost certain that if every disease-producing germ in existence could be destroyed at once, new ones—perhaps not of quite the same nature as those

now existing, but very closely allied to them—would be generated. Although a state of society in which such care would be taken of the public health as would entirely prevent the production of disease-producing germs is theoretically conceivable, it is practically impossible that such a state could be established in the present condition of the civilised world.

It seems to me improbable that we can distinguish the different kinds of specific disease-producing living particles from one another. I have endeavoured to show that every kind of living matter exhibits certain common characters, and that the living matter of a high and complex organism is not to be distinguished from that of a low and comparatively simple one, by any peculiarities we can recognise, on submitting the matter to the various methods of examination at present known. The living matter of the tissue-destroying pus corpuscle resembles that from which the normal tissues are produced. It is only in the rate at which such masses multiply—the pabulum they are capable of taking up—the products resulting from their life and death—that the several kinds of living matter can be said to differ from one another, and these phenomena are all dependent upon the agency which I have spoken of as vital power. In fact, it appears probable that the living matter, which is the *materies morbi* of contagious diseases, differs from the living or germinal matter of ordinary pus and that of normal tissues, or the white blood corpuscles, &c.—not in form, in force, or in property—but in vital power. I think that if we consider the difficult problem from this point of view we shall learn much from new work; but if we continue to regard these poisons as gases, or vegetable or animal organisms, or as ferments, or as inorganic matter, we shall never succeed in adding to our knowledge of vital phenomena. Most assuredly we should never cease to inquire until we have discovered the real nature of these terrible diseases which it is our duty to try to prevent, to mitigate, or to cure; for until we are acquainted with the exact nature of a disease, are we not at a great disadvantage in attempting to treat it?

[These contagious poisons doubtless produce their peculiar influence upon the organism through the blood, in fact, that they pass into this fluid, grow, and multiply there, giving rise to changes in its composition.]

Admitting that contagious poisons generally consist of living matter, and that this living matter enters the blood; let us inquire how the living particles may enter the vascular system where there is no inoculation—no solution of continuity of the vascular walls. We may explain the fact that of a number of persons equally exposed to the influence of a contagious poison, some will contract the disease while others will escape, by supposing that in the latter case the living particles are prevented from passing through the vascular walls, or that, coming into contact with the blood, they do not grow and multiply, but become destroyed.

How, then, do the living particles pass through the vascular walls and enter the blood, and what are the circumstances that would favour or interfere with their passage? Thin-walled capillary vessels, as is well known, come very near to the surface in many parts of the body. Those of parts of the mucous membrane of the nose, mouth, and conjunctivæ, even in a state of perfect health, are covered with a very thin layer of protective epithelium, while those of the air-cells of the lung are practically bare. In many morbid states the epithelial covering of the mucous membranes enumerated above is very soft, and sometimes it is reduced to a thin layer of a soft pulpy mucus, in which any foreign particles would very readily become embedded. It is reasonable to suppose that the contagious germs find in this soft mucus a nidus favourable for their reception, and at the same time soluble materials adapted for their nutrition. They would grow and multiply, and probably minute offsets from them would soon find their way to the external surface of the capillary walls.

I have shown that there is strong reason for concluding that when the capillaries are much stretched, as when fully distended with blood, particles of living germinal matter, probably derived from the white blood corpuscles, may make their way through the walls with some of the serum, and grow and multiply in their new position. Thus, an "exudation" will in many instances

contain particles of living growing matter, and these particles absorb nutrient matter, and soon become the spherical granular cells which are often seen external to the vessels in inflammation. They do not become aggregated together, as Dr. Bennett supposed, but each absorbs nutriment, and, just as in the case of all other masses of living matter, grows. and divides and subdivides. This view concerning the origin of many of the corpuscles in exudation is supported by the fact that in pneumonia and in some other conditions in which the capillary distension is very great, red blood-corpuscles, which are much larger than the particles of living matter just referred to, pass through the capillary walls without complete destruction of the vessel taking place. Every one who has made very minute injections is familiar with the fact that minute longitudinal rents or fissures may be made quite wide enough for a red blood-corpuscle to pass through edgeways. It is therefore certain that particles of minute size may pass through capillary walls without the occurrence of actual rupture.

Besides which, there are at very short intervals in the capillary walls masses of germinal matter or nuclei, which increase considerably in size when supplied with increased nutriment. These may divide and subdivide, and give rise to collections of little granular corpuscles, as seen in cases of inflammation of the pia mater, and also in cases of tubercular disease of the same membrane. In the situation of these nuclei the walls of the vessel are probably weak and liable to alterations, as they increase or diminish in size. Hence there is no difficulty in accounting for the passage of minute particles of the living contagious germinal matter in cases in which the capillary walls have been unduly stretched and then rendered somewhat flaccid. The little particle of contagious matter having gained entrance into the blood, probably absorbs nutrient material rapidly. It is possible that fibrin may become coagulated around these little bodies just as if they were pus corpuscles, and the mass being too large to pass, becomes impacted in some part of the capillary system. The large size of the white blood corpuscles in very many blood diseases is also a fact not generally known, although of great importance, which I will discuss elsewhere. The nutrition and multiplication of the germinal matter would proceed and give rise to secondary phenomena familiar to all of us, and of the utmost interest; in those organs in which the circulation is slowest—as the spleen and liver—the conditions would be very favourable to the multiplication of such particles of living matter. I must not however, pursue this part of the inquiry any further on this occasion,

Now, the state of things referred to above—a soft moist state of the mucous surfaces, a dilated condition of the capillaries, combined with a weak, flaccid state of their walls, which would follow long-continued congestion, and which is ultimately connected with a weak heart's action and feeble condition of the nervous system, are those which would facilitate the passage of such living germs as I am supposing, and are not these the very conditions which exist in the organism about to be the victim of a contagious fever? No doubt in these cases the composition of the blood is altered and its fluid constituents manifest a tendency to permeate the vascular walls more readily than in a perfectly healthy state. This state of blood would doubtless affect the action of the nervous centres presiding over the contraction of the arterial walls, and regulating the flow of blood through them, thereby influencing the nutrition of the part, and in this way a relaxed state of the arterial walls and a congested state of capillary vessels might be induced, or already existing, might be increased. It is this low and but too often ill-defined weak state of health that we should endeavour to detect by most careful observation, and at once treat. Nay, it seems to me almost certain that every serious acute disease dangerous to life to which we are subject is preceded by a condition of system which in many particulars is a departure from health. If this can be altered, the liability to the supervention of the acute attack no longer exists. It is in the direction of anticipating the occurrence of actual serious well-marked disease that those most earnest in advancing medicine may reasonably hope to do useful work. And it seems certain that the more minutely we investi-

gate, the more likely shall we be to learn how to discover and to appreciate that slight departure from the healthy state which precedes, and often by some considerable time, the development of many of the most serious and most fatal maladies.

I will try to sum up, in as few words as possible, the most important of the conclusions at which I have arrived with reference to contagious diseases.

The *materies morbi* of contagious diseases does not consist of lifeless organic or inorganic matter, nor of any form of gas or vapor generated in the decomposition of animal or vegetable substances, nor of any matter set free during the decomposition of fæcal or other excrementitious matter of animal origin; nor is it any species of animal or vegetable organism or parasite; but the active contagious material consists of exceedingly minute particles of living germinal matter which may be regarded as the direct descendants of the germinal or living matter of an organism which has been for some time living under unusual conditions. Contagious poisons affecting men and animals have originated in their organisms. The living or germinal matter of some contagious disease originating in the bodies of animals may grow and multiply in man, and *vice versa*. These particles of living germinal matter may retain their vitality for some time after they have escaped from the seat of their formation. They may pass through the air or be preserved in clothes, or various fluids, or moist solids. The smallest particle (less than the $\frac{1}{100000}$th of an inch in diameter) being introduced into the body already in a fit state for its nutrition, may grow and multiply, giving rise, in due time, to the symptoms characteristic of the particular disease, and producing myriads of particles like itself. But it is probable that such particles, being introduced into a perfectly sound organism in a state of perfect health, would not grow and multiply, but would die, or, in other words, such an organism would resist the influence of the contagious matter. Some of the germinal matter forming the *materies morbi* of certain contagious diseases may retain its vitality for a considerable period of time in a comparatively dry state, like vaccine lymph. Knowing what we do of the protecting influence exerted by vaccination, it seems probable that the ravages of many other contagious diseases besides small-pox may be mitigated or prevented by the inoculation of certain forms of contagious matter which would produce allied but much less severe forms of disease. It seems desirable that numerous experiments should be instituted on cattle with the object of ascertaining if any such protective influence would be really exerted.—*Medical Times and Gazette, Sept.* 9, *and* 23, 1865, *pp.* 273, 329.

4.—ON OZONE.

By Dr. T. Moffatt, Hawarden.

In *The Times* of August 26 and 30th, there were two letters on "Cholera and Ozone," in which directions are given for the production of ozone by the use of phosphorus, and the latter is recommended as a disinfectant. For upwards of seventeen years Dr. Moffatt has used phosphorus for producing ozone, and for five years he has employed it as a disinfectant.

In a paper "On the Luminosity of Phosphorus in connexion with Atmospheric Conditions," which I read at the Cambridge meeting of the British Association in 1862, I stated that phosphorus was a valuable disinfectant, but that it was effective only during its luminous condition, as ozone is formed only when it is luminous.

The luminosity and non-luminosity of phosphorus are influenced by atmospheric conditions. High pressure, a low degree of temperature, and wind from the north points of the compass are the conditions of non-luminosity; and low pressure, high temperature, and wind from south points of the compass are the conditions of luminosity. The former atmospheric conditions are those of cholera periods, and cholera disappears with the setting in of the latter.

About the 1st of September, 1853, cholera appeared in Newcastle. On the

20th the number of deaths was—cholera, 108; diarrhœa, 10. On the 19th the south, or ozoniferous, current of the air set in and continued. On the 28th the number of deaths reported in *The Times* was—cholera, 18; diarrhœa, 2. From the 1st of September to the 19th the mean quantity of ozone was scarcely 1·0 of my scale; but from the latter date to the end of the month it ranged from 3 to 8 daily. The epidemic in London in 1854 disappeared with the setting in of similar atmospheric conditions.

For purifying apartments I use phosphorus in the following way:—I take a quart bottle with a wide mouth, into which I put rather more than half a pint of water; a piece of cork carrying a flat piece of phosphorus with a clean cut surface, floats upon the water. The mouth of the bottle is loosely covered with a cork. The bottle is then placed, first in one part, and then in another, of the apartment to be purified until the peculiar smell of ozone is detected, or until my test papers indicate 1 of my ozone scale. The process of purifying may be performed night and morning, or oftener. For purifying air in the neighbourhood of street gratings or in sewers, I simply suspend a piece of phosphorus from the grating. In apartments the temperature may be sufficiently high to keep phosphorus luminous under all atmospheric conditions; but in sewers it will be luminous and non-luminous according to the height of the barometer, the temperature of the surrounding air, and the direction of the wind, and ozone will be produced only when it is luminous.—*Lancet, Sept.* 9, 1865, *p.* 303.

4.—WHAT IS SCARLET FEVER?

By Dr. William Addison, F. R. S., Brighton.

The order of events observed in scarlet fever is as follows. First, fever; secondly, a rash upon the skin, sore-throat, and swelling of the glands about the throat; thirdly, exfoliation of the cuticle, mucous discharges from the throat and nose, and perhaps abscess of the submaxillary glands.

This order or succession is typical of the order of events as they take place in the blood, which we make out to be as follows:

1. Infection of the corpuscles by a miasm inhaled through the lungs. This is announced by symptoms of fever.

2. The corpuscles of blood being disordered by a specific poison, the waste materials which they discharge into air and into the liquor sanguinis are altered in quality and quantity. Less carbon is expired by the lungs (Graves), and less urea discharged by the kidneys (Simon); but a new contagious poison is developed, which gives the liquor sanguinis pathological properties—renders it impure.

3. Impurity of the fluid of blood excites local inflammation, which in scarlet fever is seen in the skin, throat, and glands about the throat. The local inflammations comprise the production of a vast brood of epidermal and mucous, or of epidermal mucous, and pus-cells.

4. These cellular productions are discharged from the skin by exfoliation of the cuticle; from the throat, by its mucous discharges; and from the glands about the throat, by abscess and suppuration. That they carry off a *materies morbi* from the blood seems proved; because the exfoliating cuticular cells and the breath of the patient are vehicles of a contagious poison. Moreover, all symptoms of fever and inflammation disappear upon their discharge.

5. The actions in the skin, throat, and glands about the throat are called *critical*, because, if any interruption or check be given to them, the task of eliminating the poison from the blood is thrown upon the cells of the secreting surface of the kidneys or other internal organs, to the great danger to life.

The rash and affection of the throat in scarlet fever are forms of inflammation; and the action in the glands about the throat comprises abscess and suppuration? Can these things be beneficial?

If a thorn pierce the flesh and remain there inflammation and abscess ensue. The action loosens the foreign body, so that it can be easily removed. A slough cannot be detached without suppuration. In a comminuted

compound fracture, loose pieces of bone are detached by inflammation, suppuration, and granulation. *Granulations* and *pus* are two forms of cell-growth (inflammation, abscess, and suppuration, are simply words applied to the attendant phenomena).

In severe boils and carbuncles, dead connective tissue lies beneath the living skin; and it is only by inflammation, abscess, suppuration, and granulation, that it can be loosened and discharged. The work is transacted without any loss of blood. Hundreds of blood-vessels are first sealed up; and then these and other bonds of connection are severed by the absorptive properties of the cells of granulations and pus; whereupon, the slough is easily removed, and the wound heals or is cured spontaneously.

In necrosis of bone, the meaning of inflammation and suppuration is the loosening and detachment of the dead bone. Here the work to be done lies deep beneath a mass of living muscles, and is necessarily an extremely difficult one, requiring assistance from the hands of the surgeon. Nevertheless, to the philosophical observer, the intent of the natural efforts is plain and evident. In this example, so difficult is the work, that a host of material is spoilt in it; hence the necessity of conduits or sinuses for its escape. Should the surgeon try to close the outlets and succeed, nature resents his interference and speedily forms others; but if he remove the dead bone—even if it be with gauges, saws, and chisels—his interference is placidly accepted, and the whole chain of phenomena subside into cure, which includes the growth of a new bone.

Without further multiplying examples, it may be said generally: In all parts of the body if dead and injurious matter is to be got rid of, inflammation, abscess, suppuration, and granulation, are the means. And if they are so, they are the means of cure. It is upon these visible examples of cure that we base the principle of Cell-Therapeutics.

In like manner, in fever, where hurtful matter must obtain outlet from the blood, some more or less pronounced form of inflammation, some unusual cell-production is observed. For what are the facts?

In small-pox, there are hundreds of abscesses in the skin, every one of them a focus of cell-growth. The blood is continually passing the abscesses or pustules; and it improves in quality, or becomes better fitted for its normal purposes, as they increase in size and fulness. Growth of the pustules and a beneficial change in the qualities of blood are concomitant events. For, if the maturation of the pustules be checked, the symptoms of the patient become worse. Therefore we say the pustules flourish and multiply by the absorption or morbid matter from the fluid of the blood. Similarly, in scarlet fever, myriads of little foci of an unusual cell-growth are established with phenomena of inflammation in the skin. The exuberant morbid cuticle produced is composed of millions of adherent-cells, whose growth is concomitant with a healthy change in the blood. With the change in the blood, the conditions which encouraged preternatural cell-growth are exhausted; the growth ceases; inflammation subsides; the morbid cuticle exfoliates or sloughs off; and the patient is cured.

No distinction can be shown between the cells of laudable pus—those of a healthy suppuration, which are loosening and fitting for discharge a foreign body, a slough, or dead bone—and the deep cuticular cells of the skin, which, in scarlet fever, in their outward progress, multiply and grow upon morbid elements in the fluid of blood. Nor can any difference be shewn between these cells and those which exfoliate in abundance mixed with the mucous discharges from the throat and nose. The doctrine of cell-therapeutics is illustrated and confirmed by phenomena of exanthematous fever.

In scarlet fever, it is exemplified in exfoliation of the morbid contagious cuticle, which is established by rash on the skin; and the rash is a form of inflammation.

These are reasons why no one thinks of curing, in the sense of suppressing, the pustules of small-pox, or inflammation in the skin in scarlet fever. And when the submaxilliary glands swell and inflame and tend to abscess, it is well known to be wrong to resort to strong measures to suppress it. These

outward manifestations, so far from constituting the disease, are actions necessary to the cure of the disordered blood. It is in harmony with our doctrine that, in all cases of scarlet fever where there are no complications, experience has shewn that nothing more is necessary in the medical treatment, than:

To keep the patient warm in bed, in a well ventilated, airy chamber; to relieve him—having reference to the work going on in the blood—from pain, restlessness, and other discomforts;

To take precautions that the diet be cooling and simple;

To watch and regulate the function of all those organs which give outlet to excretionary matter from the blood—the skin, kidneys, lungs, and bowel: and to guard against the spread of contagion.

The sequence of events in the blood in scarlet fever, as we have described it, is natural and logical. A poison in the air causes disorder of the corpuscles of blood; disorder of the corpuscles causes distemperature or impurity of the fluid in which they swim; and impurity of the fluid of blood causes unusual cellular growths, with phenomena of inflammation, the cellular growths discharging the impuriy.

It corresponds with the order of the symptoms as they come on and as they go off. At the onset, fever precedes the rash, and the rash precedes exfoliation of the cuticle; and, in the going off, fever subsides when the rash is at its height, and the rash subsides when the morbid cuticular growth has performed its task, or is prepared for exfoliation.

But it is to be observed that, in proportion as disorder of the corpuscles diminishes by their throwing off morbid matter, in like proportion impurity of the fluid of blood is increasing, and the fluid of blood recovers its normal condition in proportion to the activity of the local cell-growths which flourish upon the morbid matter in it.

Relief of the corpuscles devolves a pathological state upon the fluid of the blood; and relief of the fluid devolves a pathological state upon the skin, upon the skin and throat, or upon the skin, throat, submaxillary gland and kidneys. That is to say, cure of the corpuscles transfers morbid matter to the fluid of the blood; and cure of the fluid of blood involves the transference of morbid matter from it to some local part by the agency of cells. These statements contain no more than the received physiology of blood. In respiration, the corpuscles transfer poisonous matter—some form of carbon—to the air; and a healthy state of the fluid involves the transference of poisonous matter—urea—to the kidneys. And here is the most important point of this discussion.

We must recognize the mixed character of the actions of cure. They are physiological and pathological at the same time. The pustules of small-pox are physiological to the blood, but pathological to the follicles of the skin; as shown by the deep pits and scars remaining after cure. The rash and double cuticle of scarlet fever may be regarded as disorder to the skin, but healing to the blood.

In fever, one part recovers at the expense of another. The corpuscles of blood infected by an aerial poison recover at the expense of the liquor sanguinis; and the liquor sanguinis recovers at the expense of some local part.

These observations have their bearing upon the affection of throat in scarlet fever. There can be no doubt of this being part of the critical actions in that species of fever. If so, it is physiological to the blood, but pathological to the throat itself; and we must preserve the power, or relieve the pain, of swallowing. According to our view the object of gargles, poultices, warm fomentations, leeches, or a blister, as applications to the throat, is not to cure the inflammation, and thereby suppress the increased mucous discharges from it; but to moderate and limit the actions going on with reference to the comfort of the patient in swallowing. This being done, cure of the throat will take place with cure of inflammation in the skin; and cure of inflammation in the skin takes place when the fluid of the blood has rendered up its morbid material.

It would be to ignore the teaching of the microscope, did we hesitate to declare our conclusion. Cure, whether of external injuries or of scarlet

fever, is wrought by cells with characters as fixed and specific as any of the other things of Nature. No person, at his will, can command the growth of skin or bone, or furnish a safe outlet for poisonous matter in the blood. Nevertheless, these things are accomplished; and our researches point out the agents employed; viz., *various broods of cells*.

The physician and surgeon each in his department can watch and, in a measure, regulate the work—remove hindrances; promote or moderate, encourage or discourage, the preliminary actions; but the essential elements of cure—cellular bodies with their rapid multiplication, growth, metamorphoses, and deep interior vital properties of sedative absorption—are not producible by manual art or physic.

The orderly course of the majority of cases of scarlet fever—the fixed characters and place of the local actions, and the relief which these afford to the patient when not exceeding their proper limits—demonstrate that the disease has a specific method of cure. It requires vigilant medical supervision, because of the mixed nature of all the actions of cure—partly physiological and partly pathological; and because of the difficulties and complications so apt to arise.

The history of every outbreak of the disorder shows how various these complications are in different persons; how they protract or interrupt the actions of cure; and how often they cause its failure. These complications spring up from various quarters. Usually they will be found based upon one or other of the following grounds.

Because some previous malady in former years has crippled an important organ, and thereby entailed upon the individual what is called "delicacy of constitution."

Because, from unwholesome diet or overcrowding in unwholesome air, the blood is impoverished and weak at the onset of fever.

Because a chill or other accident has interrupted the specific local action and its cellular exfoliations; or,

Because, from some degenerate anatomical lesion, spoiled matter has ebbed back into the liquor sanguinis, and redisturbing the corpuscles of the blood, has renewed symptoms of fever.

What is meant by the crippling of important organs as a source of complication in fever?

If a child be severely burnt about the face and neck, the wound occasioned by the injury heals, cure is effected; but an ugly scar remains, which is permanent, and greatly circumscribes the movement of the head or mouth. Similar things may be said of a severe burn at the elbow or upon the fingers.

A comminuted fracture may be cured: but the injured limb never recovers the strength of the sound one. If a fracture passes through a joint, the injury may be cured, but the joint never recovers it natural mobility.

Pleurisy and inflammation of the lungs are cured; but the respiratory organ does not recover its pristine condition.

And so most other internal maladies may be cured, and yet the organ the seat of them never recovers. Cure and recovery are terms of different meaning.

All the most recent pathological investigations demonstrate in every internal malady, particularly in children, that, though one organ or texture has suffered more severely than the rest, from which therefore the disease derives its name and most prominent features, yet it is not there that the whole disorder is found. Anatomical changes are not confined to that organ, but are more or less extensively present in others also. When, therefore, we speak of infants dying of teething, convulsions, or pneumonia, or of youth dying of measles, hooping-cough, or scarlet fever, it is necessary to bear in mind that these designations convey no information whatever as to the ramifications of the disorder. It is from examination of the dead that we see, besides the organ from which the chief symptoms of the malady arose, that several others are the seat of minor anatomical changes which contributed to the fatal issue.

This fact being demonstrated in the fatal forms of illness, there is no reason for doubting it in the more numerous cases where health is regained. On

the contrary, there is every reason to conclude that what are called complications and sequelæ of fever are the more prominent of these minor anatomical changes gaining strength and evolving symptoms.

When, then, we discourse of infants recovering from teething, convulsions, or pneumonia, or of youth recovering from measles, hooping-cough, or scarlet fever, it must be borne in mind the term recovery can mean no more than the relief of the patient from the chief malady and all its pronounced collateral anatomical lesions. Things latent—anatomical changes which give no sign—may, with the recovery, return to their natural state; but it does not necessarily follow they do so. On the contrary, the permanent scars and cripplings which remain after the cure of severe burns or other surgical injuries, and the pittings of the skin in small-pox, render it almost certain that what is called "delicacy of constitution" is the expression of the non-recovery of one or more important internal textures which have been cured of the malady that affected them, but which have not been restored to their free natural state. As when pleurisy has been cured, but the lung does not recover, it remains adherent to the wall of the chest; or as when rheumatic fever is cured, with permanent thickening of the coverings of the heart; or pneumonia cured, with consolidation in some part of the lung. All these and such like conditions are positive grounds for more or less constitutional infirmity; they are important items in the history of future fever, and give aptness to the popular saying: "He has never been well since the measles, hooping-cough, or scarlet fever." Organs in these weakened conditions may not betray their weakness by moderate using; but if put to the slightest unusual stress, the defect is proclaimed.

"I do not exaggerate," says an eminent pathologist, "when I state that four-fifths at least of the old men, whose organs I examine after death, present evident incontestible traces of disease, not of recent disease, but of a former malady." Again, another says: "It is not after birth only that man, for the first time, experiences those maladies which afflict his race. Their origin must be sought in a remoter source. They commence with organisation; and the annals of medicine present a number of facts which attest that children may be born healthy, sick, or recovered from former disease." In the Children's Hospital at Vienna, it is calculated that not more than 20 per cent. of the inmates are free from some form of constitutional infirmity. Whenever the nutrition of the child has been imperfect, disorders associated with poverty of blood become widely diffused, materially influencing the mortality at an early age, and if the age of childhood be survived, affecting the future health of youth and manhood.

When, then, we speak of a patient being cured, or of his recovering, it is necessary to agree upon what is meant by the words. Cure does not mean that all the organs or textures implicated in the disorder—all the anatomical changes incident to it—have returned to their natural state; for, though we may presume, we cannot know in the living body that such is the case. No affirmation can be made respecting thickenings, scars, puckering, and cicatrices, in internal organs, which have no present symptoms other than those comprised in the terms delicacy of constitution.

The term recovery is applied to persons who have regained health sufficiently to return to their accustomed avocations; and the physician dismissing his patient hazards no opinion about the state of organs which give out no symptoms, but which, nevertheless, may be a source of complication, should hereafter scarlet fever make its attack. These observations receive illustration from the history of scarlet fever itself. It frequently happens the patient to all appearance has recovered, but the kidneys have not regained their natural state; a little time elapses, and their weakness is proclaimed by dropsy—the dropsy depending upon the enfeebled condition of the kidney, and the enfeebled condition of the kidney arising from the just passed fever.

Lastly, as regards these internal anatomical disablements, it has been remarked of external wounds which have healed and cicatrised—that is to say, have been cured—that when subsequently fever or other maladies arise, the scar of the old wound is very apt to become painful, and a focus of renewed

inflammation. Forms of inflammation are necessary to the cure of exanthematous fevers, and an internal puckering or cicatrix may complicate the course of scarlet fever, by taking on some irritative morbid action. Sanitary proceedings and dietetic rules will not efface the permanent marks of the cure of former maladies, nor restore to their natural vigour damaged organs.—*British Medical Journal, Oct. 7, 1865, p. 364.*

6.—ON THE TREATMENT OF SCARLET FEVER.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases.

For many years, plans of treatment have been before the world, which, so far as we can judge, ought to have almost as effectually defeated the inroads of this malady as vaccination has defeated small pox; yet week after week and year by year this relentless scourge sweeps away its victims, not by hundreds, but by thousands. There must be something strangely wrong in a system which presents such a sad and startling contradiction as this.

Long ago I endeavoured to bring under the notice of the profession a mode of treatment, which had proved very successful in Germany, but the attempt met with no response except from Mr. Erasmus Wilson, Dr. Routh, the late Dr. Snow, and one or two others. It consists in daily rubbing with hot fat or lard, and the manner in which it checks and quells the violence of the disorder is, if the accounts given be true, most extraordinary. Death is a rare result; the patient never takes cold; dropsical symptoms are almost unknown, no infection is given, because the sources of it are mechanically closed, the distressing heat and irritation of the skin are abated, and, according to Dr. Routh, the pulse will fall thirty beats in three hours.

All that needs to be done is to rub the patient over from head to foot with fat—lard or fat bacon is the best material, and the salt should be removed as far as possible. The rubbing is to be done once at least, twice if thought proper, in the twenty-four hours. No harm can come from rubbing in too much; a great deal may ensue from rubbing in too little. The patient wants no washing, may wear the same clothes all the time, and requires little medicine and no precautions beyond those which common observation would direct.

The next best plan, but yet seemingly behind it in efficacy, is that of cold affusion, a plan attended with such benefits that one is puzzled to know how it ever came to be abandoned. Bateman lingers and dwells with fervid energy upon the wonderful soothing powers of cold affusion, the great febrifuge as it has been justly called. For ten years, he says, he had used Dr. Currie's plan, without once seeing inconvenience, much less injury, from it.

Mr. Wilson considers sequi-carbonate of ammonia to be a remedy, which in its power over scarlet fever and rubeola may be ranked with quinine in ague, iron in erysipelas, arsenic in eczema, and sulphur in scabies. I think myself that the success recorded to have followed the use of this remedy is among the most striking triumphs that discovery can boast, and its success in small-pox is equally cheering. Dr. Witt says, "It is the only remedy yet known which appears to have any decided effect upon this terrible malady;" and Mr. Wilson adds, "My own experience of the sequi-carbonate of ammonia fully corroborates the value of Dr. Witt's suggestion; and I feel that the adoption of this method cannot be too forcibly pressed upon medical men." In scarlatina the success has been marvellous. Out of several hundred cases, one gentleman did not lose more than one in a hundred. Mr. Henry Jackson, of Sheffield, with his father, treated patients in this way for fifty years, in which time they did not lose more than four patients, and they died from typhus following the scarlatina. Another gentleman reports that he rarely lost a patient during twenty years' use of the remedy; whereas his successor, who rejected the treatment by ammonia, lost seventy-four out of a hundred and six cases in a single epidemic. These results contrast strongly with the havoc this dreadful complaint used to make under the treatment with antiphlogistics, two, three, and even four or five deaths in a single family, and which I am much inclined to believe, from what

I saw of it in my younger days, never saved a single bad case, if it even did not hasten the progress of thousands to the grave.

It does not seem necessary to prescribe anything with the ammonia. From three to six or seven grains, according to the age of the patient, may be given at first every hour or two, till a decided effect is produced upon the disease, after which it may be taken less frequently. Simple or cinnamon water seems to be the favourite vehicle, but nothing can answer better than camphor mixture. In some slight cases, where there is not much difficulty in swallowing, ammonia may be given in the form of an effervescing drink, the carbonate in solution being mixed with lemon-juice.

Belladonna has obtained a high reputation on the Continent, not only for its curative but its preventive powers in scarlatina. Some of the German physicians seem to think that it is the best remedy in use; one of them goes so far as to say that it may be considered as effective against scarlatina as vaccinia is against small-pox. Even supposing this view to be overdrawn, enough still remains to show that belladonna does exert a most powerful control over scarlatina, and that there is every reason to believe, if it were freely and judiciously used at the outbreak of an epidemic, the number of fatal or even serious cases would be materially diminished.

It may be given in the form of extract dissolved in water, sweetened and flavoured with orange, cinnamon, or nutmeg; about three grains of the extract to an ounce of fluid are perhaps the best proportions. Of this solution two or three drops may be given to an infant under a year old twice a day, three or four drops to a child between one and two years old, and from this time of life a drop may be added for each succeeding year.

These, then, are the remedies which the experience of very successful practitioners warrants us in selecting as the most reliable—cold bathing, rubbing with fat, ammonia and belladonna. They are few, but potent, and all the more valuable because they are few. All the other means, such as gargles, aperients, &c., are only auxiliaries, for the use of which no general rule can be laid down, but which the experience of every one capable of profiting by experience will teach him how to use.—*Milton on Diseases of the Skin, p. 73.*

7.—ON THE TREATMENT OF GOUT.

By Dr. H. Bence Jones, A. M., F. R. S., &c.

In the present state of our knowledge the treatment of gout may be divided into two parts: first, the specific; and second, the expectant treatment. Each method has its own advantages and disadvantages, and by the right use of both proceedings the greatest good with the least harm may be attained.

The specific treatment aims to get rid of the attack as soon as possible; that is, to put an end to the pain at once; to stop the fibrinous and crystalline thickening of the joints, and to allow the patient to return to his usual mode of life in the shortest possible time.

The disadvantages of this treatment are, first, that the specific sometimes acts more violently than is intended; and secondly, that cutting short an attack leads more quickly to a return of the disease, because the usual mode of life is that which has caused, and therefore will again cause, the gout; and thirdly, that the urate of soda existing in excess in the serum and diffusing into all the textures, where it is oxidised, if stopped in its active oxidation in any part, is more liable to set up a process of active oxidation in the fibrous textures of some internal organ, as the stomach, the brain, or the heart.

The expectant treatment aims to keep the gout fixed in the extremities until the textures and blood are freed as much as possible from the urates by oxidation, and to effect a long-continued change for the better in the usual mode of life, at least during the illness; and from both these causes a long interval is likely to intervene between the fits of gout.

The disadvantages of this treatment are, first, that the patient may get very feeble from the long-continued inflammation and confinement, and that

out of this, other ailments may thus arise; secondly, that the joints, in consequence of the duration of the inflammation, may become much more thickened by fibrin and urates than they otherwise might be, and that thus also the general health may give way from the loss of exercise.

The value of the specific treatment is seen when some more important organ than a joint is attacked by violent gouty or rheumatic inflammation. Then, to avoid irreparable injury to the organ, the specific inflammation must be stopped as quickly as possible. Acute inflammation of the eyes furnishes by far the most striking example of the use of colchicum. By the specific treatment the eye may be saved; by the expectant treatment it will be lost; and the activity of the specific treatment must bear a direct proportion to the intensity of the inflammation. In a slight attack the expectant treatment alone may be required, but in a violent outburst the most energetic specific treatment, even with the help of local or general bleeding, will scarcely be able to preserve the sight of the inflamed eye.

The specific treatment consists in giving colchicum, veratria, sabadilla, and different quack medicines the composition of which is unknown, but which probably contain one or other of these remedies.

By far the most popular specific is colchicum; and if the action of this substance was fully known, the action of all the other gout specifics would be understoood.

There is no question that the alkaloid colchicine is the active substance in colchicum; and, though this substance is not yet isolated and analysed, there are good grounds for belief that it closely resembles veratrine, perhaps as closely as quinine does cinchonine.

Colchicine $=CxHyN_2Ox$.
Veratrine $=C_{64}H_{52}N_2O_{16}$.
Strychnine $=C_{42}H_{22}N_2O_4$.
Quinine $=C_{40}H_{24}N_2O_4$.
Cinchonine $=C_{40}H_{24}N_2O_2$.
Morphine $=C_{34}H_{19}NO_6$.

It is very evident that the analyses of these different alkaloids give no clue whatever to their medical properties; and so the analysis of colchicine will give no explanation of the mode of its action. But, however much these alkaloids differ in their action, they all have this in common, that they act energetically on the nervous system.

The problem to be solved is, how does colchicum stop the excessive oxidation set up in the parts where the urates are most liable to accumulate, partly, perhaps, in consequence of the non-vascular nature of the textures?

That colchicine acts as a diuretic, and throws the uric acid out of the blood, is not capable of proof, and if it were would not give any explanation of its action on the joint. That colchicine promotes the oxidation of the uric acid by directly furnishing oxygen, as peroxide of lead might do, is contrary to all chemical probability. There remains the unproved assumption that colchicine, like these other alkaloids, acts on Dr. Liebreich's protagon that the alkaloid has a chemical action, by actual contact with the nerve substance of the blood vessels, where the gouty inflammation is going on, causing such an action on the vessels that the inflammation ceases.

This assumption makes the action of colchicine comparable with the action of veratrine, strychnine, and morphine, all of which act with intensity on the nervous substance; and this assumption leads to a conjecture as to the action of quinine, that it also may act on the nerves of the blood-vessels, stopping the internal congestion, on which the external rigor of ague depends. The deafening and prostrating effects of large doses of quinine can arise only from an action on the auditory and cardiac nerves, and the arrest or prevention of an ague fit can best be comprehended by the assumption that quinine acts on the nerves of the capillaries, stopping congestion; whilst colchicine acts on the same system of nerves, stopping inflammation.

M. Roudanowski has stated to the Académie des Sciences that, after poisoning animals by strychnine, nicotine, opium, and chloroform, he has detect-

ed by the microscope changes in the nerve cells or in the myeline; but his observations cannot yet be received as conclusive.

In M. Laville's specific, veratrine is probably the active alkaloid; most probably a small amount of morphine is also dissolved in the wine with which it is prepared. It is said to be made from sabadilla seed; but whatever the source or nature of the alkaloid, it must have an action corresponding to colchicine, and the good and the harm from this specific cannot be much more nor less than the good and the harm of colchicum. It cuts short the attack, and thus saves the injury to the joints from the continuance or severity of the inflammation; but it does this by stopping the oxidation of the urates in the textures, and hence a relapse or renewal of the inflammation more quickly and easily takes place.

Moreover, like all other specifics, it gradually loses its effect when it has been used in many attacks, and one at the least of those who have been its most public supporters has abandoned it for some fresh, though far less potent, remedy.

Used in half-drachm doses every four hours, or one-drachm doses twice or thrice daily, it acts often most decidedly. If anything, it causes less action on the stomach, and more action on the bowels than colchicum, and from the disposition to drowsiness which it frequently occasions, probably the irritative action is moderated by some opiate.

There can be little question that colchicine used by subcutaneous injection into the neighbourhood of the inflamed part would put an end to the attack; but the danger of an overdose would, perhaps, be greater than when taken in by the stomach, and the benefit derived from its use would not compensate for the fear of its abuse. It would probably act more quickly and more strongly than when taken into the stomach, but this rapid relief would be gained at much greater risk to life from the alkaloid acting on the nerves of the heart and causing dangerous syncope. Moreover, relief from pain, and time for the full action of the colchicine when taken by the mouth, may be obtained by the subcutaneous injection of one-fourth or one-third of a grain of acetate of morphine in ten or twelve minims of water into the neighbourhood of the inflamed joint.

Late one evening I was sent for to a gentleman suffering with a most violent attack of gout in the knee; the heat and the pain were excessive. There was some swelling and some redness. The attack had begun in the previous night, and had increased in intensity all day. I injected the third of a grain of morphine into the inside of the thigh near the knee, and gave a drachm of Laville, ordering another drachm to be taken when he awoke. He slept for upwards of six hours without waking, and he seemed very certain that but for the injection the night would have been passed in even more agony than the day. After four drachms of Laville the attack subsided, and the following night no morphine was required.

In sciatica the use of the subcutaneous injection is most beneficial; sometimes almost whilst giving the morphia the pain subsides, and in eight or ten minutes drowsiness comes on, and sound and continuous sleep follows; but the alkaloid acts more strongly than when taken into the stomach, and an overdose may easily be given, and the poison is beyond the reach of an antidote.

The expectant treatment consists in preventing the accumulation of the urates in the serum and textures, and in promoting its elimination and oxidation.

The accumulation of urates is greatly to be prevented by a strict diet, chiefly as regards the quantity of animal and vegetable albuminous food that is eaten.

The elimination of urates is best effected by washing the serum and textures—that is, by increasing the flow of urine by means of water or diuretics. When the serum is comparatively free from urates, then the urates in the textures diffuse back into the blood and may be thrown out.

The oxidation of the uric acid in the textures and blood can be effected directly or indirectly by air, diet, and medicine.

So that the expectant treatment consists of diet, to prevent accumulation

of urates, and to promote its oxidation; diuretics to promote its elimination; and air and medicine to promote its oxidation.

During and after an attack of gout, when thickening of the joints exists, medicines that promote the absorption of the effused fibrin and serum are very useful. Of these iodine externally and iodide of potassium with alkalies internally are the most efficacious. Iodine, perhaps, acts indirectly as an oxidiser.

But first of the diet:—

Let us suppose that the minimum diet of health may be represented by 10·5 grammes (162 grains) of soluble nitrogen, and 240 grammes (3696 grains=8 ounces nearly) of soluble carbon—

If one ounce of meat when dry contains 108 grains of nitrogen and 33 grains of carbon; or,

If one egg contains 118 grains of nitrogen and 62 grains of carbon,

And one ounce of butter contains 355 grains of carbon,

And 5 ounces of dry bread contain 232 grains of nitrogen and 464 grains of carbon,

And 21 ounces of arrowroot contain 3633 grains of carbon,

Then it is easy to see that 2 ounces of dry meat, or 2 eggs, or 5 ounces of bread will give more nitrogen than the system requires to repair ordinary losses, and the excess may have to be thrown out, partly in the form of urates, when the oxidation is insufficient to produce urea.

If 2 ounces of dry meat or 2 eggs only are taken, about 10 ounces of butter would be necessary to furnish the fuel and power for the system, or an equivalent quantity of fat must be absorbed from the stores deposited in the cellular tissue. Or, if 5 ounces of dry bread only were eaten, then above 9 ounces of fat must be taken up in twenty-four hours; but 2 ounces of dry meat, or 2 eggs, or 5 ounces of bread, requiring the wasting of the body by 9 or 10 ounces of fat daily, could not long be endured. Moreover, these quantities of nitrogenous food produce more uric acid than a less nitrogenous diet would do.

If a little more than 21 ounces of dry arrowroot were taken daily, this would supply more carbon than is lost in the daily oxidation. The excess of carbon must either be deposited as fat, or must take away the oxygen, so as to leave none free to act on the nitrogenous substance that is passing out from the albuminous textures. Hence, with carbonaceous diet in excess, the whole of the uric acid from the tissues might pass off through the blood unoxidised.

It follows, then, that in gout a minimum of albuminous food should be taken in order to produce the least uric acid; and a minimum of carbonaceous food in order to allow the uric acid to be oxidised as much as possible.

If a pint of ordinary beef-tea, free from fat, contains 46 grains of nitrogen and 160 grains of carbon, and if an ounce of dry arrowroot contains 363 grains of carbon, then 3½ pints of beef-tea and 9 ounces of arrowroot will contain the full amount of elements that are required even in health for the system, and far less than this would be desirable when the minimum production of urates is required.

Secondly, as to air.

In oxygen the energy exists, without which all our heat and all our power, and, indirectly, if not directly, all our growth would stop. The nitrogen of the air probably only dilutes the oxygen to the strength we can bear. Some experiments of M. Regnault's show that in less dilute oxygen more of this gas is not inhaled than in our atmosphere. M. Pettenkoffer will probably give us more accurate knowledge of this subject, and enable us to see the amount of intermediate oxidations going on within the body, by comparing under natural conditions the total amount of oxygen that goes in with the amount that comes out in the carbonic acid and water that are the products of the ultimate oxidation only.

The use or uselessness, in the treatment of gout, of the inhalation of oxygen and of ozone can be determined only by the most careful experiments. At present, voluntarily increased rapidity of respiration by exercise (blowing

the fire, in other words) is likely to add more oxygen to the blood than strengthening the mixture of oxygen in the air.

The oppressive and debilitating effect produced by the respiration of air heated to the temperature of the blood proves that diluting the oxygen by expansion produces a very decided effect on the oxidising action in the body. The hotter the air, the less will be the amount of oxygen in a given space; and the colder the air, the more oxygen will the lungs obtain. Moreover, the diffusion of the gases in the vesicles and the bronchial tubes must be greatly influenced by the different temperatures of the different parts of the column of air. The purer the air, the less carbonic acid, and the less mineral, vegetable, and animal impurities will it contain in the form of dust. Any increase in the amount of carbonic acid in the inspired air produces a chemical impediment to the escape of carbonic acid from the blood, and the passage of smoke and dust into the air vesicles themselves mechanically interferes with the actions of solutions and diffusion on which respiration depends.

When there is reaction, cold air (or cold water) promotes oxidation; but continued external cold directly or indirectly causes contraction of the vessels of the surface, and drives the blood within; and hence the greatest oxidation takes place by keeping the circulation free on the skin and extremities by the warmest possible clothing by enveloping the inflamed part in cotton and oil-silk, or in flannel.

Thirdly, on the action of medicines in promoting oxidation, elimination, and absorption. For indirectly promoting oxidation the two most potent medicines we possess are alkalies and iron.

Alkalies assist in the oxidation of organic substances, not by giving oxygen, but by promoting the formation of vegetable and animal acids, and ultimately forming carbonates out of the burning matter. Familiar instances are the saponification of fats, the use of caustic potash in Trommer's test for sugar, the use of lime in Moore's test; the use of potass in Liebig's method of determining the amount of oxygen gas by pyrogallic acid.

Hence caustic alkalies, potass, soda, magnesia, are more efficacious as oxidisers than carbonates, and carbonates more potent than bicarbonates. But even bicarbonates, when heated with organic substances, lead to the formation of organic acids, which displace the carbonic acid, and thus promote the oxidising action. Thus diabetic sugar with sulphate of copper and carbonate or bicarbonate of potash in the cold gives no reduction, but when heated to 212° the reduction is evident. Ammonia is at least partly oxidised in passing through the system, and therefore it is of no avail in the treatment of gout as an oxidiser, though it may be used to neutralize acid in the stomach, or to act as a temporary stimulant.

Iron indirectly is an oxidiser, by its power of drawing oxygen into the blood. The great object of giving iron as medicine is the formation of blood-globules. For this hœmato-globulin itself would probably be the best preparation, as being nearest in composition to the substance ultimately formed; but hitherto all attempts to prepare even hæmatin in quantity as a medicine have failed, and dried colouring matter of the blood is the simplest substitute. The reduced metal, the oxide, the carbonate, or some vegetable salts, are usually given, and all with good effect. The large amount of iron that passes off in the fæces and urine shows how little metal attains the intended object. In ten minutes I have found a solution of sulphate of iron passing out in the urine, and in twenty-four hours in the fæces. Hence very small doses of hæmato-globulin would probably be equivalent to the large doses of other iron medicines which are given. In the active state of gout iron of any kind is inadmissible; in the passive state it becomes a most efficacious remedy.

All the salts of the organic acids that pass through the stomach into the blood, as, for example, the acetates, tartrates, citrates, are oxidised and pass into the urine and textures as carbonates; and hence all effervescing saline draughts may be considered as acting in the tissues as the carbonates of the alkalies act; that is, they promote the formation of organic acids in

the textures; and these acids pass by the absorbents, and by the veins back into the blood, to be oxidised and thrown out by the urine and by the skin.

The mineral waters, reputed good for gout, are mostly dilute solutions of alkali—*e. g.*, Vichy water. Generally the stronger the water the more efficaciously it acts, but the action of the water itself must not be disregarded—*e. g.*, Buxton; the water diffuses not only into the urine, making that more dilute, but it dilutes the serum of the blood, and diffuses into the textures, dissolving deposited urates, and hindering fresh deposition of crystal. Thus water alone in sufficient quantity acts not only as a diuretic, but as a dialytic remedy, and in gout the action of the water of mineral waters might be still more exactly defined as urilytic. The water takes the uric acid as urates from the textures and carries it to the blood to be oxidised, and thrown out as urea and carbonic acid.

In the gouty deposits, however, there is another deposit—namely, fibrin, which is by no means soluble like the urates, for water and alkaline carbonates have but little action on the deposited fibrin. How far iodine, bromine, and mercury possess the power of indirectly or directly oxidising fibrin has yet to be determined. In the present state of our knowledge, nitrate of potass is of all remedies the one which can pass most readily into the joints, and can exert a solvent action on the fibrin that may be deposited there.—*Med. Times and Gazette, May* 27, 1865, *p.* 530.

8.—ON THE GERMS OF RHEUMATIC FEVER.

By Dr. Horace Dobell, Physician to the Royal Infirmary for Diseases of the Chest.

[Cases of acid dyspepsia are seen every day. The symptoms are as follow:]

1. Pain at the epigastrium, coming on within an hour after food, generally within half an hour, very often within a few minutes, especially excited or aggravated by cheese or malt liquor.
2. Urine very acid, of high specific gravity, high coloured, and depositing urates on cooling, and uric acid crystals after standing some time; often suddenly changing to limpid and low specific gravity, but quickly returning to the other condition.
3. Tongue red at the tip, with elevated papillæ; often red at the edges also.
4. Appetite usually good, often very good, especially for animal food.
5. Epigastrium tender on pressure.

Such are the features of the majority of the cases, of course complicated and modified by the existence of other diseases, the habits of the patient, &c.

The most marked of these is the time at which pain begins after food; it is very soon after. Sometimes we find an old neglected case in which the pain comes on later, even an hour and a half, or, rarely, two hours after food; but if it be a case of acid dyspepsia, you will find, on close inquiry, that when the symptoms first set in, the pain was directly after eating; and that the time of its onset has become later and later. In these cases, too, the appetite will be bad, instead of good; and the food will often be vomited. They are, in fact, exceptional cases, in which the digestive power has become paralysed by the neglect of the original disease. With these exceptions, if the pain does not begin till more than one hour after eating, the case is not one of acid dyspepsia. To demonstrate these exceptions, I may mention to you Case 64,297, domestic servant. She complained of pain in the pit of the stomach, beginning from one to two hours after food, with much flatulence; but said that when it began, five weeks before admission, it came on much sooner after food; and as the complaint yielded to treatment, the pain came on sooner and sooner after food, till it ceased to come at all. In Case 64,525, the food was brought up undigested; and in Case 64,207, there was no appetite and no taste for food on admission; but when the complaint began, two months before, the appetite was very good.

So much, then, for the symptoms of this complaint. The next point is as simple and as striking as the symptoms; viz., that the cases almost invariably get well in a few weeks under a very simple plan of treatment.

1. We order them to take out-of-door exercise, and to attend to the condition of the skin.

2. We forbid them to take malt liquor or cheese, and allow whisky or brandy instead; and advise water to be drunk freely.

3. We neutralize the secretions by soda and potass in full doses, with lemon juice or citric acid; then follow this with an alkaline stomachic powder of soda, potass, ginger, and calumba before meals: and, if the patient be weak, give iron and quinine to finish. Sometimes we have to put a very small blister over the pit of the stomach, but not often. This alkaline plan of treatment must be thoroughly carried out: and it is surprising what large quantities of alkali are often required. If you find the symptoms obstinate, you will be sure to find that the secretions are still too acid: they must be completely neutralised at the onset; and then you will quickly find the tongue lose its red tip and edges, and there will be no more pain after food. This feature in the treatment brings me to the point upon which all the interest turns—the link which connects these common cases of acid dyspepsia with the cases of disease of the heart.

It lies in the fact that this acid dyspepsia is a symptom of a rheumatic state of the system. If you inquire into these cases carefully, you will discover in the majority a rheumatic history. You will find that they belong to rheumatic families, or that they themselves have been the subjects of rheumatic affections. The fact is illustrated by many cases now under treatment.

There is every reason to believe that the acid, which is found in such quantities in the stomachs of these patients, is either identical with or closely allied to the *materies morbi* of rheumatism—the acid which must exist in excess in the organism before a person can have rheumatic fever.

Whether it is first generated in, or poured into, the digestive tract, and afterwards absorbed into the blood, may be open to question; but, whatever be the exact order of events, it is quite certain from clinical observation, that it appears in the stomach very early in its career of mischief, and there gives rise to the symptoms of acid dyspepsia, which may, therefore, be taken as an important signal for alarm, never to be disregarded. It shows that the enemy is collecting his forces for an attack.

For this reason, as you may have observed, I seldom discharge patients who have been under treatment for acid dyspepsia without giving them directions for their future management, and the prescriptions of their alkaline medicines to be resorted to whenever they find the symptoms of their complaint returning. By adopting this plan, I have known many persons keep free from rheumatic fever and rheumatism for years who previously had been subject to periodic attacks at short intervals.

If you inquire into the histories of persons who are subject to rheumatic fever, you will find, as a general rule, that they frequently suffer from acid dyspepsia; and, if you inquire into the histories of persons subject to acid dyspepsia, you will generally be able to make out either that they have suffered from some rheumatic affections, or that they belong to rheumatic families. In these two sets of cases, then, which are so numerous at this infirmary, we see the two ends of a long and sad story. So far as I am aware, the connexion between them has never before been specially pointed out; but I believe that I cannot too strongly impress upon you the importance of bearing in mind, when you get into practice, the fact that acid dyspepsia and valvular disease of the heart are to be regarded as the germs and vestiges of rheumatic fever; and that, if you keep in check the acid dyspepsia, you will not have the rheumatic fever; and that thus you may prevent the occurrence of the valvular disease.—*British Medical Journal, March* 11, 1865, *p.* 240.

9.—ON THE BLISTER TREATMENT OF ACUTE RHEUMATISM.

By Dr. HERBERT DAVIES, Finsbury Square.

The real questions appear to be simply these: Does the blister treatment remove the inflammation, pain, and swelling of the joints quickly and effectively?—Are relapses rare under the plan?—and further, Is the heart in the large majority of cases shielded from endocarditis and pericarditis by the method, fully and boldly carried out?

To all these points I can answer most distinctly in the affirmative, not only as the result of my own experience in hospital and private practice, but from the testimony of several who have adopted this plan of treatment. Thus Mr. Owen of Leatherhead says: "I have been so well pleased with the blister treatment of acute rheumatism that I have been induced to try it in subacute cases as well, and I may say with very decided benefit; but the relief experienced is not so striking. I have now treated about a dozen cases, and have found relief within twenty-four hours of the application; and, as a rule, have rarely had to use more than eight blisters." And he concludes by observing: "I do not think that I have had a single case of cardiac disease since using the treatment; and I may sum up by saying that if I had acute rheumatism to-morrow I would trust entirely to it."

Again, Dr. Duchesne, of Woodford, reports to me the case of a middle-aged man, a gardener, who suffered from agonizing pain in all his joints, acid urine, and all the usual symptoms of acute rheumatism. "I put on six blisters, and followed them up by linseed-meal poultices. No medicine. In ten days the man was out and at work. Much of the success in this case arose, I believe, from the fact that the patient was treated at an early stage of the disease."

The following case, under the care of Dr. Cory, of the Commercial-road, aptly illustrates the advantages of the treatment: "I have just had a case in which your blister treatment has had a most marked and beneficial effect in the person of a foreman to a builder, exposed to various vicissitudes of temperature. He was attacked with violent rheumatic inflammation in one hand and wrist, and the ankles also were so tender and swollen that he was unable to bear them even touched. Having passed two wretched nights, in spite of a grain of morphia at bedtime, I resolved to try your system of blistering, which was proposed to my patient, who gladly gave his consent to any plan which would promise relief. Blisters were applied around each ankle and wrist; relief was obtained in a few hours; and he slept soundly all night, awaking much refreshed. He had no more swelling or pain, no relapse or retrocession; and this morning he went to work, seven days after the application of the blisters."

Mr. Adams, of Lymington, Hants, has kindly sent me the following brief notes:

"W. V., aged sixteen, had severe acute rheumatism of both knees; excessive pain and immobility; not much general fever, and no heart symptoms. Circular blisters of about three inches in width were applied above and below the joints. Next day almost all the pain gone. The wrist and elbow were now attacked in a similar manner. Blisters were applied above the wrist and above and below the elbow. All pains had ceased on the following day. The knees were stiff and weak for a few days; but in less than a fortnight from the commencement of his illness he had returned to work. The only medicine taken was an aperient dose."

I might quote other accounts given me of the success of the plan in private practice; but a sufficient number of cases has been already laid before the profession to entitle the treatment, I believe, to its full consideration. In answer to the question as to the greatest number of blisters I have had to apply at one time in private practice, I cannot remember as many as ten being simultaneously required; but I have had several most successful cases where five, six, eight blisters have been at one time used, and by a little tact and explanation I have rarely had any difficulty in persuading my

patients to allow of their application. The slightest reference on my part to the great probability of the heart being saved by this plan has almost invariably induced an eager consent to the employment of the blisters. And I may further add that neither in hospital nor in private practice have I ever heard any serious complaint of the severity of the treatment; on the contrary, in the pamphlet which I have published on the subject will be found many instances where the patients expressed great satisfaction at the rapid and lasting relief which the blisters afforded. In some cases the "pains began to leave the inflamed joints as soon as the blisters began to draw," and in one case the patient said that "he would prefer to have forty blisters applied than undergo the agony of rheumatic fever." The same was the result in the majority of the cases treated on this plan by Dr. Jeaffreson in St. Bartholomew's Hospital. Using the words of the editor, "Reduced temperature, slower pulse, less acid urine, sleep and appetite, are the happy train of symptoms which follow hard on extensive vesication. In most of these cases the reports would lead us to think that the patients were quickly relieved by the blisters."

I can only add that the above are facts, the result of daily and most careful observation; and I would say further, that in no single instance have I seen any unusual "weakness of convalescence" or "slow healing from blistered surfaces." As to the time during which the cases reported were in the hospital, it will be observed from my pamphlet that many were discharged under three weeks, and that some remained in the wards for four, five, or six weeks. These latter cases were individuals of feeble constitution, who had been over-worked and under-fed, who would have recovered their general tone but slowly after any malady, and who required rest, food, and shelter from inclement weather for some time previous to their resuming their ordinary occupations.

I have already trespassed at some length upon your valuable columns, otherwise I should have been inclined to make some remarks upon the alkaline treatment of rheumatic fever. I will only quote from my paper the following statement: "That the blister and alkaline treatments are not incompatible with each other; and I believe a *mild* course of alkali to be a useful adjunct to the directly-eliminative blister plan, but not essential, except in those cases where the rheumatic virus attacks the heart in the first instance and appears to hesitate in fixing itself upon the joints." My object in publishing the results of the plan in fifty cases was to prove that the local treatment, fully and efficiently carried out, is all-sufficient in the very large majority of cases to remove the disease rapidly and safely, and without necessitating the internal employment of such large doses of alkali which, while undoubtedly tending to counteract the acid *materies morbi*, considerably impair for some time the general health and tone of the constitution of the patient.

The blister plan acts quickly, decisively, and to the point; at once throwing out the poison, diminishing the temperature of the body, restoring the urine to its normal reaction, and, without interfering in any way with the digestive powers of the alimentary canal, rapidly allows of the return of the appetite. Such a result is cheaply bought at the expense of a little pain.—*Lancet, August* 19, 1865, *p.* 213.

DISEASES OF THE NERVOUS SYSTEM.

10.—CASES OF REFLEX PARALYSIS, WITH OBSERVATIONS.

By Dr. Alexander Robertson, Physician to the Town's Hospital and City Parochial Lunatic Asylum, Glasgow.

[An important group of so-called functional derangements is believed to have its origin in some peripheral excitation. There are not wanting pathologists, however, who maintain that they should be classified with the organic forms.

A strong reason, however, for believing that the central lesion, if any, is slight, is the fact that recovery speedily follows the removal of the irritating cause.]

The first of the two cases that I have to submit illustrates one of the rarest forms of derangement of the central nervous system—reflex hemiplegia. I shall shortly narrate the history and leading symptoms.

On the 16th of last November, about 8 a.m., I was called hurriedly to see J. C., an hospital sick nurse, who was reported to have been seized with a shock of palsy. I was rather surprised at this message, as the woman was not old, forty-one, and, although delicate looking, had certainly not the aspect of one disposed to apoplexy. Besides she had been attending to her duty the day before, and made no complaint. I found her in bed, quite calm and intelligent as usual, ready to answer all my questions, which she did with promptitude, and manifested not the least mental confusion or excitement. She told me that when a child of about seven or eight years of age she took "nervous fits" three or four times a-day for three years: that afterwards she got quite well, but since then has been of a nervous disposition. About ten months since became affected with giddiness and ringing in the ears, and one morning was seized with numbness of the left side, which troubled her for about two months, when she recovered completely; but the latter symptom returned for a few days in the month of May, again leaving her in her ordinary health. For several years has occasionally suffered from severe pain in the left hip, extending down as far as the knee, which was aggravated on both of the occasions referred to. Believes that this pain is connected with constipation of the bowels, as she always experiences relief when they are free. About eight days ago, and she thinks owing to anxiety about her family, she had a return of the symptoms, especially the numbness of the side; but they were not so severe as to unfit her for her work. However, at 1 o'clock this morning the pain in her left haunch, which had been annoying her a good deal for some days, became excruciating, and as before, extending down the back of the thigh as far as the knee. At the same time she felt that she had partially lost the power of the left arm and leg. Suffered severely all night; but, about an hour since, at her own request, another nurse gave her a soap and water injection, clearing out a quantity of impacted fæcal matter; and in three minutes she was almost free from the pain in the thigh. Says that now the numbness continues, and affects also the same side of the face. Cannot hold small articles, such as a needle, in her hand; the sense of touch, however, when tested by compasses, is not much impaired as contrasted with the opposite side. Feels the left side colder than the right, though no difference is observable. Has no other abnormal sensation. Retains some power of grasping with the left hand, but it is decidedly weaker; can move the leg in bed in all directions, though not so freely as usual; and when she tries to walk she drags it after her. Has full power of bladder, and has been passing a moderate quantity of healthy urine. Sight of both eyes, particularly the left, is impaired; but this is not recent, as it has been ailing for some years. Considers it is due to constant sewing, by which she earned her livelihood before she became nurse; pupils are natural. The other special senses are in their normal condition, and so also is the power of swallowing. There is no distortion of the face nor impairment of speech; protrudes tongue correctly. Menstrual discharge is not now flowing, but has been seldom free from it during the last three months. Heart sounds natural; pulse 100, small and soft. Tongue is clean, but is troubled with flatulence, and has a poor appetite. Is of a nervous temperament, but possesses considerable vigour of mind, and is not easily excited. The bowels were acted on freely, and afterwards had a draught of tr. assafœtida and tr. valerian ammon., of each ʒss., in mist. camphor ʒj., every four hours.

On the 18th, the third day after the seizure, the report bears that she had greatly improved, and was attending to her duty, being able to walk nearly quite well, though not free from numbness of the left side. Prescribed tr. ferri. muriat., gtt. xv.; quinæ sulphat., gr. j., thrice daily, with full diet.

From this time her recovery continued, and she acted as nurse till she left the establishment about six weeks afterwards.

She called at the hospital in the beginning of April, when she told me that she had been working regularly at her employment as a seamstress since she left, and had continued free from paralytic symptoms, except on several occasions after an unusually hard day's work, when the numbness and loss of power returned in a slight degree, but passed away after a few hours' rest. She incidentally mentioned that when she had the serious attacks above described, an intensely itching eruption had broken out on her head, which lasted about a week, and produced an unusual quantity of scabs during its continuance; she also remarked that the left side of her face had often been swelled a little without any cause that she knew of, and was quite sure that the right had never been affected in the same way.

Remarks.—Here, then, we have severe sciatica, arising at intervals from a recognized cause,—impaction of the lower bowel; but on the last occasion the morbid impression produced on the lower part of the spinal cord had been very severe, and the woman's system, naturally a susceptible one, was more than ordinarily so, owing to her mental anxiety, and the drain on her vital powers from the prolonged menstrual flux; hence, probably the reason why the impression was transmitted upwards along the cord to the medulla oblongata, producing a partial hemiplegia, which, like the sciatica, was confined to the left side. The effects of the treatment pursued support this view of the case; for instant relief from the pain was experienced, and paralysis quickly passed away after the removal of the apparent exciting cause of both—the impaction of the bowel. It may be suggested that some of the symptoms indicated hysteria; and that the case might be classed under the hysterical hemiplegia described by the late Dr. Todd in his lectures on paralysis. But the leading features of that disease were wanting; and besides, some cases of palsy which have been held to be of this nature would probably have been more correctly viewed as reflex in their origin. The eruption on the scalp, attended by increased exudation, and the fulness of the left side of the face, are very interesting, as they indicate that the paralysing influence had produced a decided impression on the trunk of the cervical sympathetic. It is also worthy of notice that the anæsthesia was more pronounced than is usual in this kind of paralysis, as not frequently sensation is unimpaired.

I have now to record a case of paraplegia arising from an eccentric cause.

J. R., aged thirty, clerk, admitted May 22, 1864. Nearly two years ago was convicted of embezzlement, and was sentenced to eighteen months' imprisonment. When he had completed about ten months of his term, his health began to give way,—he got nervous, slept indifferently at night, his appetite became weak, and he began to pass small worms in his stools, which have troubled him ever since. Though he was ordered a generous diet, with metallic tonics, cod liver oil, &c., and had local treatment for the worms, he did not improve in any respect. About three or four months since felt his legs becoming very weak, much more so than his arms; and when his period of confinement had expired, a fortnight ago, this weakness had increased so much that he was unable to walk above a few steps at a time. About a month after he began to lose the power of his legs he noticed that he could not pass his urine with the same freedom as before. Since then this difficulty has varied much in degree, being sometimes much less than at others; but latterly it has increased so much that at present he has completely lost the power of his bladder, and has passed no water since yesterday. Is an intelligent, well-informed man; and gave a clear statement of his symptoms. Has a pale, nervous, depressed aspect; complains of dull headache, and says he has been subject to it during his confinement. Special senses are correct; and pupils, though rather open, contract readily under light. Motor and sensory power of the upper extremities and body generally are and have been unaffected. Has no pain in back. Sensation is perfect in lower extremities; and can also walk some steps, but very soon gait becomes tottering. Is not subject to starting of his legs, nor can reflex movements

be produced by tickling. Complains of great itching about anus and scrotum. Pulse only 52; skin rather cold. Urine had to be drawn off for several days by the catheter; but then he became able to pass it himself, though slowly and with difficulty. Was ordered full diet; to take twice daily a pill containing strychniæ, gr. $\frac{1}{10}$; ferri sulphat., quinæ sulphat., āā gr. j.; extr. aloes, aq., gr. 1½. To have an enema twice a-week of tr. ferri muriat, 3ij.; aq. calcis, ℥viii. Cold douche to spine, and surface afterwards to be thoroughly rubbed. The continuous galvanic current was also passed repeatedly between the lower part of spine and the hypogastrium. Under this treatment he improved; but there was great variation in his progress to recovery. Thus on June 8, I find recorded in my note-book, "That his urine had again to be drawn off by the catheter, though he had been passing it himself for some days previously. Itching at anus is very troublesome." Again on the 24th of the same month the entry is:—"Bladder and legs have recovered their power to a great extent; but the stools are not yet free from thread-worms, though they are fewer, and the itching is much less." From this date he progressed favourably; and on August 2 he was dismissed cured. His condition on dismissal is reported as follows:—"Worms are now quite away. Has been walking about for some weeks, and feels strong on his legs, Bladder has recovered its tone; but has a feeling of heat in urethra when he passes urine. Is returning to his occupation as a clerk."

Remarks.—This is a characteristic case of reflex paralysis. 1st. There is the obvious cause, the ascarides in the rectum, existing for some months before the paralytic symptoms set in. 2nd. The incomplete loss of power and the variation in its degree corresponding very much with the varying intensity of the local irritation. 3rd. There was no anæsthesia; and there was an absence of abnormal sensations in his legs. 4th. There was no pain in the course of the spine. 5th. It passed away entirely as soon as the disease upon which it depended was cured. 6th. One peculiarity is to be noticed:—The bladder was more than usually affected; but the paralysis differed materially in its character from that arising from ordinary organic causes; as, though complete on several occasions, it was not continuous, but recurrent; and generally it was only partial.—*Med. Times and Gazette, May* 27, 1865, *p.* 544.

11.—ON THE TREATMENT OF CERTAIN FORMS OF PARALYSIS BY GALVANIZATION AND FARADIZATION.

By Dr. Julius Althaus, Physician to the Royal Infirmary for Diseases of the Chest, London.

[There are only two forms of current required for therapeutic purposes, viz., the induced or interrupted electro-magnetic, and the continuous galvanic current, the methodical employment of which has been termed respectively "faradization and galvanization." Frictional electricity possesses no peculiar advantages, while the magneto-electric current is decidedly inferior to the electro-magnetic.]

The double-current induction machine, and the modification of Daniell's battery, both constructed according to my directions by Mr. Becker, of the firm of Messrs. Elliott Brothers, of St. Martin's-lane and the Strand, are sufficient for all practical purposes. The former of these apparatuses is similar to the induction machines which are now in general use on the continent; whilst the constant battery just mentioned is, for beauty and efficiency of arrangement, superior to any that have hitherto been contrived. In those constant batteries which have until now been used by medical men, the great drawback has been the presence either of nitric, sulphuric, or chromic acids, which not only after a time destroy the battery, but also entail considerable variations in the power of the current, and much loss of time on the part of the operator. It was therefore desirable, in the construction of a battery suited for medical practice, to dispense with acids altogether, in order to render the current as constant as possible, and to

avoid inconvenience and loss of time. For these reasons the machine mentioned is only charged with water and a solution of sulphate of copper, with the result that after having been once put into action a constant current is obtained, which continues reliable for about six months, even if the battery is daily used. No acid being present, the cast zinc cannot be destroyed, and remains quite unchanged. After the lapse of the period mentioned, the deposit of copper must be removed from the zinc plates, and a fresh solution of sulphate of copper be substituted. If this be occasionally repeated, the battery will last for any length of time; while most constant batteries, when daily used, are destroyed in about a twelvemonth, and some even in so short a space as six weeks. Another advantage of the absence of acid in this battery is, that the current gains in quantity while it loses in tension, and can on this account be safely used for acting on the centres of the nervous system.

The physical relations and the chemical and physiological effects of the continuous and the interrupted current are widely different from one another; and it may therefore be inferred that each one of them has also its own special sphere of action in therapeutics. The continuous current, which is produced by the chemical action of two heterogeneous conducting bodies, moves always in the same direction, and has considerable chemical effects; as it easily decomposes water and saline solutions, oxygen and acids being attracted to the positive pole, while hydrogen and alkalies accumulate at the negative pole. Induction currents, on the contrary, are of instantaneous duration, move alternately in contrary directions, and have therefore only a slight chemical action; for, as each wire serves alternately as positive and negative pole, their chemical effects are, in a great measure, neutralized as soon as produced. As regards the difference in the physiological action of the two currents, it may be laid down as a fundamental principle, that *the induced current only acts on the parts directly submitted to its influence, unless a very high power be used; while the continuous current, by reflex action, also affects distant parts, and more especially the centres of the nervous system.* As this is a new proposition, it will be necessary to adduce proofs in order to establish its correctness.

If the induced current is, by moistened conductors, applied to the face, it causes a peculiar sensation and contraction of the muscles; while the continuous current, if applied in the same manner, not only causes a peculiar sensation, and a contraction of the muscles both at its commencement and at its cessation, but also a vivid flash of light; and if the current be one of some force, even sickness, giddiness, and fainting may ensue. These latter phenomena, which are caused at whatever part of the face or nape of the neck the current may have been applied, can only be explained by assuming the physiological transmission of part of the current to the encephalon. There are also facts to prove that the continuous current has a physiological action on the spinal cord and the sympathetic nerve, if applied to the skin of the back by moistened conductors. Thus we may often cause the iris to contract by directing a current of large quantity to the lower cervical and upper dorsal vertebræ, showing that there is physiological transmission of part of the current to the cilio-spinal region of the cord and the corresponding ganglia of the sympathetic, which preside over the functions of the iris. Again, by applying a continuous current to the lumbar portion of the spine, we may cause a glow in the legs and feet, without any direct application to these latter, showing that the influence on animal temperature, which M. Claude Bernard and Dr. Brown Séquard have proved to belong to the sympathetic, is brought into play by the application of the continuous current. These facts would appear sufficient to establish the correctness of the proposition with which I started—viz., that the continuous current is capable of influencing, by reflex action, the centres of the nervous system, both cerebro-spinal and sympathetic; while the induced or interrupted current has no distant, but only local and immediate effects.

The therapeutical experience I have gained in various forms of paralysis with both kinds of current, entirely coincides with these physiological

premises. It is to the effect that the interrupted current proves useful in local paralysis, due to injury of the motor nerves and muscles, to pressure, rheumatic effusions, poisoning by lead, &c., but can have a beneficial influence in paralysis from diseases of the nervous centres only after the original lesion has subsided, and in reflex paralysis only after the irritation in the spinal cord has passed off. The continuous current, on the other hand, proves efficient in certain forms of paralysis due to affections of the nervous centres, more especially in those cases which are caused by effusion in the spinal canal and incipient softening of the cord, as well as in most instances of reflex paralysis where irritation of the cord is still present.

[The author illustrates the therapeutical use of Galvanization and Faradization in paralytic diseases, by a short series of cases which have been under his care in private and hospital practice.]

I. Faradization.

1. *Cerebral Paralysis.*—Faradization is useful in cases of hemiplegia after the complete formation of the apoplectic cyst, when there is no longer any irritation within the cranium; where the speech is little or not affected; where there is not much, or no muscular rigidity, but where the arm and leg remain nearly, or entirely, useless.

Case.—A gentleman, aged forty-five, had an attack of hemiplegia of the right side in June, 1863. I was consulted in April, 1864, when there was complete loss of power in the arm, and only slight recovery of power in the leg, the patient being just able to walk a short distance with the aid of another person's arm. Articulation satisfactory; no rigidity; nutrition in paralysed limbs much below par. Faradization for six weeks, after which the patient could write, dress, and feed himself, and walk a couple of miles, with the aid of a stick, without much inconvenience.

2. *Paralysis of the Portio Dura.*—Where this is due to rheumatic effusions in the sheath of the facial portion of the nerve, or in the muscles themselves, faradization is a very certain remedy, more especially where the contractility of the muscles is lost or diminished. On the contrary, where the intra-cranial portion of the nerve is suffering, a continuous current of low tension (from five to fifteen cells of the battery previously described) proves more useful. This latter often causes contractions of the muscles where a powerfully induced current fails to do so.

Case.—A lady, aged forty-two, was affected with left facial paralysis in January, 1862. She consulted me four months after the commencement of the affection, when the only muscles which had recovered their tone, to a certain extent, were the frontal and corrugator; but all others were still completely paralysed. Uvula straight; no deafness. Faradization for a fortnight. Improvement after the first operation. Cure at the end of the second week.

I have treated a number of cases of intra-cranial paralysis of the portio dura by the aid of the continuous current; but as in every one of them other active remedies were administered at the same time, I will not give any particulars, but merely say that there was no doubt about the beneficial effect of galvanization in these cases, not only with regard to the gradual recovery of power, but also to the relief of pain, which is scarcely ever absent.

3. *Rheumatic Paralysis.*—Cases of loss of power in the extremities, arising from exposure to damp and cold, even if of a severe kind and of long standing, are generally cured by faradization, which also relieves the muscular pains, which are sometimes very severe.

Case.—A gentleman, aged thirty-seven, having been much given to fishing and hunting, suffered for three years from great pain and loss of power in the upper as well as the lower extremities, which he himself attributes to continual exposure to the weather. I saw him in October, 1858, when there was much wasting of muscular tissue, especially in the right deltoid and the extensors on the back of the forearm. No signs of central disease. The

urine has been very thick, but is now normal. After five weeks' faradization the patient recovered.

4. *Paralysis from Pressure on Nervous Plexuses.*—Cases of loss of power from continued pressure, especially of the head on the arm during intoxication either by chloroform or alcohol, are generally curable by faradization.

Case.—A lady, aged twenty-three, had her first confinement in November, 1864, during which she was for some time under the influence of chloroform. While in this condition her head rested heavily on the left arm, and pressed so much on the brachial plexus that a number of muscles animated by the latter became entirely paralysed, there being also complete anæsthesia of the left arm. I saw her in January, 1865. The affection was most severe in the muscles of the forearm, the patient being quite unable to lift the wrist, which was much swollen, and had to be bound up with a splint. As she also complained of great weakness in the other limbs and the back, I combined the use of a continuous current of fifty cells to the spine, with faradization of the left shoulder and arm. After six weeks of this treatment, the patient felt very much stronger generally, and had entirely recovered the use of the left arm.

5. *Paralysis from interrupted Continuity between the Spinal Cord and Motor Nerves (Dr. Marshall Hall's Spinal Paralysis).*—Where the connexion of motor nerves with the spinal cord has, by external violence, been interrupted, a period varying from six to twelve months is required for re-establishing nervous conduction. The two cases recently published by MM. Nélaton and Laugier, which would seem to throw doubts upon this point, are, in my opinion, not sufficient to make us give up the above proposition, which has been deduced from most careful clinical observation of a very large number of cases, as well as from physiological experiments on animals. Experience shows that faradization soon or immediately after such accidents is useless, and does not even prevent atrophy of the muscles deprived of their connexion with the cord. On the contrary, if resorted to from six to twelve months after the accident, faradization frequently restores the power to the paralysed muscles.

Case.—A porter, aged thirty-two, suffered compound fracture of the right arm and other injuries from being run over by a van in December, 1860. After three months the fracture was healed but the arm remained useless. He was sent to me in May, 1861, when I found complete anæsthesia, paralysis, atrophy, and loss of electric contractility of the muscles from the acromion downwards. I recommended him to do nothing for three months, and then to present himself again. This he did in the October following, when he appeared to be much in the same condition. I then commenced the faradic treatment, and after about four months, the patient attending every other day, he had to a great extent recovered the use of the arm, although it was still much weaker than previous to the accident.

6. *Reflex of Paralysis.*—In cases of this kind faradization proves beneficial after the irritation which, by its transmission to the spinal cord, caused the paralysis has subsided, but where paralysis nevertheless continues. In other cases faradization or galvanization may remove the irritation, and thus prove curative. For cases see my work on Paralysis, Neuralgia, &c., pp. 155–158.

7. *Hysterical Paralysis.*—Where this affection occurs in only one limb, or a particular set of muscles, faradization of the suffering parts is of use; but where it appears in the form of hemiplegia, paraplegia, or general paralysis, galvanization of the nervous centres is more effectual. (See a case of this description in the Lancet for Feb. 18th, 1865, p. 175.)

8. *Lead-Palsy.*—In paralysis from poisoning by lead, good results are generally obtained by faradization, even in severe cases, and after muscular atrophy has set in.

Case.—A painter, aged thirty-three, had for more than a twelvemonth suffered from weakness of sight, colics, and dropping of both wrists. He had had a long course of iodide of potassium, under which he somewhat improved, but not one of the symptoms was entirely removed. I saw him in Novem-

ber, 1864, and treated him by galvanization of the optic nerve and abdomen, and faradization of the suffering muscles. Within a month he was so much improved that he could be discharged.

9. *Progressive Muscular Atrophy.*—There are two forms of this disease—viz., the partial, which begins in the hand or the shoulder, and does not affect any muscles but those of the upper extremities; and the general form, which may destroy nearly all the voluntary muscles throughout the body, and is mostly fatal. Cases of the former affection are curable by faradization; and such of the latter kind may be considerably improved, and their course be arrested, by a combined use of faradization of the muscles and galvanization of the nervous centres, if resorted to at an early stage of the disorder.

Case.—A gardener, aged twenty-five, became, in consequence of overwork, very weak and lost flesh in the right arm and shoulder, in August, 1863. During the following two months the affection spread to the forearm and hand, and he was then quite unable to follow his employment. He was sent to me in December, 1863, when I found atrophy of most of the muscles of the right upper extremity. Faradization for nearly five months, after which the power of the muscles had so much increased that the patient could return to his occupation.

10. *Paralysis of the Vocal Cords.*—As Dr. Morell Mackenzie has lately published a number of cases of aphonia due to a paralytic condition of the vocal cords, and successfully treated by faradization, I will only say that my experience in this particular quite agrees with that of Dr. Mackenzie.

II. Galvanization.

1. *Cerebral Paralysis.*—In certain cases of cerebral paralysis with intracranial irritation, a cautious use of the continuous galvanic current proves of decided benefit, in allaying pain, spasms, and muscular rigidity. I abstain from giving cases of this description, as in every one of them which have fallen under my notice other remedies were adminstered at the same time. The continuous current is likewise useful in cases without pain and spasm, but when the speech is much affected and the paralysis of arm and leg continues; and it can be safely employed about six months after a seizure.

2. *Ptosis, Double Vision, and other Paralytic Affections of the Third, Fourth, and Sixth Nerve, and of the Iris.*—In these conditions, even if they are caused by encephalic disease, the continuous current, cautiously and gently applied, may produce very striking results. For the majority of such cases, five or ten cells of the battery are quite sufficient, and the operations must be very short; for long and strong applications only do harm. An able and elaborate paper on this subject, by Dr. Benedikt of Vienna, may be found in Von Graefe's "Archiv für Ophthalmologie," 1864, vol. x., part i., p. 97

Cases.—A gentleman, aged forty-nine, had for ten months suffered from ptosis of the left eyelid, for which he had undergone a variety of treatment without benefit when he consulted me in Dec., 1864. There were many other symptoms exciting the suspicion of encephalic disease. Galvanization by five cells, for not more than half a minute each time. Within a fortnight he had recovered the power over the eyelid, although there was no improvement in the other symptoms.

A lady, aged forty-five, suffered from mydriasis of the right eye. She was otherwise in good health, and unable to account for this affection. She was sent to me in July, 1862. Galvanization by three cells of Bunsen's battery, with immediate improvement during the first operation. She left town after four operations, much improved.

3. *Incipient and Progressive Softening of the Spinal Cord.*—In this affection good results may be obtained by the application of a powerful continuous current, of from fifty to a hundred cells of Daniell's battery, to the spine. Such operations, when judiciously performed, evidently exercise a direct influence on the nutrition of nervous matter. In the late stages of the disorder—that is where actual destruction of nervous matter has taken place—of course no benefit can be obtained.

Case.—A commercial traveller, aged thirty-eight, felt pins and needles in his feet, and loss of power in the back and lower extremities, in March, 1864. I was consulted in October last, when there was tottering gait, loss of sensation in back and legs, difficulty in passing urine, costiveness, and other symptoms of softening. Galvanization by from eighty to a hundred cells, ten minutes each time, for two months. Remarkable improvement in every respect, so that the patient was able to return to his avocation.—*Lancet, Aug.* 12, *and Sept.* 2, 1865, *pp.* 178, 259.

12.—ON PROGRESSIVE LOCOMOTOR ATAXY.

By Dr. C. B. Radcliffe, Physician to the Westminster Hospital, and to the National Hospital for Paralysis and Epilepsy, &c.

This malady was discovered seven or eight years ago by M. Duchenne (de Boulogne), and described under the name of *ataxie locomotrice progressive.* In it there is no deficient action of the will upon the muscles individually, and no loss of muscular force or change of muscular structure; but there is deficient action in that involuntary instinctive co-ordinating power by which various groups of muscles are made to work harmoniously together in such acts as standing, walking, or handling. The term ataxy (*a*, privative, and *ταξις*, order), which has reference to the deficiency in proper co-ordinating power, is not very fortunate, for the adjective *ataxic* is very commonly used in a different sense, as in ataxic fever, where what is meant is low fever in which nervous exhaustion is the predominant condition. To meet this difficulty other names have been suggested; but *the* satisfactory name has yet to be found, and for the present, therefore, we must be content to use the one employed originally, and call the disease in question ataxie locomotrice progressive, or progressive locomotor ataxy, or ataxy simply.

Until M. Duchenne (de Boulogne) drew attention to it, ataxy was confounded with spinal paralysis in general, and especially with that vague form of this paralysis which is known under the name of *tabes dorsalis.* The disorderly movements of the muscles were regarded as symptoms of paralysis. It was not perceived that the muscles in which these disorderly movements were manifested were healthy, and behaved well in all respects except when they were called upon to act in concert. Moreover, it was not known that this loss of co-ordinating muscular power simply was associated with other symptoms in a definite category. In a word, to M. Duchenne (de Boulogne) must undoubtedly be ascribed the honour which belongs to him who discovers a disease, sufficiently definite in it characters, which had been confounded previously with other and very dissimilar diseases.

The case which serves me for a text is now in the wards. It is that of J. C., a sailor, thirty-four years of age, living at Islington, and admitted into the National Hospital for Paralysis and Epilepsy on the 3rd of April, 1865. The notes I have run thus:—

Present Condition.—J. C. is a man somewhat under the average height and weight, well-proportioned, well-developed as to muscle, especially in the upper parts of the body, with scarcely any subcutaneous fat, with abundance of brown hair on the head and face, and with a complexion much dried and browned by long exposure to sun and sea.

He is capable of walking without a stick, but his gait is peculiar—staggering, precipitate, the legs thrown about vaguely and spasmodically, and the heel brought down with force at each step. With his eyes shut, or in the dark, he reels over at once, and would fall helplessly unless prevented from doing so. In the sitting posture he can lift either leg steadily into any position, and keep it there, and when the leg is put out in this manner, he can keep it extended in spite of a very strong effort on my part to flex it. In order to this, however, he must see what he is doing, for if his eyes are shut the leg at once becomes unsteady, and little force comparatively is required to bend it. The right leg is a little weaker than the left, but not to any very

considerable degree. He finds it very difficult to come down-stairs, or to quicken his pace much, and he is speedily fatigued by the acts of standing or walking. On being told to shut his eyes, and touch his nose with his forefinger of each hand in turn, he did so with tolerable accuracy, especially with the forefinger of the left hand. On being told to stretch out his arms, and keep them out, he did so quite well, but only so long as he was allowed to see what he was doing, for on holding a book before his eyes, his arms, shoulders, and neck and head—the upper part of his body generally—at once became affected with convulsive titubation. When the book was taken away these movements speedily came to an end, but not before they had issued in a fit of crying and sobbing, which was not a little distressing to witness. This fit took the patient quite by surprise, and it could not be accounted for by the examination having been conducted roughly, or carried on for an undue length of time; indeed, the holding of the book before the eyes, which was its immediate cause, did not occupy more than a minute at the most.

The muscles of the lower limbs generally are less developed than those of the upper limbs and trunk, but there is no appearance of wasting in them. Their electro-contractility is perfect; they stand out firm and hard when made to contract by the will, and the contraction seems every whit as strong as it ought to be. Indeed, as has been stated already, when the leg is put out and stiffened, it is not in my power to bend it against the will of the patient, except by the exercise of great force. There is no tremulousness anywhere; and no marked reflex movements are produced by tickling the soles of the feet.

What the patient complains of chiefly are excruciating, stabbing, boring, nipping pangs of pain, flitting from one spot to another in a very erratic manner, in paroxysms lasting from a few minutes to twelve, twenty-four, or forty-eight hours; and generally fixing upon one spot only in any single paroxysm. These pangs are most frequently felt in the two feet, especially about the outer side of the metatarsal bone of the little toe; they are also felt not unfrequently at the back of the thighs, in the nates, and in the upper arm about the lower part of the belly of the biceps. They are scarcely ever absent, especially at night; at night, too, there is often a sensation of great coldness, with some degree of constriction, in the painful parts.

Measured by the compasses, tactile sensibility is found to be much impaired in the feet (especially in the soles), in the lower halves of both legs, and to some degree also in the back of the thighs, in the nates, and in the palms of the hands. The ground is felt very obscurely, but so far as it is felt the sensations are accurate—that is to say, it does not seem to the patient as if there were elastic cushions, pebbles, or other imaginary bodies, or as if there was nothing at all, under the feet.

Very rough pinching is scarcely at all felt in the benumbed parts, but elsewhere the sensibility to painful impressions is keen enough.

In the benumbed parts also, and in a lesser degree in some other parts, muscular sensibility (and with it probably the proper sensibility of the joints and bones) is manifestly diminished. In bed, for example, when there is no light, J. C. says that he cannot clearly tell where his feet are, and that not unfrequently his legs get quite out of bed without his knowledge. There is also very little feeling of pressure when the muscles of the feet and legs are squeezed with considerable force; and in the arms, as well as in the legs, the faculty of measuring and adjusting the amount of muscular action necessary for any given act is evidently not as much alive as it should be. For instance, when two weights are placed in his hand one after the other, one of them being at least a pound heavier than the other, he finds it difficult to say with certainty which is the heaviest of the two.

The sight of each eye is defective, and glasses afford no relief. The pupils are equal in size, and respond fairly to the light. The conjunctivæ are injected. There is no arcus senilis. There is no squinting or ptosis.

The hearing of each ear is so dull as to make it necessary to speak in a very loud tone in order to be heard. There are also constant singing and booming noises in the ears—"almost like the wind in the shrouds," he says.

The memory is bad, the spirits are very despondent, the head generally "feels very heavy," and of late (this statement is volunteered by the patient) there has been a frequent temptation to commit suicide.

The pulse is feeble, and about 70 in the minute. The appetite is good. The bowels are somewhat constipated. In any case a long time is spent over a stool: "it wont come out," is the patient's own description of the trouble in this quarter. The urine is also voided slowly and with difficulty although the urethra is free from stricture; and now and then it escapes in bed at night. Sexually, the state may be spoken of as approaching to, if not amounting to, spermatorrhœa.

Previous History.—Five years ago J. C., it appears, began to suffer from pains in the legs and back, and to be unsteady in his gait; about the same time, also, his sight and hearing began to fail; and from that time to this he has continued to get gradually worse and worse. Four years ago he had a sun-stroke in the West Indies, of which the immediate symptoms were violent agitation and shaking, without loss of consciousness, and for which he was taken into an hospital and bled; but this accident was twelve months *after* his present malady had commenced, and therefore, it is not possible to look upon it as a cause of this malady.

C. was at sea seventeen years in all, chiefly in hot climates, as the West Indies and West Coast of Africa, and he continued at sea three years after he had begun to suffer from unsteadiness of gait and from the other symptoms which have just been mentioned. Once during the time he was at sea he had chancres, without secondary symptoms; and repeatedly he had the diarrhœa; but, with these exceptions, his health on all occasions appears to have been very good. He says that he was always very careless, often sleeping almost without clothes upon the bare deck or upon the ground; and that he was always "too much given to drink and women." For the last two years the sexual power has been much damped; but before this time, according to his own showing he appears to have been little better than a very satyr. Two years ago, when obliged to abandon his calling as a sailor, he was for a while treated in the hospital at Quebec for rheumatism. Afterwards he found his way to this country, and became an out-patient first at one hospital then another. During this time he appears to have been frequently blistered along the spine, and on one occasion to have been salivated. For the rest I have only to add that his father died early in life of consumption; that his mother died young from some unknown chronic disease; and that a brother, the only member of the family besides himself, is now dying of the disease which proved fatal to his father.

M. Duchenne, whose description is the best as well as the first, marks out three stages in the course of the malady of which this case is an example. In the first stage, the patient suffers from paralysis, often temporary only, of one or other of the motor nerves of the eye, from some degree of amaurosis, usually accompanied by unequal pupils, and from the peculiar boring erratic pangs of pains from which C. suffered. In the second stage, in addition to the symptoms of the first stage, the characteristic unsteadiness of gait begins to show itself, together with diminished sensibility to touch and pain in the skin of the lower parts of the legs, in the skin of the soles of the feet especially, the interval between the first stage and the second varying in length from a few months to several years. In the third stage the malady becomes more profound and general, the disorder in involuntary co-ordinate movement increasing in the legs, and extending to the upper parts of the body, the anæsthesia increasing in the parts first affected, and extending to other parts, not of the skin only, but even to the muscles, joints, and bones. M. Duchenne does not consider the affections of the bladder, the rectum, and the genital apparatus as essential symptoms of the disease in any one of these three stages; he speaks of them as "epiphénomènes" only.

So far as it goes, the history of C. does not tally with this division of the disease into stages; for in it the failure of sight, the pains and the irregularity of instinctive co-ordinate movement are seen to have made their

appearance together. Nor is this history altogether exceptional in this respect; and therefore the only conclusion to be drawn is that there are some cases, perhaps the majority, in which the symptoms admit of being grouped in the three stages indicated by M. Duchenne.

In the majority of cases, undoubtedly, the principal symptoms of ataxy agree with those which are met with in C.'s case. There is the same irregularity of involuntary co-ordinate movement in muscles perfectly healthy in other respects, the same peculiar pains, and the same anæsthesia. Still there are other cases in which the pains or the anæsthesia are either not very conspicuous phenomena or else absent altogether. Thus, I have met with four cases out of seven, not very advanced in degree it is true, in which there were no pains to speak of, and with two cases (out of the same seven) in which there was no anæsthesia. In the majority of cases, perhaps, the eyesight fails at an early period, and in many cases this failure is associated with some paralysis, often transitory, of the motor nerves, one or other, of the eye, or of some branches of the motor nerves of the face; but there are other cases in which none of these phenomena are present. In a word, the affections of the bladder, rectum, and genital apparatus are not the only features which must be looked upon as accidental—as "epiphénomènes"; and the simple truth is that the idea of the disease becomes clearer just in proportion as this fact is clearly realised.

Ataxy, it is said, may be confounded with several diseases, especially with simple loss of muscular sensibility, with disease of the cerebellum, with general paralysis of the insane, with general spinal paralysis and common paraplegia, with saturnine paralysis, with Cruveilhier's disease, with paralysis agitans, and with chorea and some other affections of the kind; but fortunately, as a rule, very little attention will serve to prevent such confusion.

Simple loss of "muscular sense" has been supposed to be the cause of ataxy, and undoubtedly this is a malady frequently associated with ataxy, and most easily confounded with it. In *simple* loss of muscular sense, however, the sight can supply what is lost; and thus when the eye is open and the attention alive, the involuntary co-ordinate movements as well as the voluntary movements of the affected muscles are all executed regularly.

In disease of the cerebellum there appears, at first sight, to be the same disorder in muscular movements as in ataxy, but this similarity is deceptive. There is now, curiously, in the same ward, and also under my care, a boy in whom there appears to be some congenital defect in the cerebellum—want of due development probably. This boy has had a very peculiar gait; he reels and rolls about in walking, as if he were giddy or drunk: there is nothing peculiar in the way in which he plants his feet: and this way of walking is precisely that which appears to me, more or less, characteristic of disease in the cerebellum. In ataxy, on the other hand, the gait, as has been said, is staggering, precipitate, the legs thrown about vaguely and spasmodically, and the heels brought down with force at each step. There is something very peculiar in the way in which the feet are planted; the gait does not give the idea of giddiness; there appears to be some want of balance between the flexors and extensors in each leg, the flexors having the advantage; and, in addition, the muscles, when they act, seem to contract with a sort of jerk—spasmodically. In *certain* diseases of the cerebellum, also, other symptoms are likely to be present which will assist in the formation of a correct diagnosis, especially violent pains, augmented by movement, in one or other part of the head, and frequent and obstinate vomiting. Moreover, in these diseases the onset is generally sudden, and the progress uncomplicated with the peculiar pains and anæsthesia of ataxy proper.

In general paralysis of the insane, the hesitation in speech, the tremulousness of the lips and tongue, the general tremulousness, the true paralytic weakness of the muscles as to voluntary movement, and the mental condition of the patient, must readily serve to prevent the unsteadiness of gait and other evidences of disordered co-ordinate movement from being con-

founded with those which occur in ataxy; and in other respects also the features of the two diseases are sufficiently distinct.

In general spinal paralysis and in common paraplegia there is true paralysis, more or less complete, of the muscles as to voluntary power, and the muscles moreover are much damaged as to their nutrition and contractility, and generally as to their sensibility too. In the majority of cases, also formication is associated with numbness, and, what has never yet been noticed in ataxy, the sensibility to difference of temperature is much impaired or altogether lost in the paralysed parts. Very generally there is tenderness in some part of the spine, and not unfrequently pain in the same region or in the paralysed parts; and if there be pain, it is, as a rule, more like dull rheumatic aching than like the peculiar excruciating, stabbing, boring, nipping pangs of pain, flitting from one spot to another in a very erratic manner, and occurring in paroxysms of varying duration, which are present in C.'s case, and which are rarely absent in cases of the kind. And in those cases where progression is possible the gait is almost always sufficiently characteristic—*not* staggering, precipitate, the legs thrown about vaguely and spasmodically, and the heel brought down with force at each step, as in C.'s case and in other cases of the kind, but hampered and s w, partly from want of voluntary power over the legs, and partly from he muscles becoming stiff in moving, each leg being brought forward with evident difficulty, even with the help of an upward hitch of the whole side of the body belonging to it, and the part of the foot first brought in contact with the ground being as a rule, not the heel, but the toes.

In saturnine paralysis it is the voluntary power over certain muscles which is impaired and gone, and the muscles are atrophied and deprived of electric contractility when the malady has reached its height. Indeed, it is difficult to understand how this affection can be confounded with ataxy.

In Cruveilhier's disease, as I showed in a former lecture (Lancet, Jan. 16th, 1865), the wasted muscles are changed in great measure into fat, and, as it were, dissected away, and any errors in movement are such as may be accounted for by this atrophy and absence; whereas in ataxy the muscles are plump and to all appearance perfectly healthy, and the errors of movement are those which refer to want of proper involuntary co-ordinating power. Moreover, in Cruveilhier's disease the pains and anæsthesia of ataxy are not met with.

In chorea there is great want of co-ordinating power in muscular movement, but the rest of the history is quite different from that of ataxy—so different and so well known as to make it altogether unnecessary to recall it to your memory. And so likewise in regard to those other affections which come into the same category as chorea.

And lastly, in paralysis agitans a mistake cannot well be made, for the general features of the disease are more akin to those which are present in general paralysis than to those which are characteristic of ataxy.

In some cases Duchenne's disease has been associated with other diseases of the nervous system, as with Cruveilhier's disease, or with general spinal paralysis or common paraplegia, and in these cases the distinctive characters of the disorder may be somewhat masked; but in ordinary cases there can be but little difficulty in arriving at a correct diagnosis.

The pathology of ataxy is yet in its infancy. The most marked change detected after death as yet is degeneration and disappearance, more or less complete, of the posterior columns and of the posterior roots of the nerves of the spinal cord. Mr. Lockhart Clarke has shown me some beautiful microscopic slides, which furnish most conclusive proof of this fact, But I am not prepared to think that these are the only changes to be met with. On the contrary, I fully believe that corresponding changes will eventually be found in those parts of the cerebrum wherever they are, which have specially to do with the co-ordination of muscular movements

The prognosis is full of gloom. The progress of bad to worse may be slow, very slow, but as yet there is too much reason to believe that the term

"progressive" in the sense of going on from bad to worse is not misapplied. Still I know of two cases which justify a much more hopeful opinion.

The treatment in C.'s case consists in good food and rest, in hypophosphite of soda and cod-liver oil, and in the electrifying of the region of the cord with free positive electricity, and we shall see in time whether any good will come out of it. I am not very sanguine; at the same time I have this to encourage me—that this is the kind of treatment which has done and is doing good in the two cases to which I have just referred.—*Lancet August* 19, 1865, *p.* 193.

13.—CASES OF PAIN OF THE SPINE.

Under the care of Dr. HENRY DICK.

[Patients who suffer from the kind of pain referred to in this paper are generally females of a weakly constitution, suffering from some derangement of the uterine system.]

Six cases of this kind came under observation at the hospital during the year 1864-65. The patients were of ages varying from twenty-four to fifty. The first was a young woman, aged twenty-four; the second a Frenchwoman, aged forty-five; the third a Scotch schoolmistress, aged forty-six; the fourth a strong, healthy woman from Bristol, aged fifty; the fifth a London needlewoman, aged twenty-six; the sixth was a man, sickly-looking and suffering from spermatorrhœa, aged twenty-six.

We now proceed to describe the first case, treated at the hospital as an inpatient, the opportunity being afforded of watching it closely.

M. L. G., spinster, aged twenty-four, from Boston, Lincolnshire. She menstruated at fifteen, but irregularly during the first year. In that year she was under medical treatment for fluor albus, Soon afterwards she went to Germany and got better, and remained well until she attained the age of twenty-one years. For the last four years she held a situation as cook, and during the last three she had been suffering with fluor albus, accompanied with great pain along the spine. For this she again sought medical advice, and was bled; she felt worse, and lost both appetite and strength. For eight months before her admission into the hospital (February, 1865) she could neither walk nor stand, and had such acute pain in the spine that she could not bear it to be touched; and so constant had been the pain for the last seven months that she could not obtain any sleep, and had no appetite.

Such were the symptoms which she presented when she entered the hospital, and which to some extent she had suffered from for three years, sometimes the pain being worse in the morning, but latterly more so in the evening. On a superficial examination of the back, no deformity could be distinguished; but as Dr. Dick had been very sceptical for a long period on the subject of "hysterical pain of the spine," he had been for many years in the habit of examining with great care every case of the kind which came under his notice.

In the present instance Dr. Dick found the spinal processes perfectly normal, but the muscles of the left side of the inferior part of the dorsal portion of the spine, where they pass over the transverse processes, were very slightly raised; there was also a similar very slight prominence of the muscles on the right side of the lumbar portion. So extremely slight were these prominences that they might easily have been overlooked, had not three post-mortem examinations presenting similar appearances attracted Dr. Dick's close attention to the subject.

As the patient could not walk or stand, it was necessary to keep her in bed; as her digestion was also very bad, and she seemed anæmic, the tincture of sesquichloride of iron was ordered, and with her food a few drops of lemon-juice; and the nurse was directed to rub her back with cold water and volatile liniment. She improved a little under this treatment; but in addition a spinal instrument was ordered to be made for her, which was con-

sidered the only real means of cure. The instrument is constructed as follows: A clavicular and a pelvic belt are connected behind by two strong narrow steel bars extending down each side of the patient's back. To the upper part of the steel are fixed two bullock-horn-shaped crutches (for the arms), which in front are connected with the clavicular belt. At those parts of the steel bars or uprights which lie adjacent to the slight prominences of the dorsal and lumbar muscles, as mentioned above, are fixed two oblong narrow steel lever plates, the direct and forcible pressure of which can be increased or diminished at pleasure, by means of a peculiar mechanism already described by Dr. Dick elsewhere.

From the time this instrument was applied the patient was able to leave her bed, her appetite began to improve, sleep was restored and her general health became better. After she had worn the instrument for three weeks, it was taken off in order to exemplify the benefit of wearing it; but the patient was, in consequence, unable to leave her bed, or to remain in the standing position when so placed. On reapplication of the instrument she became immediately at her ease. In the beginning of May she left the hospital to return to Lincolnshire, and has since written to state that she is quite restored to health and continues to wear the instrument.

Similar results to the foregoing attended this plan of treatment on the five out-patients, who from the commencement of wearing the instrument felt relieved, and soon became capable of resuming their usual occupations.

The following clinical remarks were made by Dr. Dick: I ascribe those acute pains in such cases as the foregoing to rotation of the spine, the cause of which I demonstrated before the British Medical Association at Cambridge in 1864. I have shown, that as soon as lateral deviation occurs, rotation necessarily ensues, and therefore it must be obvious that those pains are caused by the rotation. But what is of far greater importance, the treatment based on this conclusion is always successful. Rotation of the spine may be much advanced in degree, and not recognised in the living subject. I will advert to the details of Dr. Gideon Mantell's case in illustration, as related by Dr. Hodgkin and Mr. William Adams, and the correctness of my assertion will be at once apparent. The most important symptom in Dr. Mantell's case was the agonizing pain at the commencement of his complaint. At the post-mortem examination much astonishment was felt at finding such extensive rotation of the lumber vertebræ, attended with comparatively so little mischief to the spinal processes. In 1851 I had occasion to make a post-mortem examination on a young woman, aged twenty-eight, in whom, as in Mantell's case (except that the affection was a little higher up), I found deviation and rotation of the bodies of the vertebræ, with but little mischief in the spinal processes. This post-mortem inspection was the means of first drawing my attention to the causes and peculiarities of spinal rotation. The following conclusions may now be drawn:

1stly. In all cases of chronic pain on either side of the spine; attended with even the *slightest* undue prominence of the muscles over the transverse processes, an anatomical lesion of the ligamentous and bony part of the spine exists, combined with deviation of the bodies of the vertebræ and rotation.

2ndly. That the term "hysterical" pain in the spine should have no place in our nosology.

3rdly. For the relief and cure of those spinal pains, mechanical means are requisite, and which can be accomplished by the action of the instruments above described.

4thly. In cases where the general health is disturbed, appropriate means must be adopted.—*Lancet, July* 22, 1865, *p.* 91.

14.—CASE OF EPILEPSY WITH INSANITY, CURED CHIEFLY BY MEANS OF ICE TO THE SPINE.

By Dr. John Chapman, Somerset Street, Portman Square, London.

[The patient was forty-two years of age. He had fits in childhood, but they

left him. In 1854, however, when thirty-three years of age he had a severe epileptic convulsion. Two years subsequently he had another fit, which was followed by insanity of three weeks' duration. After this he had constant fits up to the present time. His attacks have gradually increased in frequency: during the last twelve months he has had on an average three fits daily.]

Besides his fits, he has curious visions of animals of abnormal forms, of boys destroyed by machinery, &c. Has dreams of the same kind. He often has a sensation in the eyes as of a glowing electric light, which, changing, assumes various colours; often, too, he sees men working in the flames. Has much headache, the forehead being "blazing hot" each morning; generally when he does sleep, sleeps very heavily, and frequently lies awake. His dreams are so horrible and distressing that he is afraid to go to sleep. His memory is very greatly impaired; when going from one room to another to fetch anything, often forgets what he intended to fetch; and any instruction given him by his wife or others when he goes of an errand he is always obliged to write down. Not infrequently, and without first having a fit, he loses his consciousness for short periods. During such times, he is said to talk more or less incoherently, and often to appropriate small objects within his reach. His eyes are bloodshot every morning, and have a mist before them; they are also habitually lachrymose. Has suffered from piles; ten days ago passed blood from the bowels, which are often constipated. Passes urine with difficulty; often it merely dribbles away; he has insufficient expulsive force. His hands and feet are always cold and clammy: gloves and socks, however thick, will not enable him to keep them warm.

I ordered ice to be applied along the whole spine during two hours four times in each twenty-four hours, one bag being applied at bedtime, one being used during the night, and the other two during the day.

16th. Has had no fit since the ice was first applied. The headache has ceased; the sleep is greatly improved, and is much more refreshing; the appetite is also much improved, and the patient looks much better. His hands and feet are becoming warm. Pulse 84; tongue clean; bowels "nicely open;' urine still expelled with difficulty. To continue the ice as before, and to take acid. sulph. dilut., ℥ xii.; quinine disulph., gr. ss., b. d.

29th. Has not used ice either yesterday or to-day, being unable to buy any, and having already pawned his coat. Had a "bad night" last night, and this morning while walking he had a slight wave of vertigo, but has had no fit since the 16th inst. I saw when he entered my room he did not look so well as he did on the 26th. He looked just as if he had had no sleep last night. I applied ice along the back of his neck and between the shoulders. Immediately he began, he said, to see more clearly, and shortly afterwards asked me if I did not see a change for the better in his face, which, indeed, became plainly visible while I was writing this note. He says his memory is wonderfully improved, and that, excepting last night, he has slept "splendidly." Pulse 80. The discharge of blood from the bowels has not recurred. They are now slightly constipated. To continue the ice and medicine as previously prescribed, and to keep the bowels open by castor oil.

June 2. Since 29th ult. has used ice, and taken the medicine as ordered. Has had no fit. Sleep and appetite continue "first-rate." Does not think he has had any dreams during the last three nights, and has had no hallucinations in the day time. His forehead is now cool, and his eyes are no longer either bloodshot or obscured by "the mist before them" when he rises in the morning. They have not even been watery during the last two days. "My feet," he says, "are very warm now." Pulse steady, 84. Continue the ice as before, and take quinæ disulph. gr. jss., acid. sulph. dilut., ℥ vii. before dinner daily.

10th. Still no fit; feels quite well; is delighted with the improvement in his memory; can now expel his urine with natural force, and in full continuous stream; bowels constipated three days; took castor oil on the 8th inst., when they were freely opened. To use two bags of ice during each night as before, but only one in the day time, and to let that one be wrapped flannel.

12th. On the night of the 10th he tried the experiment of going to bed without the ice-bag, but could not go to sleep, and was very restless, although very weary. He went to bed at 9.30 p. m., and continued awake until after 12, when he resumed the ice-bag, and went to sleep comfortably forthwith. He says he can tell the state his brain is in by the apperance of the candle-flame, which, before using the ice, always seemed to him, as it does now at times, as if surrounded by a large, luminous, coloured haze, the flame itself being like a red mist. If his brain is in an excited state this phenomenon is still observable; but on using the ice it disappears: the candle assumes its true character, and then he becomes capable of refreshing rest and sleep.

18th. While standing talking to me, and seeming quite well, he became silent for a moment, and then began to talk incoherently, saying, among other things, "Yes, I can take the flowers for my brother." His face became pale, and his hands quite cold, and, after I had placed him on a sofa, he turned a parcel, which he held in his hand, round and round, looking at it in a vacant unmeaning way. I, at once, applied cold water to the back of his neck, then eau de Cologne—blowing upon it to produce rapid evaporation; he was thus not only kept from becoming worse, but actually recovered a little; meanwhile I procured ice, the application of which speedily and completely restored him. His hands during his restoration became quite warm again. After recovering he was surprised to find some metal crests which did not belong to him in his pocket; these, which he must have appropriated while at a saddler's shop, whither he had been on an errand, he begged me to restore. I sent him home with a bag of ice along his spine, requesting him to continue the treatment last ordered.

July 10. Still no fit since the first day I prescribed for him. Sleeps well, and continues well in all respects. Each bag of ice to be now enclosed in thick flannel.

13th. No fit yet. Having been reading all day (Sunday), and having been without ice all the afternoon by way of experiment, he found, about five o'clock, that the words became unclear. He continued reading, when the words began to blend with each other, and then having got a candle to read by, he found the flames surrounded with a halo which gradually increased. About 8.30 p. m. he went out for a walk with his wife, when he observed the lamps looked fiery red, and as if surrounded by a haze of the same colour. On returning home and observing the candle he saw as far as the light extended a generally diffused red haze. He felt no pain, but great nervousness and excitability, and, as he says, would "soon have quarrelled with any one." He "then felt it time to stop," and therefore put on an ice-bag. After doing so he lay watching the candle-flame, which gradually assumed its natural appearance—completely so in about 45 minutes. He then fell asleep, slept soundly all night, and feels and looks quite well this morning. He ate half a pound of beef-steak on Saturday; he dared not have done such a thing, he says, before I began to treat him. To use only two bags of ice daily—one at night and one each morning.

August 17. This patient continued to improve in health and strength so decidedly, that early in this month I expressed the opinion that he might resume regular work, provided it were light.—*Medical Times and Gazette, Sept.* 2, 1865, *p.* 249.

15.—CROTON OIL IN NEURALGIC AND RHEUMATIC AFFECTIONS.

By Dr. S. C. Sewell, A. M., L. R. C. S. E., Ottawa, Canada.

[Dr. Sewell gives the following account of the use of croton oil in neuralgic and rheumatic attacks.]

I employed croton oil in other neuralgiæ with most gratifying results. Here are two cases of sciatica selected at random to shew the rapidity with which relief is afforded. In January, 1853, was sent for to Sandpoint to see

Mr. A. M., aged fifty-six. He was suffering under fearful sciatica. The history of the case was that he had had five or six attacks, and had never experienced any relief under a fortnight or three weeks. I gave two minims of croton oil. The bowels were acted upon with progressive relief to the pain, after every evacuation, the lancinating stabs diminished in intensity, and in seven hours there was only a slight dull pain between the sacrum and femur, and he was able to walk about., He got three quarters of a grain of muriate of morphia repeated for two or three nights, no further treatment being required. He was ordered pills of croton oil and ext. coloc. comp., half a drop in each dose to be taken when another attack came on. He was thus enabled to cut short the attacks for years, and has now been free from them for three or four years.

Case No. 2.—October 4th, 1864, at 5 a. m., was called to see Mr. H. M., who had sciatica of the left nerve. Had been ill for three weeks, said, "*that he had tried everything*" and was suffering worse than ever. He was lying on his right side, and could only turn his head a little to speak to me, and even that motion brought on a paroxysm sometimes. I ordered two drops of croton oil, which operated very copiously; at five that evening he was free from pain, got out of bed for the first time for three weeks, dressed himself, spent the evening with his family, and walked up and down stairs as well as ever he did in his life. After three or four days, the pain returned, but in nothing like the same intensity: the after treatment was in order as follows, turpentine, iron, iodide of potassium—the hot summer change of air; and, by the advice of a physician in New York, enemata of ext. belladonna at bedtime. In these and all other cases of neuralgia and rheumatism, where I have used this remedy, there is always a most extraordinary quantity of black fæces evacuated accompanied by very severe tormina. If the dose be repeated in two or three days, the evacuations will be natural and the tormina trifling. As these black stools always occur even if the patients have already taken Cooper's pills, calomel and jalap, or other drastics before my arrival, I am led to believe that there is some connection between most neuralgiæ and an impacted state of the saccula or volvulæ conniventes of the colon, otherwise it is impossible to explain where all these black fæces come from, and relief to the neuralgic pain *pari passu* with the progress of the evacuation. That croton oil exerts a specific action over neuralgiæ is probable from the following experiments made on two cases of sciatica. I selected elaterium as being the purgative most analogous in its ostensible operation to croton oil, but the evacuations were attended by no relief to the pain. Half a minim of the latter, given after the exhaustion produced by the former was over, gave the desired ease. There are certain forms of neuralgia almost wholly confined to women, although sometimes effeminate men and onanists are afflicted with them. These are neuralgiæ affecting portions of the fifth pair of nerves, and always associated with oligaemia more or less pronounced. The principal seats are half the scalp or hemicrania, the vortex, the exit of the three branches of the fifth nerve upon the face, the frontal and supramaxillary exits being more commonly the seats of pain. It must be first ascertained whether a diseased tooth be the cause of pain, or whether there be the suspicion of malarial taint; one will require the dentist, the other arsenic, &c. There being no suspicion of either, we have to observe whether the paroxysm be attended by local congestion indicated by slight heat and redness over the affected nerve. If so, then muriate of ammonia gives speedy relief, to be followed by constitutional treatment such as iron, porter, exercise and the shower bath. Should there be no local congestion, then chloroform, aconitine or atropine, applied externally or entropically, give ease, as does tr. belladonna internally, the constitutional resources being manganese and iron combined, quinine, exercise, sea-bathing, &c. But even in these cases we sometimes derive benefit from croton oil, where there is habitual constipation, or scanty blackish liquid stools, which often are present when the bowels are loaded with fæces. In these cases I give one quarter minim croton oil combined with ten grs. of pil. rhei co. The following is an instructive case, being one of malarious neuralgia. On 10th January, 1864, I

was called to see Mr. J. H., at 7 a. m. For nine days he had been seized every morning with severe neuralgia of the right frontal nerve. At first it began at seven a. m., but each morning it began earlier, and this day had awakened him at five o'clock. It always went off at one p. m. I ascertained that at the age of three years he had had an attack of fever and ague, and was still residing on the same farm, he now being twenty-eight years old. The place had formerly been much afflicted with intermittent. I gave him a drop of croton oil, which operated in half an hour; the pain immediately ceased: keeping the malarious cause in view, I ordered a scruple of quinine to be taken at bed-time. The pain never returned. I afterwards extended the use of this remedy to chronic rheumatism, which I divide as follows:

1st. Where the minute ramifications of the nerves supplying the muscles, and more particularly the terminal nervous loops, are the seat of disease. In this form the pain is constant and relieved by heat; it is a true neuralgia.

2nd. Where the muscular fibres are the seat of inflammation; when uncomplicated with the former, pain is only felt during motion or on pressure. The sterno-mastoid, the deltoid and the deep lumbar muscles are the most common seats of this form. It is alleviated by heat, and stimulating embrocations.

3rd. Where the periosteum and fibrous insertions of the muscles are at fault. Here the pain is deep-seated, affects principally the long bones, and is aggravated by heat, being worse when warm in bed. When the case has continued any length of time, the periosteum of the affected parts is always found thickened. I believe these cases never occur except where there is a mercurial, a syphilitic or a mercurio-syphilitic taint; these are amenable to iodide of potassium, biniodide of mercury, guiac. sulphur, ginger, medicated baths, &c.

4th. Where two or more of the above forms exist together.

The first, second, and fourth forms are all benefitted by the use of croton oil; the formula I usually apply is the following: ℞. Ol. tiglii ♏ ij. ext. colo. co. ℈ ij. M. ft. massula in pillulas xij. dividenda, sig. sumat i. vel. ij. vel. iij. p. r. n.

The first form being purely neuralgia, needs not to be illustrated by any cases; but it may be doubted by many whether the second form is equally benefitted, I shall give a case. On the 20th of July, 1860, 7 a. m., called on Mrs. W., æt. 22. who had been suffering for the three days with "*crick in the neck*," which had continually augmented in intensity. The sterno-mastoid was slightly swollen, painful on pressure, and the slightest movement, or even a person walking heavily across the floor, caused intense agony. This was without doubt a case of pure muscular rheumatism, I ordered a hot pediluvium and a liniment of equal parts of Fleming's tr. of aconite and laudanum with a little soap. An hour after, her husband came to me, and said that his wife could not stand the pain, and that something more must be done. I ordered two minims of croton oil, which she got at 9 o'clock. At 2 p. m. the pain was entirely gone. Pleurodynia is a good example of the mixed form, 1 or 2 being predominant. Commonly a hot foot-bath, fifteen grs. of Dover's powder, with or without a mustard plaster will remove the complaint in a few hours. Some cases, however, are more rebellious; these can be speedily cured in a few hours by the remedy in question.

In acute rheumatism, both fibrous and articular, I have fancied that it diminished the pain and shortened the disease; but many careful observations are needed to establish its value in a disease whose treatment is still a matter of such discussion, that many doubt the utility of any mode of treatment.

It is to be remarked that the use of croton oil does not always or even generally exclude other treatment. If in a case of sciatica it be resolved to follow up the treatment, by oil of turpentine, time must be given to allow the liquor sanguinis to re-accumulate, else one drachm will produce severe strangury.—*Canada Medical Journal, Dec.*, 1864, *p.* 277.

16.—NEURALGIA AND HYPOPHOSPHITES.

Dr. T. Hayes Jackson, Darlington.

[Last autumn Dr. Jackson was attacked with neuralgia in the back, left thigh, and leg.]

In a few weeks, the pain became more agonizing; and I lay in bed for days a gaunt specimen of mortality stuffed with drugs, potent and impotent. Hypophosphite of soda having failed to relieve (in September last) in ten grain doses (as recommended in an able article written by Dr. Radcliffe, in the British Medical Journal, for November, 1863), I determined to make a second trial of the hypophosphites of soda in much larger doses. I began with a drachm three times a day in beef-tea. In a few days, the effect was magical; the pain that I had endured for six long weary months subsided; and now I hope to enjoy better health, and thank God for all his mercies.—*British Med. Journal, March* 4, 1865, *p.* 234.

17.—OBSTINATE CHOREA TREATED WITH THE CALABAR BEAN —ENCOURAGING RESULT—PARALYSIS AGITANS TREATED BY THE SAME.

Under the care of Dr. John W. Ogle, Physician to St. George's Hospital.

[The patient was fourteen years of age, and had suffered from chorea for several months. Steel, antimony, zinc, arsenic, and valerian had been tried, but without any favourable result. On admission it was determined to try the effect of Calabar bean.]

Having obtained a number of the beans, I got Messrs. Bullock and Reynolds to make me a tincture or alcoholic solution. Of this I gave four drops for a dose thrice daily, increasing it gradually during the course of nine weeks until he took eighteen drops three times a day. During the whole of this period no change was observed in the pupils of the eyes, but the boy, without the use of further medicines, recovered almost entirely the power over his muscles, so that he left the Hospital and went home into Hertfordshire about the 6th or 7th of April almost quite well, the want of control in certain movements being only occasionally perceptible. He was requested to return to the Hospital if any relapse occurred, but we have seen nothing of him.

I have now an obstinate and violent case of chorea (in a young woman) in St. George's Hospital which I am treating with Calabar bean.

I have recently been trying the Calabar bean upon an out-patient with very severe so-called *paralysis agitans*, but I have not obtained (as, indeed, I hardly expected I should) any desirable result. Indeed, no result was obtained either as to power of controlling the movements of the muscles or as to the action of the pupils of the eyes. The form of preparation used was the alcoholic solution supplied to the hospital by Bell, of the strength of one drachm of the bean to one ounce of rectified spirits of wine. Of this, twenty drops were given, diluted, thrice a day, and increased, by ten drops at a time, to one drachm in the course of six weeks. For one month he took one-drachm doses thrice a day, and then it was increased to eighty drops. This he took for three weeks, and then he took one-and-a-half drachm doses for two weeks. At the end of that time, no effect being produced either on the muscles of the limbs or the eyes, I discontinued the remedy.

I have given the details of this unsuccessful case to show what doses of the Calabar bean might be given with impunity.—*Med. Times and Gazette, Sept.* 2, 1865, *p.* 256.

18.—ON A NEW REMEDIAL AGENT IN THE TREATMENT OF INSANITY AND OTHER DISEASES.

By Dr. S. Newington, B. A., Oxon., Ticehurst.

[The following remedy is a powerful and valuable means of withdrawing the blood from any diseased organ to which there is an abnormal determination.]

While staying at Matlock Bath, I was induced to try the effects of being wrapped up in clothes steeped in mustard-and-water, and applied to the whole legs and to the lower part of the abdomen. After the removal of a wet towel which had been applied round the head and was very uncomfortable, I began to experience the most soothing effects, and gradually passed into a dreamy semi-conscious state, which lasted during the half-hour I was under treatment. On getting up, I felt very lively and joyous, the liveliness lasted the whole day; and for nearly twenty-four hours there remained a pleasant tingling sensation in the legs, which were affected in no other way than by redness. It occurred to me at once that this kind of application might be very serviceable in certain cases of insanity, and immediately on my return home I set about making experiments for the purpose of testing its value. The first experiment was upon myself.

On retiring to rest I ordered a large basin of linseed-meal and mustard (ten parts of the former to one of the latter) to be made into a paste, and spread upon a sheet of brown paper sufficiently large to cover the whole abdomen, a piece of muslin being interposed to keep the skin clean. In a short time I fell asleep, and was conscious of nothing till eight in the morning, when I was partially roused by persons about me; but I was unable to speak or move. One of my medical assistants was thereupon sent for, and he pronounced me in a state of stupor from some narcotic. Though I was unable to speak, I heard the whole of the conversation, and was in a dreamy semi-conscious state. On the administration of some stimulant I presently recovered.

Another form in which I use the mustard is this:—Two handfuls of crude mustard are tied in a cloth and placed in hot water, then squeezed in the hand until the strength of the mustard has been extracted. A thick towel, long enough to reach round the loins, is then wrung out of this infusion, wrapped round the body, and covered with a large piece of macintosh. In one case a patient suffering from acute mania, who was restless, sleepless, and refused food, was thus treated with the greatest benefit. Before the application the pulse was 108, but after two hours of this treatment it had fallen to 60 in a minute, and the patient was in a quiet semi-conscious state. Afterwards he took his food regularly, and in a short time left perfectly recovered.

A third form in which this derivative treatment may be applied is as a mustard bath: in other words, an ordinary warm bath into which have been thrown five or six handfuls of crude mustard. In some cases the deep hip-bath only may be used; but in severe cases of mania the whole body of the patient, with the exception of the head, should be placed in the bath. A lady so treated, who had during the last year had four attacks of violent mania, each lasting for five or six weeks, has now for twenty-two weeks had no further attack, although the symptoms usually forerunning the seizure have on several occasions occured: the mustard bath appears to have warded off the recurrence of the excitement. In this case the bath was used once every twelve hours, for half an hour at a time, during a period of ten weeks; so that the skin was kept in a constant state of redness. It may be hoped that the habit of diseased action has now been broken, and that this patient, after due probation, may be discharged as recovered.

Mr. W. was brought to me in a strait-waistcoat, and as many as six people had been, it was said, necessary to control him before his arrival at Ticehurst. Notwithstanding repeated doses of opium, he had not slept for

six days and nights; and through the night after his admission he was excited, restless, and talkative. On the following night he was placed in a mustard bath for half an hour, so that he was perfectly red on being taken out. During the next eight days he had six of these baths, and at the end of a fortnight after admission returned home on trial.

A lady, who, notwithstanding repeated doses of morphia, had not slept for seven days and nights, was admitted in a state of mania, extremely incoherent and excited. After being in the mustard bath for half an hour she became calm and comparatively rational, and expressed herself as feeling much more comfortable. She was then wrapped up in a blanket and put to bed, where she soon fell into a sleep that lasted for seven hours; and in the morning she awoke free from excitement. The treatment was continued for six nights, and no further excitement occurred, although, as she had been insane for two years, her mind remained unsound.

In using the mustard bath, it is necessary to protect the privates with a folded dry towel; and it is of course desirable to have the bath placed near the bed, so that the patient may pass directly from it into his bed. If a little constraint is required on the first occasion of its use, it will rarely be found necessary on any subsequent occasion.—*Lancet, June* 10, 1865, *p.* 621.

19.—ON THE TREATMENT OF INSOMNIA.

By Dr. W. A. Hammond, New York.

[The measures to be adopted in cases of want of sleep may be arranged into two classes.)

1st. Those which, by their tendency to soothe the nervous system or to distract the attention, diminish the action of the heart and blood-vessels, and thus lessen the amount of blood in the brain. 2nd. Those which directly, either mechanically or through a specific effect upon the heart and blood-vessels, produce a similar effect.

Under the first head are embraced many agencies which from time immemorial have been known to cause sleep. Among them are music, monotonous sounds, gentle frictions of the surface of the body, soft undulatory movements, the repetition by the insomnolent of a series of words till the attention is diverted from the exciting emotion which engages it, and many others of similar character which individuals have devised for themselves. In slight cases the measures belonging to this class often prove effectual, but in persistent insomnia they are generally altogether nugatory.

Under the second head we shall find comprehended the means which are chiefly to be relied on in the treatment of cases of morbid wakefulness.

Bromide of potassium can almost always be used with advantage to diminish the amount of blood in the brain, and to allay any excitement of the nervous system that may be present in the sthenic form of insomnia. That the first named of these effects follows its use, I have recently ascertained by experiments upon living animals, the details of which will be given hereafter. Suffice it now to say, that I have administered it to dogs whose brains had been exposed to view by trephining the skull, and that I have invariably found it to lessen the quantity of blood circulating within the cranium, and to produce a shrinking of the brain from this cause. Moreover, we have only to observe its effects upon the human subject to be convinced that this is one of the most important results of its employment. The flushed face, the throbbing of the carotids and temporals, the suffusion of the eyes, the feeling of fullness in the head, all disappear as if by magic under its use. It may be given in doses of from ten to thirty grains—the latter quantity is seldom required, but may be taken with perfect safety in severe cases.

Alcoholic liquors are highly useful in asthenic insomnia, though in the sthenic variety they are of course altogether inadmissible. A short time since Dr. Van Buren, of this city, mentioned to me an interesting case of asthenic insomnia, in which he had used them with perfect success. They

should be given to the extent of producing gentle and continued stimulation for a few hours before bed-time.

The *warm bath* is also a very valuable means of determining blood from the head, and calming nervous irritability. Frequently, especially in children, I have found that simply putting the feet in water of the temperature of 100° F. has been sufficient to induce a sound and healthy sleep, when laudanum and other means have failed.

Cold water applied directly to the scalp is often of great effect in diminishing the amount of blood in the brain. It is not admissible in the asthenic form of wakefulness. When the individual is strong, the heart beating with force and frequency, and the mental excitement great, its influence is almost invariably good. The exact temperature is a matter for the judgment of the physician. I have often used it as cold as ice could make it—32° F. or thereabouts.

In the action of cold water, applied to the head in cases of insomnia, we have another proof of the real nature of this affection. It is known that in Thibet mothers place their wakeful children in such positions as will admit of a small stream of cold water falling from a slight elevation upon the head. I have in some work—on which I cannot now lay my hands—read a very full account of this custom, and seen a cut representing the process. The children very soon fall into a quiet sleep. I have often seen the application of the cold *douche* to the heads of refractory prisoners bring on a deep sleep.

The effects of *position* in aid of other remedies have also been alluded to. I make use of its advantages in all severe cases of insomnia which come under my charge, and we have, in its efficacy, additional confirmation of the correctness of the theory that the condition of the brain in such cases is one of hyperæmia.

The *diet* of individuals affected with morbid wakefulness must be regulated upon those principles of common sense with which every intelligent physician is acquainted. *Physical exercise* should be moderate and regular.

But all means will fail in those cases dependent upon severe and long continued mental exertion, unless the affected individual consents to use his brain in a rational manner. The disposition of the age seems to be to ignore the fact that the nervous system can exhaust itself by excessive intellectual labour. Even as these last lines are being written, intelligence is received from abroad that one of the most distinguished men of Great Britain has committed suicide, in consequence of insanity produced by over-exertion of his mind. Thus one more victim is added to the long list of those who have disregarded the laws of their being; and again we are reminded that there is a limit to the exercise of our intellectual powers, beyond which we cannot pass with safety.—*New York Medical Journal, June*, 1865, *p.* 202.

20.—ON THE HYPODERMIC INJECTION OF MORPHIA.

By H. W. Freeman, Esq., Resident Medical Officer to the Bath United Hospital.

[The following paper does not give anything markedly original, but offers some good observations on the hypodermic method in certain forms of disease, in comparison with the usual effects of morphia when taken by the stomach.]

It is just ten years ago since Dr. Alexander Wood of Edinburgh first introduced the injection of morphia into the cellular tissue of a part affected with neuralgia. Since that period it has been used extensively on the continent and in our own country, not only locally, but generally; the injection of the drug at a remote part of the body has not been found to materially alter its properties.

The endowed cancer ward of the Middlesex Hospital has, perhaps, afforded as large a field for the observation of its effect as most metropolitan institutions of its kind; and in, perhaps, but few hospitals has the hypodermic method of morphia received so fair and impartial a trial in all varieties of

cases, both medical and surgical. Long since it has stood alone as the great remedy for pain in those malignant forms of disease, no other class of remedies seeming so efficacious.

The most convenient form of injection is that by a syringe made by Coxeter, which is graduated into six minims. The standard solution used at University College and the Middlesex Hospital is a neutral one, and prepared so that six minims contain a grain of morphia. Sufficient acetic acid is used to dissolve the salt, and afterwards neutralized with liquor potassæ. The physiological action is the same essentially, although not quite identically, as when the morphia is taken by the stomach. Its effects, however, are more marked and increased in intensity when used hypodermically, one-third of a grain acting as powerfully as one grain taken by the mouth, and much more rapidly. Much as with opium, the action appears to be modified by age, habit, temperament, and idiosyncracy, the tendency always being to rapid alleviation of suffering, to stupor and to sleep. The stimulating stage, although brief, is marked, the patient first experiencing a sensation of slight vertigo and of approaching intoxication—then follow its calmative effects, and something approaching insensibility.

In illustration of the general phenomena, let me draw your attention for a moment to a case now under observation at the United Hospital, under the care of Dr. Falconer.

Louisa M., a middle-aged woman, is suffering from carcinoma of the pylorus, with intense pain, vomiting and its usual accompanying symptoms. After giving her an injection into the outer side of her arm with a third of a grain, a minute and a half to two minutes elapse before its effects are first felt. She complains of a tipsy feel, slight giddiness, and warmth of body, with other pleasurable sensations. The calmative feeling gradually passes over her in from ten to twelve minutes, proceeding from above downwards, and usually in fifteen minutes she is found sleeping. She remains in a state approaching torpor, and free from pain for a period averaging six to eight hours. Her pupils never markedly contract, and ten or twelve minutes elapse before any effect is produced on the irides. Her temperature rises 1° to 1½° for the first hour after the injection, generally declining as the effects of the drug pass off. This patient has now been under this mode of treatment for upwards of three months; and, although the disease gradually creeps on, and the effects of the drug are transient, yet it remains clear that pain is relieved, her existence rendered less miserable, and her life has been prolonged.

In this case, as usually happens, forty to fifty minims of the solution of acetate of morphia have been given, and repeated at intervals without relieving pain or producing sleep. Since adopting the subcutaneous mode, her appetite, before much impaired, has much improved; the nausea and vomiting have been considerably relieved; and, after the narcotic action of the morphia, she rarely, if ever, complains of headache, constipation, nausea, sweating, or dryness of fauces. This is only one of a number of similar cases accompanied by the same phenomena, in which we notice the absence of the usual after-effects of opium and morphia when taken into the stomach, where becoming mixed with the various ingesta, they find their way slowly into the veins, and are probably in part decomposed in the stomach by its secretion, and are carried out of the system without having entered the circulation; while, if given subcutaneously, the full dose becomes absorbed, and produces its narcotic effects almost at once.

As a remedy in *neuralgia*, much has been written for and against subcutaneous injection. It has been urged that its action is transient, its power non-curative, generating nausea, and sickness, and producing a deplorable state of system, worse than the original mischief. A portion of this might hold true in some individual cases; the same exception might be taken to the general class of narcotics; but, in taking the majority of cases, I do not think we can fairly assume that such always occurs practically. Two brief cases, shewing its value in some cases of neuralgia, may prove interesting.

Case 1.—Jane T., aged twenty-four, was admitted into the United Hospi-

tal, under the care of Dr. Falconer, with subacute rheumatism of the lower extremities. The blister mode of treatment was adopted. She drew rapidly to convalescence, but got a relapse, with intense neuralgia affecting the inter-muscular fibrous septa of the deep muscles of the thigh. A third of a grain of morphia was injected into the affected region. The usual phenomena were displayed, and within an hour she felt free from pain and remained easy for three days. Symptoms of the same kind of pain were then experienced in the other thigh; a fourth of a grain was injected. She got rapidly well, pain disappeared, and within a week after the first injection she left the hospital. No other drug was administered during the treatment.

Case 2.—Selina W., a hospital nurse, aged twenty, also under the care of Dr. Falconer, was taken with rigors, pain in the limbs, and acute febricula with intense lumbar neuralgia. The prodromata of variola were predicted. The febrile symptoms, however, gradually subsided, but the neuralgia remained persistent. A fourth of a grain of morphia was injected into the outer side of the arm. The calmative effects of the narcotic were well developed for some time, and existed over many hours; she became very drowsy, and inclined to sleep. She recovered without a return of the neuralgic pain.

Both these patients were nervous, hysterical girls, the latter stout and plethoric. Nausea was slightly complained of by one; no headache by either; no dryness of throat, or anorexia; and no subsequent return of the disorder. In these two cases we might observe that the pain was in each purely neuralgic, *unconnected* with any organic disease, or with any known irritation in any distant parts of the body; the effect of the drug being the same on each, although in one applied to the affected spot, the other at a distance in the arm.

Within the province of ophthalmic surgery, subcutaneous injections of morphia have been found of the most signal use.

Some papers of Professor von Gräfe, which appeared in the Journal of the Association a half year since, went to show the individual merit of atropine and morphine injected about the orbit for wounds of the cornea, ciliary neuroses, iritic or glaucomatous, and for various forms of neuralgia and reflex spasms, due to corneitis occurring traumatically or idiopathically. He said that the use of atropine by injection was very limited: to relieve pain, and to produce its mydriatic effect, it is preferable to drop it on the conjunctiva, unless the eye will not tolerate its presence. In neuralgia of the eye, atropine injected appears quite inert. Morphine I have seen repeatedly prove invaluable. A case in point occurred under my own observation whilst ophthalmic assistant to Mr. Soelberg Wells at the Middlesex Hospital.

Fanny B., a markedly strumous girl of sixteen, was admitted with granular lids, which had produced corneitis and pannus, accompanied with intense photophobia, lachrymation, and ciliary neurosis with excessive spasm of the orbicularis palpebrarum. It was impossible to get a glimpse of the eyeball unless chloroform were used. Atropine and belladonna were subsequently applied to the conjunctivæ for six weeks. No progress took place; spasm and the hyperæsthesia remained intense. It was found, however, on making pressure over each supraorbital nerve as it escaped from its notch, that the lids could be slightly separated; and as the supraorbital nerve gives off some few filaments to the upper lid, and the spasm being thought to be reflex, it was suggested to divide the nerve externally to the notch. Happily the injection of morphia over the brow was adopted; and within forty-eight hours, two injections, each one-sixth of a grain, having been given, the girl could partially open her lids without much distress, the hyperæsthesia daily became less with a nightly injection, and in a week or ten days she could see to read No. 8 of Jäger's test-types.

To further enhance the value of the hypodermic injection of morphia in a surgical point of view, it has been but recently discovered how marked are its effects in prolonging the anæsthesia of chloroform, by injecting from a *third* to *half* a grain of the narcotic before the patient becomes quite sensible to external impressions.

It is just a year ago since the *Versailles Medical Society* first published

their interesting researches on this subject of prolongation of anæsthesia. 1. Their conclusions went to show that five to ten *centigrammes* of morphia injected alone without chloroform produced a kind of intoxication, going on to torpor, but not giving rise to insensibility, properly so-called. 2. Salt of morphia injected during the anæsthesia of chloroform prolonged its duration in proportion to the quantity employed. 3. Although the experiments were performed on dogs, yet it might be used without danger on man, more especially when the duration of an operation gives rise to fears in continuing the anæsthesia by means of chloroform. Three months ago, a second series of experiments, made on dogs, went to support the surgical view, which was quite correct, and to show that injection might be employed without risk.

For upwards of two years past, all capital operations at the Middlesex Hospital have been treated in this way. All the amputations, ovariotomies, herniotomies, for the most part, have received from a third to half a grain of morphia subcutaneously immediately after the operation, and in the majority with good effect; and modern surgery knows how valuable it is in a practical point of view, to calm the nervous agitation of patients after operation, to lessen shock, to procure sleep, and, more especially in ovariotomies and herniotomies, to keep the stomach in a state of physiological rest for some hours afterwards. In these latter two classes of operation, nothing seems to do so well. Nausea might be induced; but whether from shock, chloroform, or morphia, is, perhaps, not quite clear; but, judging from the usual phenomena, it would be fair therapeutically to assume that most probably it arose from the effects of the chloroform, seeing that the after-injection of morphia entailed no such symptoms. Allow me to mention two cases, recently under observation, to bear out this fact.

Case 1.—Sarah S., aged forty, was operated on by Mr. De Morgan for ovariotomy; the operation lasted twenty-eight minutes. Half a grain of morphia was subcutaneously given when the chloroform anæthesia was subsiding. Some spasm occurred during the operation, and continued for an hour or so subsequently. At the end of three hours, a third of a grain was repeated, and again four hours after. After the second dose no tendency to sickness supervened; the patient became placid, and remained in a state of stupor. Some good sleep followed, and she awoke refreshed.

Case 2.—Caroline C., had amputation of thigh performed by Mr. Charles Moôre; the operation lasted sixteen to seventeen minutes. Half a grain of morphia was given subcutaneously. She became quiet; dozed for six hours; and had no sickness for twenty-four hours. The morphia was repeated with good effect; and the patient made a good recovery.

To compare the narcotic action of atropine hypodermically, with the like administration of morphia, is but to compare the therapeutical value of opium and belladonna as narcotics. The effect of atropine might be much more lasting, less transient, than morphia; yet the latter exerts its influence almost instantaneously. The use of belladonna must consequently be more limited in daily practice; and if its use be persisted in for any length of time, the probable atropial phenomenon, its liability to frantic excitement and gay delirium, with its own peculiar effects on the human system, would contra-indicate its continuance.

Atropine, as belladonna, relieves *par excellence* superficial pain. Morphia, as opium, relieves internal deep-seated pain. Yet, in this mode of administering morphia, it cannot be looked upon with impunity, knowing how great and powerful are its effects; and consequently great caution and a certain amount of discrimination are demanded in its administration.

Opium, inadmissible in some varied forms of disease, would be doubly inadmissible as morphia given subcutaneously. Opium is inadmissible in advanced stages of phthisis, in old standing cases of chronic bronchitis, or where the air-passages, loaded with profuse secretion, can scarcely be evacuated; and here morphia given subcutaneously, in a moderately large dose, would kill very rapidly; the tendency not being only to diminish the secretion, but to paralyse the action of the mucous passages in eliminating the

secretion. The wheezing dyspnœa of dry asthma would scarcely come within the category.

Habit, it appears, modifies the effects of morphia given subcutaneously; the dose requiring to be gradually increased, yet not so quickly as if taken by the stomach. Many a cancer patient in the wards of the Middlesex Hospital takes from two to three grains daily hypodermically, equalling six to nine grains of crude morphia taken by the stomach. Yet no other remedy seems to answer so well.

It might seem probable, that the frequent introduction of the perforating tube of the syringe into the cellular tissue would cause abscess or erysipelas; but, cautiously applied, it rarely does so. Most of the long-standing cases of carcinoma present areas covered with traces of the perforating nozzle, so as not to allow space for its repetition; and although the surface be tender, yet I have never seen either abscess or inflammation supervening. I have never seen or heard of harm being produced by this powerful remedy; yet I have witnessed results which, when it has been cautiously administered, have been as much satisfactory and astounding to the observer, as they have afforded comfort and alleviation of suffering to the observed.—*British Medical Journal, June 7, 1865, p. 639.*

21.—ON THE PROGRESS OF THE HYPODERMIC TREATMENT OF DISEASE.

By Charles Hunter, Esq., Surgeon to the Royal Pimlico Dispensary and Lying-in Charity.

My experiments in 1858 were the first to upset the previously maintained theory, that to produce a full medicinal effect upon any given nerve or locality, the medicinal injection should be into that locality itself.

As evidence opposed to that view, two cases of neuralgia were given by me, in which, as much medicinal benefit followed the adoption of my method as had previously followed the use of the method of Wood; and without being accompanied by the disadvantages, which must, under certain circumstances, attend the use of Dr. Wood's plan. So long ago as November, 1843, the injection of morphia had been used by Dr. Alexander Wood, of Edinburgh, and since that date it seems to have been extensively practised by him and his disciples, upon the principle laid down by him in that year. His instrument was made by Fergusson, of Giltspur street. The disease for which he used it was neuralgia; the agent was morphia; and his theory was that of localization to the neuralgic site.

"I would impress upon you," said Dr. Wood in 1858, "that the instrument is not to be put into the place where the patient complains of the pain, but into the spot where you find you can awaken the pain upon pressure."

Employed upon this theory, it seems that in 1858 the use of the instrument had become "nearly universal," for the treatment of neuralgia, in Edinburgh. The evils chiefly dreaded were gastric disturbances with vomiting, and excessive narcotism. Nepenthe seems to have produced less sickness in the hands of Dr. Wood than opium or morphia, but no one of these agents was found to be infallible.

For fifteen years—namely, from 1843 to 1858—the *local* injection of Wood seems to have been more and more employed in Scotland, but it was little, if at all, known or used in this metropolis. It is right to state here, that Mr. Rynd, of Dublin, claims priority in this matter. "The subcutaneous injection," says he, "of medicinal substances, to combat neuralgia, was first used by myself in Meath Hospital in May, 1844. The *Bulletin Th rapeutique* for 1861 says that Mr. Rynd employed a solution of ten grains of morphia in a drachm of *creosote* for his neuralgic and sciatic cases, a compound I should be very sorry to inject beneath the skin of any individual.

His instrument was, I believe, a clumsy one for this purpose, as a lancet had to be used, before inserting the point of the syringe. The syringe for injecting nævi and hemorrhoids, which Dr. Wood employed, was, I suspect,

inferior in construction to the one I now use, for he states that he thought it best to dissolve his morphia in sherry wine, rather than in water, lest the latter should rust the instrument.

The syringe which I am in the habit of using for injections will not rust; it is a modification of one introduced by Messrs. Savigne in 1857 (and described in the *Illustrated Exhibitor* of 1858). The tip of the pipe is finely pointed, and made of *hardened* gold, in which two particulars it differs from the French one invented a year or two before. In October, 1859, I had the barrel of the original syringe enlarged to hold thirty minims, the screw more carefully regulated, and the glass barrel graduated.

In 1858, the plan of Wood was first used in St. George's Hospital, Dr. Pitman having kindly consented to let me try it upon a man (T. G.), who had suffered frightfully from neuralgia for four years. At the same time I employed it in a case of severe hemicranial neuralgia with disorganization of the eye, in a young woman (E. P.), about eighteen years old. Both cases received decided benefit, neither was speedily cured; both had abscess in the neuralgic site, from the continuance of the localization.

I then varied the site of the injection, chiefly puncturing the arm, in both patients. The man went out cured, the young woman went to the Middlesex Hospital. She was the first patient treated by subcutaneous injection in that institution. Under the care of Mr. De Morgan, the varied site of injection was had recourse to, and the same solution that I had used at St. George's was employed. She received great benefit, and has now been for several years free from neuralgia.

It appeared fair to deduce from these cases:—

Firstly. That in neuralgia equal benefit followed *distant* injection of the cellular tissue, as followed the injection *of the neuralgic site.*

Secondly. That localization was not necessary to benefit a given part.

Thirdly. That for certain reasons it was better not to localize.

The chief being:—1. The infliction of unnecessary pain; and 2. The almost certain risk of irritating, thickening, or inducing matter in the part from the repetition. It, moreover, became evident that a large class of neuralgia would be excluded from this treatment if it was necessary to inject the neuralgic site.

But as my experiments showed such was not the case, I inferred, that diseases of any one part of the body could be effectually treated by injection of cellular tissue of any other part. Before introducing this as a *law*, it would be better to furnish one or two *more striking* examples than the two first mentioned, and as regards cure, rather prolonged cases. I will take, for example, such forms of neuralgia as would have been treated in Edinburgh by the local injection.

Case 1.—On the 3rd of June, 1863, Mrs. B., aged sixty-three, consulted me about an attack of sciatica from which she suffered. She had walked lame on account of the pain for two years. Rheumatism and great mental anxiety had preceded it. She had taken two or three grains of morphia daily by the advice of a London physician, up to the time I first saw her. The atropine injection, $\frac{1}{10}$ of a grain, in the arm, produced its characteristic phenomena upon the system; and forthwith removed the pain. She had no pain from that time for about a year, when she came up from the country and again received similar benefit.

In January, 1859, Mrs. R. had severe facial neuralgia, which had lasted some months, and defied ordinary internal as well as local treatment. I injected the arm of the same side of the body, with the acetate of morphia. It caused some gastric irritation, and yet cured the neuralgia. She has had no return of it since that single puncture six years ago.

Case 2.—G. C., aged nineteen, tic-douloureux, left facial region. Duration, three months, quinine and other treatment previously tried without effect for several weeks. Atropine puncture was used on November 16th, 1863, and at intervals of two or three days as the pain happened to recur; each puncture removed the pain before he reached home. He was cured in a fortnight. No

return when seen five months after. The injections were made in the arm of the opposite side of the body.

I will now pass on to the second part of the subject—viz., the injection of medicines *into the cellular tissue*, in order to ascertain their *general effects* upon the system. This first took place in November, 1858. *Case.*—Edmund Harrison, under Dr. Page, at St. George's Hospital, suffering from delirium tremens, was the first case thus treated. He had been very violent, and without sleep for two nights and three days. Half-grain doses of morphia, by the mouth, had been repeatedly given with no effect. I injected, at the request of Dr. Page, one-half grain into the neck; the patient was soon quiet, and slept altogether about twelve hours. He rapidly got well with the aid of two subsequent injections.

From the good effects of the morphia in this case and a few others, I deduced "that, by the introduction of narcotics into the cellular membrane of the body, we have a mode of attacking and subduing cerebral excitability, more rapid, more certain, and more pure in its action than by the stomachic doses."

Such was my proposition in 1858 relative to cerebral diseases. It was silently received but practically taken up by many leading men, especially in the hospitals of large towns.

My subsequent experience not only corroborates that conclusion, but now leads me to add the following:—That in cerebral affections, medicines called anodynes frequently act so far more effectually, and so differently from the stomachic doses of the same, as almost to warrant us in considering them as different medicines.

Let morphia be taken as a type, it is the agent used more than any other, and has, therefore, been no doubt used both ways by many now present. Taken by the stomach in any ordinary case, let us see how it often acts.

It makes the patient feel sick and qualmish, it nauseates, it even produces vomiting, it causes sleep, but the sleep is disturbed and excited; the patient dreams more than usual, he awakes in the morning with a dry mouth, with a coated tongue, he feels sick on waking, and does not enjoy his breakfast, neither do the liver or the bowels act as usual, he feels a tightness, or a weight, or a giddiness in the head, throughout the day. In fact, the agent given by the mouth, to *reach* and to *quiet the brain*, has acted as an irritant to the pneumogastric and sympathetic nervous system in contact with which it was first brought.

Let us now see how the patient fares with the same dose given by the cellular tissue. The morphia goes by a shorter road and reaches the goal at once. The pulse instantly rises, the system glows, the face may slightly flush as the skin gets warm, and rapidly perspires. The pulse then goes down ten to twenty pulsations below the normal. The brain is soothed, sleep is gentle and happy. The patient awakes restored. There is no dryness of the mouth, no nausea, no sickness nor constipation of the bowels, for the morphia has gone to the brain by the shorter and more direct road. The skin is more acted on by this mode, not as a result following the nausea of the *stomachic* dose, but almost as a *direct* effect synchronous with the *first* quiescence of the brain.

I will now proceed to indicate a few of the special advantages that attend the injection of medicines into the cellular tissue, when used for the treatment of diseases *distant* from the site of injection.

1. For derangements of the *cerebral nervous system*, we have, in the hypodermic method, a means of treatment, far superseding in its immediate efficacy any other mode of medication.

In *cerebral* cases, which I have placed at the *head* of the list, we have hardly to consider pain at all. We have to deal with a sensitive and perceptive organ, not with a nerve. An organ which, for the due performance of its functions, ought really to be at rest one-third of our lives, a third of each day. Pain in these cases is only an occasional symptom—it is one among others of cerebral nervous exhaustion.

The more frequent symptoms of cerebral over-taxation are insomnia, irritability, excitement, melancholy, delirium, mania, loss of reason, &c.

These I submit to be all phases or degrees of cerebral excitement and exhaustion—the one merging into the other; the mildest often becoming the most severe, simply because it was not checked in time. *Insomnia* is a cerebral condition that may come on during any bodily illness, whether accompanied by pain or not. The *anxiety* attending the illness may often be the only cause of the wakefulness in a nervous and irritable person. It may occur without any bodily disease, as in the hysterical, &c.

Prolonged wakefulness for many nights or weeks together may lead to excitement and delirium in one temperament, to melancholia in another, later perhaps in life. Opium and morphia will usually cure these derangements—it will *give* sleep and prevent simply insomnia going from bad to worse. In many cases the opium does its duty well, when given by the mouth; in others, it distresses the patient in various ways. Where the natural tendency is to relaxation of the bowel it seems to suit better than at other times. When constipation is natural, it makes matters, in that respect, worse.

Given by the cellular tissue, neither morphia nor opium ever constipates. I have used the morphia injection for weeks together, in cases of chronic disease, the bowels acting daily—but never acting without medicine in the same patients, if the morphia was given by the mouth.

Neither is the stomach deranged, nor the appetite interfered with, as with the stomachic dose. In many cases I have found the appetite *improved* by the injected dose.

2. *In melancholia;* the effects of morphia thus administered are decidedly beneficial. Opium in some form seems to replace the missing elements in the desponding and sleepless brain, and it does so without producing anorexia, &c. The morphia-puncture will in a few minutes restore the lost equilibrium—it will make the patient feel cheerful and happy; he will then eat and take exercise, or go through those professional duties he had entirely despaired of performing, perhaps only an hour before.

3. But it is in *delirium tremens* and in *mania*, more than in any other disease of this class, that the immediate benefit is most manifest. Although differing in their causes, I class these diseases together, for whether *mania* arises from violent excitements, from drink, or from exhaustion, as in the puerperal woman, still it resembles *delirium* from drink in this important symptom—the *insomnia*.

Most cases of this description have had large and oft-repeated doses before they are admitted into hospitals; but they have failed to give sleep—the insomnia is almost the essence of the disease—a good sleep obtained, the case is half cured.

It is in this class of cases that a single dose, administered beneath the skin, will at once break the neck of the disease. It will often at once stop the delirium, correct the mental aberration, and remove the exhaustion as produced by the injection. It is the food of the brain, the sustainer of nerve-*force*.

Much is gained by this plan in *delirium tremens*. The patient will not swallow—the subcutaneous injection obviates that difficulty. The stomach will not absorb or digest the opium—the morphia put beneath the skin is absorbed in five minutes. Time is lost by the stomachic dose; it is gained by the hypodermic; uncertainty attends the one plan, certainty the other. Dr. Robert Ferguson told me he counted twenty-four one-grain opium pills in the stomach of a patient who had died in some hospital—unabsorbed, those pills can have done no good, but rather harm, embarrassing the prescriber.

Another advantage is that *restraint is avoided.* During the hours and days that we might wait for the unabsorbed opium to act—the violence, or rather the incessant continuance of the muscular movements, has often seemed to indicate the necessity for the strait-waistcoat. This helps to cause, or increase venous congestion. In fatal cases Dr. Barclay thinks the opium has often borne the blame when the forcible restraint was really more at fault.

By the hypodermic plan the *necessity for restraint* is done away with; the patient, if not asleep in twenty minutes, is almost always perfectly quiet, neither talking nor moving: and thus we see another advantage that we gain, *the avoidance of exhaustion.*

To resume, briefly, concerning cerebral affections. This treatment has advantages especially its own, restoring the proper function of the brain, composing, and giving it sleep and tone, when the same medicine otherwise given has only caused irritation, and delayed the cure, which by the injection would have been more quick, permanent, and effective.

Passing to the *spinal affections*, we find the same advantages to attend the use of the treatment, whilst the medicine employed, and the nature of the disease, may vary much. Here *pain* in some form or other, *excessive muscular* action, or its *complete deficiency*, are the three chief evils to be contended with.

Insomnia in these cases is an induced or secondary evil.

Morphia is far from being the panacea, even when thus injected, in this class of cases, although so valuable in cerebral affections.

Acute sciatica, for example, may prevent sleep for many nights together; the morphia injection may meet the insomnia, but it does not always cure the sciatica; it palliates the *induced* symptoms, but is not always the agent to strike at the root of *this* disease.

M. Ozanan, very rightly, I think, holds the view that the various alkaloids in opium have each a part of the nervous system on which they more particularly act—morphia, on the cerebral hemispheres; thebaine, on the upper dorsal; narceine, on the lumbar region, &c.

And so with other alkaloids: atropia and strychnine, I believe, strike more surely at the root of the sciatica in many cases, than does morphia or any opium alkoloid. They produce a more tonic change in the affected nerve; and they *do so best* when injected beneath the skin.

I have often wondered whether belladonna and its alkaloids fairly reach the affected nerve when given by the mouth—so long may they be given stomachically with little or no benefit. Do they undergo chemical transformation? is a question one may fairly ask. The woorali is nearly inert when taken by the mouth; I have given from three to six grains to a patient in the course of a week without any marked effect, by that plan; but I have paralysed the legs of a rabbit for many days with only the ninth of a grain when injected.

In *painful spasmodic affections* how important it is to be able at once to arrest both the *spasm* and the *pain.*

In severe *colic* in a painter, who for hours had been doubled up in agony, a single morphia puncture relieved the spasm, and of course, the pain, and the bowels, which had for some time been confined, acted of their own accord; the castor-oil that I ordered having been forgotten by the patient.

Dr. Ward, of Winkfield, has found the morphia-puncture successful in this disease.

In *retention of urine*, when due to *spasm*, this treatment may be often useful, and relieve the patient in a very few minutes. I was led to use it in one case, having no catheter at hand; micturition took place in the course of ten minutes. When no hot bath is at hand, the catheter absent or contra-indicated, I venture to suggest the morphia-puncture as a ready method of relief.

On the 5th of November, 1858, I believe the first case of tetanus was treated hypodermically; the patient had been admitted into St. George's Hospital with a gunshot wound. The injection in this case was morphia, which gave the patient sleep, but the spasms continued unaffected. Since that time, many cases of lockjaw have been treated hypodermically, and a few cured. The agents that have been injected in this disease have chiefly been the woorali, atropia, nicotine, morphia, and aconitina; the operators MM. Vella, Briquet, Courty, Claude Bernard, Follin, Spencer Wells, Dr. Benoit de Giromagny, Fournier de Souisson, &c., &c.

In epilepsy, the effects of atropia injected seem superior to the stomachic

effects, being more permanent in curing the fits, and in lengthening the duration of time between the fits in chronic cases. There seems, as in chorea, a kind of tolerance of this agent in many cases. In paralysis, the strychnia seems in many cases more beneficial than when given by the mouth.

Limited space prevents me from entering further upon this class of cases, interesting as the results are, of atropia in epileptic seizures, and of strychnia in painful and paralytic cases; these may form a subject for future consideration.

I pass now to another important object with which the hypodermic treatment may be used—namely, *to diminish vascular action and inflammation.*

When morphia and atropia are given by the mouth, very little effect is perceptible upon the pulse. If they make the patient sick, the effect of depression, &c., is secondary; but injected beneath the skin, a primary, direct, and very different effect is produced upon the heart and arteries by these two agents: the one as quickly lowers as the other excites the pulse. Dr. Harrington Tuke felt the pulse of a patient with puerperal mania, that I injected in Queen Charlotte's Hospital, go down from 140 to 80 in four minutes. Mr. Holmes, at my request, the other day felt the pulse of a patient twenty minutes after an atropine puncture; the pulse had risen, as it usually does, to 120. The deduction is, I think, plain that morphia thus administered may be used as a decided antiphlogistic, but atropine not so. T. B., in St. George's Hospital in April, 1859, suffered excessive pain in the head, with chemosis and severe inflammation of both eyes. Leeches, blisters, calomel, and opium, and morphia by the mouth every three hours, failed to diminish the pain or arrest the inflammation. The morphia-puncture acted like a charm; it completely removed the pain, and the inflammation then rapidly subsided. Dr. Ogier Ward in 1860, just one year after the above case was described by me as an instance of inflammation, wrote as follows:—

"Another conclusion may, I think, be drawn from these cases—viz., that pain of any kind, neuralgic or inflammatory, may be relieved by subcutaneous injection; and as we can only explain the more rapid action of opiates administered in this manner by their immediate absorption into the circulation, it follows as a necessary corollary that in violent pain from acute inflammation, we need not wait to subdue it by antiphlogistic remedies, but proceed at once to the use of anodynes."

Thus, independently, and from his own experience, Dr. Ward has confirmed my views on the subject.

In *peritonitis* I have found the morphia-puncture of great value. It helps at once to check the three prominent symptoms—the excessive pain and tenderness, the rapid pulse, and the short quick breathing; it then gives or *allows* sleep.

In *ileus*, in a severe case in which there was every symptom of perforation of the bowel, Dr. Godwin tells me he found half a grain of morphia injected beneath the skin to relieve all the urgent symptoms in about ten minutes.

In ophthalmic practice, Dr. A. von Gräfe has found the hypodermic injection of especial value; and has written a paper to show the various objects with which it may be used in ophthalmic surgery.

In *ague* and *fever*, the hypodermic injection has been extensively used, especially in the Smyrna Hospital, by Drs. Chasseau and McCraith; also by Dr. Moore, of Bombay; also more recently by Dr. Desvigne in France. All these physicians agree in the superior efficacy of quinine when injected into the cellular tissue over the stomachic doses. The same rule holds good with quinine as I had shown was the case with morphia—viz., that a lesser quantity was equal to a greater, given by the mouth. "Four or five grains," says Dr, Moore, "are equal to five or six times that amount by the mouth."

I must protest once more against the way all these gentlemen introduce the quinine—namely, with a syringe after incising the skin with the *lancet!* According to Dr. Desvigne, abscess and sloughing of the cellular tissue are results not extremely uncommon, but such as, I think, should never occur.

For the various forms of *uterine pain*, especially for dysmenorrhœa, the hypodermic treatment has been used by Dr. H. Bennett, of Mentone. In all cases, he says, he obtained the benefit in from fifteen to thirty minutes without any of the drawbacks which follow the stomachic administration.

I cannot conclude without remarking, though very briefly, on the use of this treatment for *incurable* disease. In my own hands it has been a boon to many patients for months, perhaps, together, some with incurable neuralgia, others with cancer, or the agony of diseased joints. Life is made bearable by this plan to these poor sufferers. The economy of the amount of the anodyne required holds good in the majority of these cases. Dr. Giles tells me that he has now a case of cancer under his care in which one-third of a grain injected on alternate days gives all the relief that two grains daily had previously given by the mouth. Such has often been my own experience in this disease. In the cancer wards at the Middlesex, Mr. Watts and others tell me that morphia-puncture has for a long time been the main treatment employed, some patients being injected once, others twice, daily. As an *antidote*, and as a diagnostic test, I have long since ventured to propose this plan as the most reliable in cases in which the agent to be employed is fairly soluble.—*Medical Mirror, July and August*, 1865, *pp.* 392, 439.

22.—DANGER OF SUBCUTANEOUS INJECTIONS.

Professor NASSBAUM, of Munich, has just published an interesting account of an accident which happened to himself. Suffering from neuralgia, he had injected morphia under his own skin more than 2000 times—sometimes to the extent of five grains of morphia in twenty-four hours. Two months ago, he injected two grains of acetate of morphia dissolved in fifteen minims of water, and accidentally sent it direct into a subcutaneous vein instead of into the cellular tissue. He gives a graphic account of his dangerous position for two hours, after which the effect passed off. He has seen similar effects in a smaller degree in two of his patients, and the practical lessons are, that as it may be impossible to avoid veins at all times, and one may be punctured unawares, subcutaneous injection should always be done *very slowly*. The effects are so instantaneous that the syringe can be stopped at the first sign of danger, and some of the injected fluid mixed with blood may even be sucked out again by the syringe. It is very remarkable how the effects of the same dose of the same substance differ when directly injected into a vein and mixed with the venous blood, and when they filter into the blood from the cellular tissue through the unbroken coats of the vessels.—*Med. Times and Gazette, September* 23, 1865, *p.* 337.

DISEASES OF THE ORGANS OF CIRCULATION.

23.—CASES ILLUSTRATING THE ACTION OF DIGITALIS ON ENFEEBLED HEARTS.

(Under the care of the Physicians to Guy's Hospital.)

[In some clinical remarks Dr. GULL referred to a case which had been in the hospital some years ago.]

It was that of a man who was supposed to be suffering from a dilated left heart. The rhythm of the heart was exceedingly irregular, the patient was very much distressed with shortness of breath, dropsy, and other symptoms. Digitalis was prescribed for him, and it appeared to do him a great deal of good. The heart's action became by degrees more and more regular, his breathing very much easier, and in every respect he was thought to be doing well, when suddenly he died.

Dr. Gull further remarked that he could call to mind about half-a-dozen

cases of dilated irregularly acting hearts in which death had occurred suddenly, and it so happened that in every case the patient had been, or was at the time, taking digitalis. Yet, on the other hand, he had many times prescribed digitalis to patients with very irregular, feeble and dilated left hearts, without valvular disease, and with such marked relief and benefit to the patient that the remedy had gained for him the greatest credit. Nevertheless, when digitalis is given in such cases, he considered that the patient ought to be watched day by day, and that this medicine should be discontinued as soon as the heart begins to be more regular and the impulse stronger. This observation Dr. Gull made particularly with reference to the case of dilated, feeble, and irregularly acting hearts. He, however, still further observed,—"I look upon it as a fact, which clinical experience has clearly and definitely settled in my own mind, that digitalis is capable of diminishing the frequency of the heart's beat and of increasing the power of the heart's impulse; indeed, in cases of disease of the left side in which the acting of that organ is very rapid and feeble, if I prescribe digitalis I confidently expect that it will diminish the rapidity and increase the strength of the heart's action."

The next case was that of a patient with dilated irregular heart, no bruit, but physical signs of bronchitis, under the care of Dr. Wilks.

Mary J., aged fifty-four, admitted April 14 into the Clinical Ward. She was a tall, well-built woman, who stated that she had usually enjoyed good health until a few months ago, when she began to suffer with shortness of breath and with a bad cough.

When admitted she complained very much of her difficulty in breathing and of her inability to lie down in bed. The apex of the heart was lowered, and the area of cardiac dulness was considerably increased. The rhythm of the heart was irregular, but no bruit was heard. The first sound, however, at the apex was very feeble—in fact, inaudible. Large crepitation was heard all over both sides of the chest. She was ordered tincture of digitalis, ten minims; decoction of senegæ, an ounce every six hours.

April 19. She expressed herself as feeling easier. The heart's action had improved. Her cough was very troublesome. ℞. Mist. cascarillæ co., ℥j.; sp. ether sulph., ♏xx., ter die.

After this she day by day gradually improved; her heart became more regular and her breathing much easier up to the 26th, when Dr. Wilks found her up and very much better, and she had been able within the last day or two to move about the ward, when on the 27th she was sitting on the bed, dressed, and apparently going on well. The Sister of the ward happened to say to her that if she wanted to amuse herself she might help them to dust the ward. The patient made an attempt to get off the bed, but fell back and died instantly.

The Autopsy showed the left heart dilated and somewhat hypertrophied, but, as Dr. Wilks remarked, much more dilated than hypertrophied. The lungs were emphysematous, and the lining membrane of the bronchial tubes very vascular and contained some viscid mucous and purulent matter in the tubes. The kidneys were large and granular.

The next case was also an example of dilated left heart without valvular disease, under the care of Dr. Wilks.

John W., aged forty-six, a farm labourer, was admitted into the Clinical Ward, June 7, 1865. He said that he had had rheumatic fever five years ago, but had been perfectly well up to a month since. He then got wet through, and difficulty of breathing and aching pains in the limbs followed. Shortly afterwards his legs began to swell, and he was then compelled to give up his work. He had been troubled lately with vomiting and purging.

He was a tall, hardy-looking man, with large bones and well-developed muscles. His features were large and regular; alæ nasi thin; upper lip long and rather thick; teeth sound and regular, excepting the molars, which had decayed. He stated that he had always been temperate.

His present symptoms were great dyspnœa with hurried, heaving respiration, 30 per minute. His countenance was distressed and turgid. His expression was such that there could be no doubt but that he was very

ill indeed. His heart's action was very peculiar. It was rapid, irregular, and at times appeared to intermit. Indeed, the heart seemed to tumble about without any real rhythmical action. His pulse was extremely weak and could scarcely be felt, and it was impossible to count it. There was increased dulness over the region of the liver, extending nearly down to the crest of the ilium. His hands and feet were cold. Bowels had been open several times. Urine became slightly opalescent on boiling and after the addition of nitric acid. His feet and legs were swollen and œdematous up to the knees. ℞. Infusi digitalis, ℥j., 6 tis. horis.

8th. He had a sudden paroxysm of dyspnœa and great pain over the præcordial region late last night. His limbs became cold and his breath felt cold. He had taken one dose of his medicine, but that was now ordered to be discontinued, and brandy was administered. Hot poultices were applied over the præcordia, and he slowly rallied again.

9th. Dr. Wilks saw him early this morning. He appeared exceedingly ill. Pulse very feeble, and could not be counted. His breathing so short that no careful examination could be made. ℞. Tincture of digitalis, 3ss. 2 tis. horis.

After taking two doses of this mixture he appeared to improve, and expressed himself as feeling easier.

On the 10th he died.

Autopsy.—The head was not examined. The lungs were in that condition familiarly known as "heart lungs." The heart was very much enlarged, especially on the left side. The left ventricle was not contracted, and contained a small clot and some fluid blood. This ventricle was very much dilated, and its walls did not seem to be hypertrophied, neither were they attenuated. The endocardium was thickened, but there was no valvular disease. The muscular tissue of the ventricle was pale looking, and easily broken down under the pressure of the finger. Dr. Wilks called attention to the increase in the connective tissue, extending into the muscular substance of the left ventricle, which apeared as bands or layers, extending from the thickened endocardium amidst the muscular structure. The coronary arteries were not rigid, nor otherwise diseased to the naked eye. The liver was large, and had the usual nutmeg appearance in heart disease. Spleen very firm; otherwise normal. Kidneys large and apparently highly congested; on section, looked coarse, but not exactly granular.

A microscopical examination showed the muscular tissue of the hearts of the last two patients to have undergone fatty degeneration. In the case of this man this degeneration was exceedingly well marked. The transverse striæ of nearly all the muscular fibre had been replaced by bright, sparkling granules. On section, showed the connective tissue between the bundles of the muscular fibres unusually distinctly, and it had been evidently increased in quantity. In this case the fatty metamorphosis would appear to have been due rather to the pressure of the increased quantity of the connective tissue on, the minute capillaries than to disease in the coronary branches themselves.

Dr. Wilks, before ordering digitalis for this patient, remarked that, judging from his experience, this was just one of those cases in which digitalis might be expected to do good; but whether it would save this patient was somewhat questionable, considering "how very ill he was." Dr. Wilks stated at the same time that he had lately seen a case of a woman who was apparently suffering like this man, with dilated, enfeebled left heart. There was great dyspnœa, and excessive but very irregular action of the heart. No perceptible impulse. The first sound could not be distinguished. The pulse was exceedingly feeble and quick. The patient was in the greatest distress. He recommended that half-drachm doses of the tincture of digitalis should be given every four hours. After the patient had taken a few doses she began to gradually improve, and continued to do so, and became able, although but for a time, to get about again.

Another case, under the care of Dr. Wilks, that we had the good fortune to see, was that of a man who had been admitted into one of the Surgical

wards for an injury, but as his breathing was so very short, and he was expectorating a quantity of bloody mucus, and as he had other symptoms indicating chest disease, Dr. Wilks was asked to see him. Dr. Wilks found that the patient was suffering from dilated and enfeebled heart; but there was no bruit to indicate valvular deficiency. The rhythm of the heart was exceedingly irregular. There were dulness and crepitation at the base of the right lung, supposed to be due to pulmonary apoplexy. The liver was enlarged, and could be distinctly felt below the level of the umbilicus, and his face and conjunctiva had a yellow tinge. His feet were œdematous, and his urine scanty and high coloured, but it did not contain albumen. The patient was ordered half-drachm doses of tincture of digitalis every four hours.

We had an opportunity of watching the patient day by day. After the second day he began gradually to improve, his heart became more regular, the first sound could be distinguished, and the dyspnœa subsided, and after a week he was very considerably better, and he quite got over the attack. The dose of the digitalis was diminished as the patient improved.

Having now briefly mentioned these interesting cases, we will next proceed to inquire how far the sudden deaths of these patients were really due to the action of the digitalis, and how far to something connected with the disease itself. And before it is possible to come to any conclusion on this point, it is absolutely necessary that we should endeavour to determine whether experience and pathology tend to teach that patients, with enfeebled dilated left hearts without valvular, are liable to die suddenly.

Clinical experience in such cases tends to show that the rule is for such patients not to die suddenly, but rather to suffer with dropsy, hepatic enlargement, albuminuria, and with other complications, and that death is most frequently due to uræmia, to pneumonia, acute bronchitis, apoplexy, or some other accidental cause. Nevertheless, although such is the rule, we think that not a few cases might be collected which would tend to show that those suffering with this disease are liable to die suddenly. We have notes of one case in particular, which came under our notice.

A gentleman, aged sixty, was found suffering with well-marked symptoms of dilated left heart without valvular disease. His heart's action was exceedingly irregular, and there was no visible or perceptible impulse and no bruit, and the area of cardiac dulness was markedly increased. Mucous râles were heard all over the chest. There was no albumen in the urine, no marked hepatic enlargement, and but slight œdema. He was ordered to remain in bed, and five grains of ammonio-citrate of iron were prescribed to be taken three times a-day.

He remained much the same for some days, but after a week the heart's action began to improve; day by day it became more regular, and at length —fourteen days after he had been first seen—finding that his pulse was almost quite regular, that the rhythm of the heart was equally so, and as he appeared otherwise much better, he was allowed to get up and dress himself. The day after this he did not appear any worse. He was cheerful, and expressed himself as feeling better. He got up, and remained up all day, and seemed to be doing well. About 8 o'clock, however, he said he wished to get into bed; he made an effort to do so, when he fell forwards and died.

We thus see that with dilated heart death may occur suddenly, whether digitalis be given or not. We will next inquire if there is anything in the morbid anatomy or known pathology of the disease which leads us to think that such patients may die suddenly.

In by far the majority of cases of cardiac disease in which death has occurred suddenly, and that really due to the heart itself, there is evidence of degeneration in the muscle of the heart. Of course we make an exception when death is due to sudden blocking of the mitral orifice or to any analogous event.

Experience has shown that persons whose hearts have undergone fatty degeneration are liable to death from syncope; and, moreover, it has further shown that the greater the dilatation of the cavities, and especially of the

left ventricle of the heart, and the less the hypertrophy, the greater the danger to the patient. Dr. Walshe, in speaking of the liability of patients with aortic regurgitant disease to sudden and instantaneous death, says that hypertrophy of the left ventricle mysteriously lessens the likelihood of sudden death.

In comparing this kind of dilated left ventricle with that which is occasionally seen as the result of acute rheumatism in young persons, we perhaps ought not to consider that the liability to sudden death is equal in both. For in the latter it is comparatively, although perhaps not strictly, a local disease, and tissue in other parts of the body is tolerably healthy; whereas, in the case of pure dilated heart in the person of middle or rather advanced age, there is degeneration in the blood vessels throughout the body, and in consequence disturbed function and atrophy in most of the organs of the body. The consequence is that while in the one patient any functional disturbance in one organ is likely to be compensated by increased functional activity in another organ; yet in the other patient the organs themselves have so much degenerated and lost their power that they are not equal to the emergency and to the sudden calls upon them; hence liability to sudden death, if exposed to certain accidental circumstances, will be much greater in one than in the other. Therefore, there seems good reason for thinking that a patient with dilated left heart, and especially if of middle or of advanced age, when, as is usually the case, there are so frequently concomitant degenerative changes in other organs, has a tendency to die suddenly.

We would here call attention to the fact that the female patient under the care of Dr. Wilks took ten drops of tincture of digitalis every six hours for five days. On the 19th it was discontinued, and on the 27th she died, so that there was a seven days' interval between the last day of taking the digitalis and the day of death.

Now, when we call to mind how small the dose and the seven days' interval, it appears to us that it would be very difficult to say that the digitalis had anything to do with her sudden death, but rather to conclude that it was like the case that we have before mentioned, due to some unknown momentary impulse, which brought about death from syncope.

There is another interesting fact connected with one of these cases that it is necessary to bear in mind, and that is, one that Dr. Hilton Fagge called attention to at the time when the heart of the male patient under Dr. Wilks' care was being opened, that the muscles of the left ventricle were not contracted, but flaccid.

Now, bearing all these things in mind, it appears to us, that there certainly is no proof that the sudden death was in any way due to the digitalis; and when we consider that there are facts which tend to show that patients with this form of cardiac disease are liable to die suddenly; and moreover, as Dr. Gull has observed, this remedy is given with considerable relief and benefit to many patients with dilated, feeble hearts; it is probably right to conclude, with Dr. Gull, that, in such cases digitalis may be given, and probably the patient will derive great benefit from it; but it must be cautiously given and carefully watched; and further, that in cases where death is threatened, as in the case referred to by Dr. Wilks, by the extreme feebleness of the heart itself, and where there are symptoms of cardiac "asthma," digitalis appears to be a very valuable remedy. And although the cases we have brought forward may seem at first glance to be unfavourable to the view that digitalis acts as a tonic to a feeble heart, yet we venture to say that a careful consideration of the whole subject, from more cases, and taking into view the whole of the circumstances, will show that digitalis can be given, not only with safety but with advantage in such cases. Such patients are liable to die suddenly, and will do so now and then whether digitalis be given or not. That this remedy does good, for a time, at least, there can, we submit, be no doubt.—*Medical Times and Gazette, July* 29, 1865, *p.* 114.

24.—ON THE ACTION OF DIGITALIS IN DISEASE OF THE HEART.

By Dr. William Murray.

Dr. Murray having carefully observed from twenty to thirty cases of cardiac disease treated by digitalis, arrives, in reference to the action of this drug, at certain conclusions which are in accordance with the theory lately advanced by Dr. Anstie as to stimulants and narcotics, namely, that all true stimulants tend to bring tissue action to its normal standard; that when such action is in excess, a true stimulant will reduce it, and when below par will raise it to its proper level. Dr. Murray believes that the action of digitalis, when properly investigated, will confirm these views; and he proceeds to show, "1st, that digitalis will stimulate and strengthen a weak heart, and that the weaker the muscular tissue of the heart, the safer will be the administration of the medicine; 2nd, that in a hypertrophied heart it will fail to reduce the pulse either in frequency or strength, and in such cases will prove dangerous; and 3rd, that in a weak organ, acting, because of its weakness, with great rapidity, it will reduce the number of its contractions, and, as it were, strengthen or tone them down. To strengthen and quicken the action of a weak, slowly acting heart, and to reduce the number of the rapid strokes of a feeble heart, is, according to Anstie, to do the work of a true stimulant, bringing action up to the normal standard on the one hand, and reducing it to that level on the other."

In illustration of these views, Dr. Murray adduces the results of his experience, and he remarks that, in many cases, digitalis has removed irregularity and intermission of the pulse, has given power to the heart when exceedingly weak, and increased the rapidity of its action. In one case a lady, aged fifty, had suffered for nine years with disease of both aortic and mitral valves, and the circulation was so weak that there was no pulse at the wrist, while dropsy of the legs and abdomen was making fast progress. Under the use of digitalis in doses of ten drops and upwards, the heart began to beat more forcibly, and the pulse returned at the wrist, the dropsy began to disappear, and the patient returned to her usual state of health, which, though not strong, was comparatively good. But she died about a year after this improvement. Dr. Murray has alternated the use of digitalis with the strongest stimulants, and has been astonished at the superiority it possesses as a stimulant. He thinks that there is not a single disease or condition of the heart *attended by weakness* in which digitalis may not be given with safety and benefit. In hypertrophy of the heart, Dr. Murray admits that his experience is not large, but in one uncomplicated case of this condition of the left ventricle without valvular disease, in which the heart's action was rapid, and at the same time forcible, the digitalis failed to reduce this action, and even caused an increase of the symptoms, and the patient almost fainted after trying it for a week. Lastly, as a cardiac stimulant, digitalis reduces too great rapidity of action when this depends on weakness. Thus it appears that the physiological action of digitalis is that of a stimulant, in Dr. Anstie's sense of the term; and that in its therapeutic action it is especially useful in cardiac weakness, whether that weakness be accompanied by extremely slow or extremely rapid action. As a diuretic, Dr. Murray considers digitalis to be at once the safest and best that we possess.—*British and Foreign Medico-Chirurgical Review, July*, 1865, *p.* 249.

25.—ON DIGITALIS AS A CARDIAC TONIC.

By Dr. Walter James Henry, Belleville, Canada West.

[The following case illustrates the value of this medicine in heart disease.]

A thin, pale, delicate-looking woman was propped up by pillows in a rocking chair. She was sitting with the shoulders elevated, stooping forwards

The face had a most anxious, weary expression, the eyes slightly prominent. There was a slight lividity of the lips and a change of colour in the finger-nails. Respiration was laboured and uneasy, all the accessory muscles appearing in action. She could only speak in a whispered, broken manner, with a gasp between each word. From the long continued distress in breathing, the fatigue was so great that her head would occasionally fall forward on her chest in an uneasy sleep, from which some increase in the dyspnœa would waken her.

The hands were cold and covered with clammy perspiration. The pulse was almost imperceptible in the right side, weak in the extreme when felt, and at the same time extremely rapid. It was ascertained to beat 135 in the minute, and was irregular. It could not be felt on the left side for some time. The legs were enormously swelled, and pitted deeply on pressure. Over the dorsum of the foot and at the instep deep livid stains appeared on the skin.

The great dyspnœa prevented a very accurate examination of the chest being made, but the following facts were made out. The left side of the chest was more prominent than the right, and this prominence was greatest in the cardiac region. The heart appeared to beat lower down than usual, and its impulse was noticed over a more extended surface. Palpation detected valvular thrill and peculiar heaving impulse. A loud systolic bruit was very plainly heard, and was loudest near the apex. It was noticed as remarkable at the time that such a disproportionate impulse existed with a weak and irregular pulse.

The physical signs pointed to disease of the mitral valve with regurgitation, and consequent dilated hypertrophy of the left ventricle.

The kidneys had not been acting very freely, but the urine was free from albumen. The bowels had been acted on by medicine. The tongue was slightly coated, but of a deeper colour than natural, and inclining to purple.

There was not much pain complained of. It was chiefly muscular, and referred to the back and shoulders. The dyspnœa appeared to engross her thoughts, and take up all her attention. In fact, her friends were hourly looking forward to her death, as the breathing had become more laboured towards evening.

At my suggestion twenty drops of the tincture of digitalis with the same quantity of the tincture of muriate of iron were given every hour. I sat up with her to superintend the treatment during the night. Beef-tea and brandy were given every half-hour in small quantities, as some dysphagia existed.

After four doses of the digitalis had been given, there appeared to be intermissions in the very feeble pulse. The breathing became less rapid, and an attempt at a sigh was now and then made. But she complained of the hands becoming extremely cold, and looked even more exhausted than before. Recognising the effect of the digitalis, it was suspended for two hours, during which brandy was very freely given, and the tincture of the muriate of iron was administered as before. The digitalis was then resumed, and given steadily till morning with the food and stimulant.

At eight o'clock in the morning a very decided change for the better was noticed. She could sit up straight in the chair instead of crouching forward. Her breathing was much relieved, and the shoulders did not move as before. The pulse had fallen to 120, and every beat was marked and perceptible. It was now felt without difficulty in the left arm. The eyes appeared to have a brighter expression, and the lips were less livid.

It appeared on looking at the bottles that four drachms of the tincture of digitalis had been given during the night, and a little more than that quantity of the tincture of muriate of iron. As only ten doses of the digitalis had been given, the twenty drops in each dose must have contained twenty-four minims.

The dose of digitalis was now given only every three hours during the day (19th), still combined with the muriated tincture. Eggs and milk were now given to relieve the monotony of the beef-tea. In the evening she was found resting her head on the pillow with a stronger and slower pulse. Each dose

of digitalis produced a feeling of drowsiness. A number of fine punctures were made in both legs with a small needle, and warm flannels applied. A quantity of fluid drained away from these punctures during the night.

20th. Has slept comfortably in her chair. Pulse 110; regular and without intermission. The digitalis was now given only every six hours, and the muriated tincture of iron discontinued.

The strength now seemed sufficiently restored to admit of purgatives, and two drops of croton oil were given and followed up by several doses of bitartrate of potash.

22nd. Swelling very much reduced in the extremities. Breathing very much easier. Has slept well during the night. The dose of digitalis was now reduced to ten drops.

23rd. Slept in her bed last night very comfortably. The bitartrate of potash is still kept up every three hours, and is acting well on the kidneys. There is a still greater diminution in the swelling of the legs.

It is unnecessary to chronicle the further progress of the case. A febrile attack was subsequently relieved, and from that date convalescence was steady. On the 5th of August she had been walking in the open air.

The change which a few hours' treatment made in the pulse was remarkable. From being weak, extremely rapid and irregular, it became defined, full, and increased in strength and regularity in twenty-four hours. The greatest effect, however, was upon the respiration—and this may be accounted for by the digitalis reducing indirectly the congestion of the lung on which the dyspnœa depended.—*Canada Medical Journal, Sept.*, 1865, *p.* 98.

DISEASES OF THE ORGANS OF RESPIRATION.

26.—ON THE INHALATION OF ATOMIZED FLUIDS.

By Dr. Beigel, Member of the Imperial Academy for Natural Sciences, and of the Society of Physicians, Paris.

In the *Lancet* of June the 24th, Mr. Horace Swete suggested "that the little tubes now seen in every druggist's window for blowing out a fine cloud of scent, &c., might be utilized for the application of solutions, astringents, &c., to the larnyx, or, indeed, the eye. . . . . This simple instrument," continues Mr. Swete, "seems to me quite as effectual as the elaborate and costly one figured in Dr. M'Kenzie's book as 'Lewin's Pulverizateur.'" This suggestion has long been executed, for the same Dr. Lewin has, in his works on Inhalation, described and figured, besides his large apparatus, a smaller one, which he carries about in his pocket. It consists of two tubes, which by means of a hinge are folded, and when wanted for use are placed at a right angle to each other, so that the fluid which is to be atomized, and in which the vertical little tube is dipped, whilst air is blown through the other, issues forth according to the principle of insufflation already described.

The apparatus now mentioned is, for private practice, of the greatest utility, and perfectly answers its purpose. But in every well-managed hospital, and particularly in hospitals destined for the reception of patients suffering from diseases of the organs of respiration, an inhalation-room should not be missing, otherwise such an institution will want those appliances which have proved of the utmost benefit in the cure of such patients. Where a small steam apparatus is already present the arrangement can be effected with great ease; but also in cases where an apparatus for compression of air must be procured, the inconsiderable expense would be in no proportion to the benefit derived by it. The principle applied by Windle of Berlin could form the basis for such an arrangement. From the kettle in which the air is compressed a tube would project, running round the walls of the inhalation-room at an appropriate height; and from which branches or arms, with a very fine opening and a stop-cock, must reach into the room. The vessel with the fluid to be atomized can be fixed in a ring from above. This vessel must

be made of thick glass, and have the form of a reversed bottle; the lower end of which (corresponding with the neck of the bottle) has a fine opening, placed at a right angle with the opening of the arm, as soon as the vessel (which can be screwed up and down) is put into the ring. By this arrangement the number of patients could be as many as there are arms, and each might inhale a different fluid. I repeat that this is an arrangement which should not be omitted in any well-managed hospital or dispensary. If we consider that by far the greater part of all patients are (so-called) sufferers from diseases of the chest, and that the inhalations in a great number of those diseases are of much greater effect than internal remedies; if we consider that the atomized fluids, according to the statements of renowned authors, are also very serviceable in other diseases—as, for instance, in certain diseases of the eye, and probably also in affections of the ear, &c.,—the force of the above-expressed opinion becomes still more evident.

The inhalations form a real specific in certain cases of hæmoptysis, in cough which is a result of eccentric irritation of the larynx or trachea, in hoarseness and aphonia as consequences of acute or chronic inflammation of the mucous membrane of the larynx. The cure is sometimes effected with amazing rapidity, after many other medicaments have been applied without effect. Of many cases which I have observed, and the publishing of which in full I reserve, I shall here mention a few only:—

Case 1.—A. B., an unmarried lady, consulted me at the end of last year for hoarseness, from which she had suffered for several years, and which was the more unpleasant to her as she sang well, and a great deal, previous to that affection—a pleasure the deprivation of which gave herself and her friends great concern. The voice was coarse and without *timbre;* the larynx was painful only at the beginning; now it is indifferent to external pressure. Laryngoscopy can easily be effected, and shows only a slight unnatural redness of the mucous membrane of the larynx and of the vocal ligaments. No other inconvenience exists. In course of the affection a great many medicaments were tried, and all without any effect. I applied inhalations of alum (ten grains to the ounce of water). After three applications a striking improvement was observed, which after five was so complete that I discharged the patient, advising her not to sing yet; but after a week she could no longer resist; therefore resumed singing, and sang as before the affection.

Case 2.—C. D., a merchant from Lima, advised by his physicians, left that country and came to England. His suffering consisted in a severe attack of a very troublesome cough, which came on every eight or ten days, and lasted for a day or two, and then ceased. During that time the sputa were tinged with a good quantity of pure blood. After each attack the patient felt very exhausted. When he came under my observation he was very pale and emaciated; his voice was coarse. The result of physical examination was infiltration of the left apex; otherwise the conditions were normal. Three days after the examination he had a severe attack; he coughed frequently and very severely, and was not ten minutes without coughing; the sputa consisted more of blood than of mucus, and were very copious. The quantity of blood he ejected during the day was about two teacupfuls. I ordered immediately an inhalation of tincture of sesquichloride of iron. The cough did not decrease; but the sputa after the first inhalation were not tinged. The patient inhaled twice a day, and had altogether thirty inhalations. The intervals between the attacks were in the mean time much prolonged. Blood never appeared during the cough, which altogether disappeared when extract of hyoscyamus was substituted for the above-mentioned liquor. The appearance of the patient had very much improved, and after six months' stay in this country he again returned to Lima, whence he has repeatedly written, assuring me of his perfect health.

Case 3.—E. F., a vocalist, had caught a severe cold, in consequence of which he was very often attacked with considerable pain in his throat, of a choking character; he felt besides a burning sensation in the larynx, and his

face was covered with perspiration. Each attack lasted about ten minutes, when they disappeared, and returned several times in the course of the day. There was no typical appearance to be observed. The larynx was indifferent to external pressure. The result of laryngoscopy was negative. Blistering, internal application of the acetate of morphia, cannabis indica, opium, and iron were had recourse to, but without any effect. I applied the acetate of morphia by means of inhalation (half a grain to an ounce of distilled water), and the effect was a perfect cure after ten applications.

In conclusion I shall proceed to make a few remarks *on the medicaments I used for inhalations*. Generally speaking every chemical body which is soluble can be atomized, and therefore inhaled. The largest number of remedies contained in the Materia Medica, can, therefore, be used for the local therapy of the respiratory organs. But it must be borne in mind that besides the local effect, the medicaments are much more readily absorbed through the mucous membrane than they are by internal application—a fact which must be taken into consideration when the dose is decided on.

The following medicaments are those mostly recommended and found beneficial by practitioners engaged in treatment by inhalation, and which I can recommend from my own experience:—

1. In inhalatory treatment of the respiratory organs, nitrate of silver deservedly occupies the first place: its dose is three to five or ten grains in one ounce of distilled water. It is particularly serviceable in inflammatory conditions of the pharynx and larynx. The strength of the solution, the frequency of the sitting, and the duration of the same, must be adapted to the nature of the particular case. It need scarcely be mentioned that proper care must be taken if strong solutions are inhaled.

2. Much milder in its effects is nitrate of aluminium, which, as far as I am aware, was first used by myself in inhalations. I prepared it from a simple solution of the metal in nitric acid: working the crystals in distilled water repeatedly, condensing the solution by evaporation, and recrystallizing. It rendered great service, not only in inflammation, but also in nervous affections of the larynx and trachea. The dose is three grains in an ounce of distilled water.

Very useful medicaments are the following:—

3. Tannin: three grains to eight or ten grains, in an ounce of water.

4. Alum: four grains to ten or fifteen grains, ditto.

5. Solution of sesquichloride of iron: one minim to five or ten minims, ditto.

6. Corrosive muriate of mercury: four grains to one or two, ditto.

7. Acetate of lead: a grain and a half to eight grains, ditto.

8. Sulphate of zinc: half a grain to five grains, ditto.

9. Common salt, which has long been considered a most useful agent in the treatment of diseases of the organs of respiration. On the supposition that it was present in the atmosphere near the sea and saline springs, physicians have been in the habit of sending thither patients affected with chest diseases; and to give the full benefit of it to those who were not able to travel, it had always been the object of physicians to create an artificial *sea-air*. But it is only since the invention of the inhalation apparatus that this desire could be fulfilled, The application of common salt for the purposes of inhalation is therefore very extensive, and produces very marked effects. I make use of it in doses of from five to ten or twenty grains to an ounce of water; and can assure its efficacy, particularly in diseases of the lungs and windpipe.

In nervous affections, particularly of the larynx, and also for asthma, narcotics have been used; especially—

10. Tincture of opium, one to ten minims in an ounce of water; and the preparations of opium.

11. The salts of iodine, bromine, chlorine, and some others. Authors report the good effects of arsenic in the shape of Fowler's solution, and in a dose of half to five minims to an ounce of water. Lastly, besides the different mineral waters, there must be mentioned—

12. Pure or distilled water, cold or warm, or even as hot as the patient can bear it. It renders, in many cases of inflammation and paralysis of parts of the larynx, great service.

On inhalations of different gases, which likewise are most effectual remedies, if applied to the proper cases, I intend to treat separately at a future time.—*Lancet, July* 29, 1865, *p.* 114.

27.—A SUGGESTION IN AID OF LARYNGOSCOPY.

By Dr. George Johnson, Physician to King's College Hospital, Professor of Medicine in King's College.

[There is generally a painful glare of light in the eyes of the laryngoscopist.]

Everyone who has worked much with the laryngoscope must have experienced the unpleasant effect resulting from continued exposure of the eyes to the glare of light. So long as the operator looks through a perforated reflector placed in front of one eye there is no mode of escape from the annoyance in question. It may be avoided entirely by the simple device of a black shade over the eyes, the shade being placed beneath the reflector on the forehead, and fastened by two hooks and eyes to the band which passes round the head. The shade is made of a piece of card covered with black silk. The eyes are sewn on the elastic band which holds the reflector, that on the right being fixed on the *inner* side of the buckle, so as not to interfere with the movement of the band through the buckle. The hooks—such as ladies use for holding up their dresses—are attached to the upper edge of the shade, one near each end. The shade therefore can be quickly attached to the band, and as readily removed.

The effect of the shade is to protect the eyes entirely from the direct glare of the sun or of the lamp, while the light is reflected from the laryngeal image, with much less of fatigue and annoyance to the eyes than in the ordinary mode of practising laryngoscopy. When a lamp is used, its flame should be about the level of the top of the patient's head. The shade must be sufficiently wide to protect the eyes from the direct glare of the lamp or of the sun, without obstructing the rays reflected from the laryngeal mirror. The shade can be made and fixed without the help of an instrument-maker, and I commend this simple contrivance to the notice of my fellow-workers in the interesting field of laryngoscopy.

I see that a recent writer in the *Lancet*, Dr. Eben. Watson of Glasgow, expresses his opinion that the value of the laryngoscope has been over-estimated by some whom he considers too zealous advocates of this new method. With regard to the alleged frequency of polypous and other morbid growths in the larynx, he appears to be sceptical, and "cannot repress the wish that laryngoscopists had these strange visions granted to them a little less frequently." Dr. Eben. Watson apparently suspects that some of these visions have been of a *spectral* character. He also expresses his opinion that "laryngoscopy is neither required for nor assists in the ordinary topical medication of the larynx, and that it is a most unsafe guide in surgical operations on that organ." The removal of warts from the vocal cords by the aid of the mirror he speaks of as "rather a wonderful feat in surgery," and he evidently doubts the possibility of such a proceeding. I should have thought that the number of well-authenticated cases in which morbid growths in the larynx have been discovered and removed by the aid of the laryngoscope, would suffice to convince even the most sceptical that growths in this situation are not uncommon, and that their removal by the aid of the mirror presents no insuperable difficulty. I have myself within the last two months removed a tumour from one of the vocal cords in no less than three cases. I have the morbid growths to show as evidence that "this rather wonderful feat" has been accomplished, and I shall shortly publish full particulars of the cases. In not one of these cases could the cause of the hoarseness have

been ascertained without the aid of the laryngoscope, much less would it have been possible to carry out a successful plan of treatment, resulting, as it has done in each case, in the complete restoration of the voice.—*Lancet, Aug.* 26, 1865, *p.* 230.

DISEASES OF THE ORGANS OF DIGESTION.

28.—ON FLATULENCE AND ITS SUCCESSFUL TREATMENT BY A NOVEL USE OF CHARCOAL.

By Dr. Arthur Leared, M. R. I. A.: Senior Physician to the Great Northern Hospital, and Physician to the Royal Infirmary for Diseases of the Chest.

Charcoal is not unfrequently prescribed in cases of flatulence, but the conditions necessary for success are not fulfilled. These are, that it shall be taken into the stomach dry and in a fresh state.

Some twenty-five years ago Dr. Belloc, a retired French officer, turned his attention to charcoal as a remedy for dyspepsia. He was himself a sufferer, and had thus an opportunity of personally testing its action. He decided after many trials that charcoal made from poplar wood is the best. He found that the charcoal of some other woods caused a disagreeable taste in his mouth—even painful excoriations of the mucous membrane—thirst, and a pinching sensation at the pit of the stomach. He was in the habit of prescribing from two to six tea-spoonfuls, but says he has himself taken as much as 500 grammes—more than fifteen ounces in a day. The Académie Nationale de Médecine reported very favourably on a memoir Dr. Belloc published on the use of charcoal, and in France it soon became a favourite remedy. Its reputation spread to this country, and now we find charcoal lozenges, and even charcoal biscuits, sold as dyspeptic remedies. Belloc's charcoal is in the form of a moist powder. He advised that it should be taken stirred up in water. In this country charcoal is occasionally prescribed in mixtures. The biscuits and lozenges must of course be thoroughly impregnated with saliva to enable them to be swallowed. Now, charcoal taken in any of the above modes always seemed to me of little efficacy. My device was to give the ordinary wood charcoal freshly prepared and hermetically sealed in gelatine capsules. Nothing could have been happier than their effect in the case I have referred to. Three or four capsules were sufficient to give complete relief each time. Nor is it often that the objective evidence of beneficial action is so plain. When the sound on percussion over the greater end of the stomach was quite tympanitic, this would completely subside into the ordinary clear tone of the part after the capsules had been swallowed a few minutes.

Encouraged by this case, the capsules were given in many other instances with excellent results. A series of experiments were now commenced with a view to determine what kind of charcoal was best for the purpose, and whether the matter admitted of further improvement.

The power of absorbing effluviæ possessed by charcoal had been known for ages, but its action upon different gases was first tested by M. Lowitz towards the close of the last century. The subject was pursued by Count Morozzo and afterwards more fully by M. Saussure. He used boxwood charcoal exclusively in his experiments, and found that a single volume of it absorbed the gases which possess a present interest in the following proportions:—

Sulphuretted hydrogen	55
Carbonic acid	35
Oxygen	9·2
Nitrogen	7·5
Carburetted hydrogen	5
Hydrogen	1·7

The able researches of Dr. Stenhouse, published in 1855, greatly increased the interest in the subject. His experiments were mainly directed to the

deodorizing qualities of charcoal, and he proved beyond question that its properties of destroying the smell of putrid animal or vegetable bodies depended, not, as had been previously supposed, on its antiseptic power, but on the very opposite quality of absorbing and oxidising effluviæ. Several others have since that time investigated this absorbent action, and Mr. Hunter has recently published an account of his experiments, showing the relative amount of various gases taken up by different kinds of charcoal.

He found that charcoal made from cocoa-nut shells possessed most power, and deduced a general rule that charcoal made from the harder woods was much superior to that from the softer. It may be added here, that animal charcoal, so useful as a decolorising agent, is very inferior as an absorbent.

In my own experiments the gases were collected by displacement in the usual way. By a simple arrangement, the amount of absorption was determined by the quantity of mercury which flowed into, and was retained in the vessel containing the gas, into which the charcoal had been introduced. As the experiments have had a practical aim, they differ in some respects from those conducted by the purely scientific chemists. The gases were not dried, since in the moist state they more closely resemble those of the alimentary cavities, and combinations of other substances with charcoal were tried in the hope of discovering a still more effective absorbent than simple charcoal. The experiments have been very numerous, but it has not been thought desirable to encumber the table with more than seem necessary to elucidate particular points; and as the absorption of carbonic acid possesses by far the most interest, this has alone been given. The absorbent action was instantaneous, and about seven-eighths of the whole was completed within two minutes. The time allowed for each experiment was an hour, but absorption continues at a slow rate for many hours. To obtain good results the charcoal must be exposed to a high heat in a crucible, until gas no longer escapes: and it would seem that charcoal newly made is superior to that which has been re-ignited.

From the table given below the following deductions may be made:—Charcoal made from the most solid vegetable substances is greatly superior as a gas absorbent to that made from the lighter kinds, like Belloc's preparation; and that made from vegetable ivory is the best.

The absorbent power of charcoal is slightly weakened by pulverization, is much impaired by exposure to the atmosphere and damp, or by soaking in water, and is altogether impeded by being covered by water.

Table showing the Absorption of Carbonic Acid Gas by Twenty Grains of Different Kinds of Vegetable Charcoal.

	Experiment.	Mean.
Vegetable ivory nut in small fragments, freshly ignited ..	2·26 2·26 2·20	2·25
Vegetable ivory, finely powdered and freshly ignited.. ..	2·20 2·20 2·12	2·17
Vegetable ivory freshly ignited and ammoniated	2· 1·19 1·94	1·95
Vegetable ivory, platonized (2 per cent.)	1·74	1·74
Vegetable ivory, freshly ignited soaked in water	1·36 1·32 1·24	1·30
Vegetable ivory, freshly ignited, covered with a layer of water,	0·0	0·0
Coquilla nut, freshly ignited	2·0 1·94 2·4	1·99
Cocoa-nut shell, freshly ignited..	2·0 1·98 1·92	1·97

	Experiment.	Mean
Acacia, freshly ignited	1·80 1·88 1·94	1·87
Peat, freshly ignited	1·80 1·66 1·82	1·76
Belloc's freshly ignited	1·82 1·80 1·70	1·77
Belloc's as sold, without ignition..	·68 ·66 ·66	·67

Charcoal, to be effective against gastric flatulence, must be introduced amongst the gases of the stomach in the same state as when fresh from the crucible. The means for effecting this, enclosing it in gelatine capsules, so that it may be set free in the stomach by solution of the gelatine, has been already described.

It may also be inferred that, as the absorbent action is protracted and is not destroyed by being wetted, charcoal may still prove beneficial when it has passed into the intestines.

Charcoal, by virtue of its porous nature, takes up gases mechanically as a sponge takes up water. Its capacity in this respect is in proportion to the number and fineness of its pores. Charcoal made from vegetable ivory or cocoa-nut shell is a compact, heavy substance, having a metallic lustre and ring, the pores being quite invisible.

This kind of charcoal, then, is best adapted to the present purpose. It should, in the first instance, be carefully prepared by sufficient ignition, and when about to be filled into the capsules should be powdered and re-ignited in a crucible. I have as yet no clinical experience of the ammoniated charcoal; but when the alkali is indicated it will probably be found the best. Charcoal impregnated with chloride of platina has been recommended as an absorbent by Dr. Stenhouse, but it did not answer my expectations.

An experiment conducted for me by Mr. Robbins, to whom I am indebted for much valuable aid, shows how necessary it is that good unsaturated charcoal should be used.

An iron bottle, to which a long tube of the same material was adapted, was filled with twelve ounces of ordinary medicinal charcoal and placed on a strong fire. A glass tube, tapered at the free end to a very small size, was adopted by means of an india-rubber tubing to the free end of the iron tube. As soon as the charcoal ignited, gas began to issue from the glass tube and continue to do so for about two hours. It could be ignited, showing the presence of carburetted and other inflammable gases. It is possible that the excess of gas was partly due to the charcoal not having been well made, and it certainly shows the necessity for caution in selecting the article for use.

Charcoal always contains oxygen, because, when taken from the crucible, it immediately obtains it from the atmosphere, and its affinity for nitrogen is much less. It may here be remarked that this easy separation of its compotent parts goes to prove that the combination of oxygen and hydrogen in common air is purely mechanical.

Charcoal has a different point of saturation for every gas, but when filled with one or more it is still capable of absorbing other gases. Thus, coarse-grained charcoal saturated with ammonia takes up more carbonic acid than charcoal without ammonia; and the same charcoal when saturated with both takes up a larger quantity of sulphuretted hydrogen than of either. This is a valuable property where, as in the case of the intestinal cavity, the gases are various. Much is still to be learned about it; but the reason why charcoal saturated by ammonia takes up carbonic acid so freely is plainly due to the formation of carbonate of ammonia. The impregnation of char-

coal with oxygen has important therapeutic actions. It is this which renders is so valuable an agent in destroying the fetor of foul sores, and when taken internally, of correcting any putrefactive tendencies; and it is also by virtue of this that sulphureted hydrogen is not only absorbed but immediately decomposed.

The gelatine capsules employed are of two kinds—those ordinarily used for taking liquid medicines, and those made in separate portions which fit into each other; in either case care is taken that they be hermetically sealed with gum or liquid gelatine as soon as filled. The limit of size for the capsules must, of course, be determined by the individual capability of swallowing them. The largest contained only five grains of the light charcoal at first used, and found so efficacious in the case mentioned, but the same capsule will contain more than ten grains of the heavy kind; thus a relative as well as an absolute advantage is gained, for the heavy charcoal possesses much higher absorbent powers.

Charcoal taken in large doses presents one disadvantage: intestinal obstruction, as in the case of magnesia, has sometimes been the consequence. Now, the table proves that vegetable ivory charcoal freshly ignited is three times more effective than Belloc's damp charcoal. But this superiority must be still greater in consequence of the mixing with water preparatory to Belloc's charcoal being swallowed; and if it sink in the fluids of the stomach, which, should any be present, it inevitably will do in consequence of the saturation, its efficacy will be altogether destroyed. But granting that charcoal taken in the ordinary way has certain absorbent power, a very large dose will be required to equal the activity of a single capsule of the heavy charcoal.

It may be objected that, although charcoal be swallowed in hermetically sealed capsules, it must necessarily undergo liquid saturation as soon as set free in the stomach. But charcoal, not being easily wetted, will float on the surface of its contents; and to test its capability of absorption when in this position I tried the following experiment:—

Into a receiver containing carbonic acid a little warm water was introduced; a capsule of heavy charcoal was then put into the receiver. The gelatine having been dissolved by the water, its contents were set free in about a minute, and the charcoal floated on the water. Absorption now proceeded actively.

The number of capsules to be taken must vary with the greater or less severity of the flatulence, but three or four will generally be found sufficient. It is not advisable to drink while swallowing them, but the process may be aided by eating a small piece of dry bread.

Twenty grains, or two capsules, of the vegetable ivory charcoal absorb more than two cubic inches of carbonic acid gas, which ought to be sufficient to reduce any ordinary distension of the stomach. If a greater effect be desired, and the dose be increased to four capsules, nearly five cubic inches will be taken up. Now, admitting that these effects are not quite so active in the stomach as they are in the experiments, there must still be absorption enough to give relief to the patient. The effect may at any time be tested and the repetition of the dose guided by percussion over the greater end of the stomach. More charcoal should be taken as long as the percussion sound is tympanitic.

The beneficial action of charcoal need not be confined to the human subject. I have suggested its use in appropriately-sized capsules for flatulence in horses, and also as a remedy for that frequently fatal distension of the first stomach from carbonic acid which occurs in cows after certain food, such as clover and potatoes, and hope soon to hear of their successful application.—*Medical Mirror, June*, 1865, *p.* 315.

29.—ON THE ASSIMILATION OF SUGAR BY THE LIVER.

By Dr. F. W. Pavy, F.R.S., Assistant Physician to Guy's Hospital.

From Brücke's analyses, which have been confirmed by Dr. Bence Jones, it seems that healthy urine contains a trace of sugar. I had premised that this should be so without knowing anything of Brücke's experiments and before Dr. Bence Jones' were made known. Looking at the diffusibility of sugar, and the fact that a minute quantity of it exists always in the circulation, I expressed myself thus in one of my Lettsomian Lectures which appeared in the *Lancet* for December 22, 1860:—"I should say, even, that the trace of sugar, which is natural to the blood throughout the body, is constantly being drawn upon by the kidney; but that the amount for removal is so small that it is not susceptible of detection in the urine. Did we possess more perfect means of detection than we do, I imagine a minute trace of sugar would be reckoned as a normal constituent of the urine."

It is a curious coincidence that Dr. Bence Jones' communication was read at the Chemical Society the very evening the number of the Lancet containing the above was passing through the press. Dr. Bence Jones, writing quite recently (Medical Times and Gazette, January 21, 1865), again states that he has satisfied himself by the most careful investigation that Professor Brücke is right in saying that healthy urine contains sugar.

Now, accepting it as proved that healthy urine contains a trace of sugar, I look upon such a fact as affording one of the strongest arguments that could be brought forward against the validity of the glycogenic theory. It is admitted that no appreciable destruction of sugar takes place in the lungs; therefore, whatever sugar is poured into the circulation from the liver and reaches the right side of the heart will also be found in the arterial system. It must hence be conveyed in due proportion to the kidney, and, on account of its diffusibility, must be in part eliminated with the urine in compliance with the laws of osmosis. Now, if from the minute quantity of sugar naturally existing in the blood, sugar can be shown to exist to a minute extent in the urine, what, it may be asked, would be the condition of the urine if the blood of the right side of the heart were strongly impregnated with sugar as was formerly supposed? I confidently assert that, under such a state of things, we must all have been diabetics. The state of the urine forms an index of the amount of sugar existing in the circulation; and let sugar to any considerable extent reach the general circulation; and it will be sure to determine a correspondingly saccharine condition of the urine.

Dr. Bence Jones, in his recent lectures in the Medical Times and Gazette, treats of diabetes as a disease of suboxidation. He seems to have carefully avoided saying anything about the subject of glycogenesis either one way or the other. After enumerating the different kinds of saccharine substances, and stating that they are not all able to pass as glucose into the urine, he says, "but they are all capable of being oxidised in the blood and textures into carbonic acid and water, whilst giving heat or motion or other energy to the body." Now, if from this we are to infer that Dr. Bence Jones is of opinion that the sugar we consume reaches as such the general circulation and undergoes oxidation as it is contained in the blood and is passing through the textures, I think his corroboration of Brücke's analysis stands directly opposed to him. I maintain that the trace of sugar encountered in healthy urine comes by the simple law of diffusion from the minute quantity of sugar always encountered, no matter what the diet, in healthy blood. The presence of this trace of sugar in healthy urine proves that the capacity for the destruction of sugar in the blood cannot be very great. Now, suppose saccharine food to be ingested, and that the sugar reached the general circulation; it would be carried to the kidneys as well as elsewhere, and what would be the condition of the urine under these circumstances? The ingestion of sugar, unless in very large quantity, does not give rise to the presence of sugar, such as is capable of being recognised by ordinary testing, in the urine; but I contend it would do so if the sugar reached the general cir-

culation. Dr. Bence Jones treats of diabetes as a disease of suboxidation, and speaks about the oxidation of sugar in the system as though such a process had been actually proved to occur. He, however, adduces no evidence, and I must confess I am aware of none, that can be taken as showing that the sugar we consume is disposed of by undergoing direct oxidation in the body.

Indeed, my experiments, to which I will now refer, show that sugar is transformed by, or undergoes its first step of assimilation in, the liver. I have found that from sugar amyloid substance is produced in the liver.

I think it may be looked upon as established that sugar derived from without is transformed by the liver into amyloid substance. It may be considered that we have here the first step in the assimilative process to which sugar is subjected in the animal economy; and should this step fail to be accomplished, the sugar would pass through the liver; and, reaching the general circulation, would occasion diabetes. But I do not say that sugar is the only source of the amyloid substance. It is quite certain that it is not so. There is still amyloid substance in the liver, notwithstanding saccharine and amylaceous materials may have been altogether excluded from the diet. I believe that the liver is capable of forming amyloid substance from the products of the retrograde metamorphosis of assimilated animal food and worn-out tissue. By this, materials would be brought into a condition for being turned to account in the system, instead of being discharged as useless.

I must not omit to refer to a property enjoyed by the amyloid substance which enables it to be retained in the liver cells, so that the gradual assimilative influence of the organ may be exerted upon it. Sugar, as I have already stated, is a substance enjoying great diffusibility. With its diffusibility, we may say it would be physically impossible that it could exist stored up in the liver cells whilst a current of blood was flowing through the capillaries around. In the amyloid substance, however, we have a body directly opposed to sugar in respect of diffusibility. It refuses altogether to pass through the vegetable parchment or thick animal membranes. Hence it has no disposition to escape from the liver cells, and the circulation can take place around without carrying it away. Should it reach the circulation and be mingled with the blood it would be immediately transformed into sugar and occasion saccharine urine.

Having thus shown that sugar is transformed by the liver into amyloid substance, the next step is to determine into what the amyloid substance is converted. Upon this point, I will simply tell you that Dr. McDonnell has recently enunciated the opinion that it is united with nitrogen, and leaves the liver through the hepatic veins as a newly-formed proteic compound, partly, perhaps, he says, as globuline, and partly as a material in its reactions resembling caseine or albumipose. From the philosophical reasoning adopted, Dr. McDonnell's opinion is entitled to consideration, although I must confess my own investigations have led me to look in another direction for the product of the metamorphosis of the hepatic amyloid substance. It is due to Dr. McDonnell, however, to admit that as yet I have obtained no data sufficiently precise to justify me in entertaining anything more than a surmise upon the matter.

Diabetes I look upon as the result of a defective assimilating action of the liver. If the cases that fall under observation be attentively examined, they will be found to present considerable variation. In some a perfect exclusion of saccharine and amylaceous materials from the diet may be made, and still the diabetic shall pass a considerable quantity of sugar. I should say here that the saccharine principle comes from the unnatural transformation into sugar of the amyloid substance formed by the liver from products derived from animal food, and from the wear and tear, or disintegration of the tissues. In others—and this is the kind of case that is especially met with amongst elderly people—sugar disappears from the urine when restriction from saccharine and amylaceous articles of food is practised. Here there seems to be simply a want of power to assimilate the sugar and starch ingested. In a healthy person, when sugar and starch are taken, they are lost sight of in the system, because there is the power to transform them,

so that they may be to the service of the economy. In such a diabetic, however, as I am now picturing to you, whatever sugar and starch may happen to be ingested will pass through the system and be eliminated with the urine, because there is the want of power to assimilate or transform them so that they may be turned to account in the economy. In other cases, again —and these are not so rare as perhaps may be thought for—there is the power to utilise a certain amount of sugar and starch, and this may be taken without occasioning the presence of sugar in the urine, whilst if the certain amount be exceeded sugar is immediately to be recognised in the urine.

I have met with several cases of the last form of diabetes that I have described to you. Such patients have come to me as ordinary diabetics. The diet has been restricted so as to exclude sugar and starch from the articles ingested. The sugar has disappeared from the urine, and after a time it has been found that a certain amount of ordinary bread and potatoes may be taken without rendering the urine saccharine. An excess, however, immediately affords evidence that the complaint has not completely disappeared. In one case—an exceedingly instructive one—the patient regarded himself as completely cured. He was able to take a mixed diet without passing sugar with his urine. All that was required to be attended to was, moderation in the consumption of bread and such-like articles of vegetable food. At an evil moment he was tempted to drink some cyder. Being thirsty, he drank off a pint; and within two hours, from the sweet taste experienced in his mouth, he felt sure his complaint had returned. His urine he soon afterwards brought to me to be examined, and the result showed that his suspicions were well-founded. This patient, although several weeks have now elapsed, has never since been in so favourable a state as before. At first, notwithstanding that he fell back upon the restricted diet, sugar was present in the urine; and now, although there is no sugar under it, yet he cannot deviate from it without passing saccharine urine. It is hence evident how easily a relapse may be brought about by indiscretion in what is taken, where it may be even imagined that a complete cure had been effected.

The point remaining to be attained in the pathology of diabetes is to go one step further than we have done and ascertain the cause of the sugar failing to undergo assimilation as it ought to do. It has been for a long time known that puncture of the fourth ventricle produces a temporary diabetes. This has not, however, helped us to unravel the nature of diabetes, because it cannot be definitely stated in what way the effect is produced. Some years ago I discovered that injuring certain portions of the sympathetic system will likewise produce diabetes. A paper upon this subject appeared in the Guy's Hospital Reports, for 1859. I found that injuring, amongst other parts, the superior cervical ganglion of the sympathetic rapidly produced a strongly saccharine state of the urine. At first each time I experimented upon this ganglion I obtained the diabetic result. Somewhile afterwards, on several times repeating the experiment, I was surprised to find that if I obtained any effect it was but a very slight one; nothing to be compared to what I had obtained before. I have been at a loss to account for this difference until recently, when it occurred to me to use the fumes of puff-ball as an anæsthetic, instead of chloroform, whilst performing the operation.

I was in no doubt about a strongly-marked diabetic effect having been produced in my early experiments. In one case, indeed, I observed as much as 20¼ grains of sugar to the fluid ounce in the urine within half an hour after the operation; and in another 34 grains to the ounce in less than two hours. I was not conscious that I operated differently to what I had done before; but yet there was the difference in the result. Reflecting upon the alteration which the administration of chloroform produces upon the calorific effect resulting from the division of the carotid portion of the sympathetic in the neck, it occurred to me as possible that the administration of chloroform might exercise an influence upon the result of my experiment. I cannot say whether I used chloroform in a manner to produce so full an anæsthetic effect in my early experiments as I do now. At all

events, I have recently substituted for chloroform the smoke of puff-ball as an anæsthetic agent, and have found in several experiments that injuring the superior cervical ganglion has been followed, without exception, by the rapid production of strongly saccharine urine.

The inference first natural to be drawn was that the artificial diabetes was occasioned by an alteration in the state of the blood-vessels connected with the liver. I therefore resolved to ascertain how such an inference would stand when submitted to the test of experiment. I tied the hepatic artery, and still found that saccharine urine resulted after operating on the sympathetic. In one hour's time the urine contained 8½ grains, and in two hours' time 12 grains of sugar to the ounce. After ligature of the cæliac axis, also, sugar has appeared to nearly the same extent in the urine. One hour afterwards there were 7·38 grains, and three hours afterwards 6·66 grains to the ounce. After ligaturing the portal vein as well as the hepatic artery, as was to be expected, there is no appearance of sugar in the urine.

With these experiments I do not see how it is reconcileable that the effect of the operation on the sympathetic can be in a direct manner attributable to an action upon the blood-vessels. It is only upon the coats of the arteries that we can conceive the sympathetic to exert any material direct influence, it being through the muscular element that it influences the circulation. Possibly the operation on the sympathetic may lead to an alteration in the quality of the blood that is passing to the liver, and so occasion the result. This is the most likely supposition that I can offer at present. Tying the portal vein so as to allow a stream of arterial blood only to pass through the liver occasions a saccharine condition of the urine; and injecting carbonate of soda into the circulation previous to operating on the sympathetic prevents the production of saccharine urine. Hence some justification is afforded for looking to the state of blood to supply the explanation required. The effect of the carbonate of soda thus ascertained by experiment in checking the production of saccharine urine after operating on the sympathetic tallies with the known therapeutic influence of the Vichy waters in diabetes.—*Med. Times and Gazette, June* 3, 1865, *p.* 565.

30.—ON A CASE OF TAPEWORM (TÆNIA SOLIUM) TREATED SUCCESSFULLY WITH THE OIL OF MALE FERN.

By Dr. JOHN CONSTABLE, Leuchars, Fifeshire.

[The patient passed two joints of tapeworm owing to the action of a purgative.]

I then ordered the following mixture, which is, as nearly as may be, the formula recommended by Dr. Fleming, of Birmingham, in his Report to the British Medical Association—viz., oil of male fern, three drachms; mucilage, an ounce and a half. Half of the above to be well mixed with half a cupful of sweet milk, and to be taken on an empty stomach at bedtime, to be followed by half an ounce of castor oil in the morning.

These directions were followed to the very letter, as the patient was most anxious to get rid of "these worms," which had caused her so much annoyance, and brought her into such bad health.

On the 21st of December she took no food after breakfast. At six p.m., she felt so very hungry that the temptation to partake of food was almost irresistible. However, as a compromise, she took the mixture as prescribed, and went to bed. In a few minutes she felt very sick and inclined to vomit; and a peculiar benumbed sort of feeling crept over her body, and particularly up the sides of her head. There was, however, no vomiting, nor even retching; but the nausea and sickness continued. In about twenty minutes after taking the medicine she felt a peculiar and remarkable sensation over the region of the stomach—a sudden starting, or, as she expressed it, a sudden and sharp "dirking," which lasted only a second or two. The sickness, however, continued till midnight, when it gradually wore off. There was no griping at all. At six a.m., and before taking the castor oil prescribed,

she felt an inclination to go to stool. A small quantity of fæces came away first, and then a large tapeworm, coiled up in a ball about the size and shape of a hen's egg, and dead. On being uncoiled and measured, it was found to be exactly twenty-two feet two inches and a half long. There was a smaller piece, also, in addition, which was detached from the rest, and measured about six or eight inches. This broke up into several pieces; and the patient, thinking it of no consequence, threw it away. The larger piece, however, was brought to me, and I have preserved it. I have no special knowledge of helminthology, but I think it is a specimen of the tænia solium. The following are its measurements:—As already stated, twenty-two feet two inches and a half in length. The breadth of the first foot is one-eighth of an inch, and contains ninety-five joints; the breadth of the second foot is a quarter of an inch. At the broadest part it measures half an inch, and there are thirty-one joints in a foot; and at the end it is three-eighths of an inch broad, and contains only fifteen joints in a foot. The *head* was not found.

In three days afterwards I ordered the patient to take the second half of the mixture prescribed, with the same directions as formerly. She experienced the same physiological effects of the medicine, but in a more intensified degree. This time there were decided griping pains, and more sickness and nausea, but no vomiting; more purging, pain on micturition, a sick headache, and the same benumbed sensation up the sides of the head. Castor oil was also taken in the morning, and there were copious evacuations, but no more joints were passed.

For some months previously to consulting me, Mrs. B. had been in a very unsatisfactory state of health. She had pain in the stomach after food, and especially after breakfast; dull headache; giddiness; a feeling of coldness over the abdomen, with occasional swelling and griping in the region of the stomach; was generally sleepy and heavy after any exertion, and disinclined to work; there was a constant itching at the nose and the anus, and intensely at the latter when the joints "wriggled" themselves away. The appetite was at times very voracious; she was generally obliged to eat something at five a.m., to satisfy the cravings of hunger. She states that she had no thirst during these months, and that the bowels, formerly costive, assumed latterly an opposite tendency.

On being questioned as to her food, she admits having eaten pork, and remembers distinctly having eaten in the beginning of 1864 a small piece of dried ham *raw*. This, in all probability, contained the cysticercus from which this tapeworm became developed. She further states that for some time she has been particularly fond of *salt* herrings, and that several joints invariably passed after eating them.

The patient's health has improved every day since this tapeworm was passed. She feels as if a burden had been taken off her; her spirits, formerly low, have greatly improved; the capricious appetite has been removed; the dull sick headache has gone; and she says she is "altogether a new creature,"

It is to be hoped that the *head* of the worm was on the slender detached piece which was unfortunately not brought to me, and that a tonic treatment will prevent a return of the patient's distressing symptoms.—*Lancet, May 27, 1865, p. 565.*

31.—ON AZOTURIA.

By Dr. Edward H. Sieveking, Physician to H. R. H. the Prince of Wales, and to St. Mary's Hospital, &c.

[It is impossible for physicians to devote too much care to the investigation of cases of dyspepsia and anæmia. These terms cover a multitude of diseases, which a greater refinement of diagnosis would readily reduce to some other denomination.]

While admitting the imperfection of my analyses, I cannot but think that,

if we find a patient habitually passing urine of a high specific gravity, averaging 1030, and not due to the presence of sugar, nor associated with albumen, and proved to contain a very large amount of urea, we are justified in terming his case one of azoturia; the more so, if on close inquiry, no tangible disease can be detected to which the accompanying symptoms are referrible.

It has been my lot to have met with numerous cases of protracted indisposition, accompanied by defective powers of assimilation, irregular digestion, debility, general and indefinable *malaise* and more or less emaciation, though this not always, in which I held the excessive waste of the nitrogenous tissues, as indicated by the increase of urea in the urine, to be an adequate explanation of all the phenomena. The diagnosis was, in part, attained *par voie d'exclusion;* but after satisfying myself of the absence of all other sources of the morbid feelings of the patient, it appeared that a persistent and palpable feature, such as the one alluded to, would justify the application of Willis's term. Dr. Prout (*On the Nature and Treatment of Stomach Diseases*, 1848, p. 94), who gives a description that in most points tallies with my own observations, distinguishes between two forms of the disease; in one of which there is diuresis; while in the other there is an excess of urea without diuresis; and he regards the affection as analogous to diabetes. He considers that he has the strongest presumptive evidence, both from observation and analogy, that, if permitted to proceed unchecked, or if injudiciously treated, it passes into diabetes or some other formidable disease, though he has no positive proof of such termination.

I have at present under observation the case of a gentleman, in whom there is an alternation of azoturia and glycosuria; though in neither case is there a secretion of urine justifying the term diabetes. The amount passed rarely exceeds three pints in the twenty-four hours; but, while at times the urine is almost solidified by the addition of equal parts of nitric acid, owing to the formation of nitrate of urea, at others it is found to contain a palpable amount of sugar, as confirmed by the examination of my friend, Dr. Matthiessen, the lecturer on Chemistry at St. Mark's Hospital.

I have shown elsewhere, that an excess of urea is, as was already shown by Dr. Prout, a frequent accompaniment of epilepsy—an exhaustive condition which might have been predicted *à priori*, if *à priori* reasoning were of any value in determining natural phenomena. The occurrence, however, is not uniform; in fact, it fails with sufficient frequency to have induced one writer to assert that the characteristic feature of the urine in epilepsy was an absence of urea. Dr. Parkes, in his admirable work on urine, states that he has never met with a case of genuine azoturia, though he admits the possibility of its occurrence, as he quotes a case examined by Dr. Ringer, in which a man who was not febrile, and only appeared feeble, passed no less than 1130 grains of urea in each twenty-four hours, or about three times the average secreted by an adult male. It is of such cases, and not of the coincident occurrence of an excess of urea in other diseases, as in epilepsy, that I am now speaking; and I cannot but think that, were a special series of volumetric analyses undertaken to determine the point, the view originated by Prout would receive more positive support than it yet has, and azoturia would be definitively accepted as a member of the nosological fraternity. It is possible that Golding Bird, who laid what I should regard as an undue stress upon the presence of oxalates in the urine, because they come and go like will-o'-the-wisps, often spoke of cases of azoturia when he discussed his hobby oxaluria. He admits that, in oxalic urine, the density increases with the quantity of urea, which is often present in large excess. Indeed, he continues, "I regard the presence of a greater or less excess of urea almost as characteristic of the morbid state of the urine for which I am contending, as the oxalate of lime itself." (*Urinary Deposits*, 1857, p. 241.) Without controverting Dr. Bird's statement, I may state that in the cases of azoturia as I have observed them, this balance between oxalates and urea has not presented itself; and, in fact, I may say, that the oxalates have but rarely occurred with the excess of the latter substance.

The following is a case to which I should apply the term, azoturia.

A gentleman, aged fifty-three, but looking much older, consulted me in January, 1861. He had always been very temperate, and had enjoyed good health till after an affray with poachers ten years previously, when he had an attack of jaundice. He recovered from this, and remained well till, four years ago, he became subject to bilious attacks accompanied by irregular action of the bowels. Three years ago, after much annoyance and mental excitement, he suffered from carbuncles on his back. Since that time he had been in bad health; suffering from a feeling of numbness in the left leg, and occasional severe attacks of diarrhœa. He complained of occasional neuralgic pains, and at times entire absence of sensation in the left foot. He walked into my room like a man debilitated by disease, but in no way resembling a paralytic. He complained of having been affected with seminal emissions. I examined him very carefully afterwards in bed, and could find no trace of loss of parallelism or loss of motor power. I could not satisfy myself, by the æthesiometer, of any material impairment of tactile sensibility; no tenderness of the spine and no abdominal derangement were traceable; nor was there any disease of the rectum or prostate. I commenced the treatment with steel, and the application of an anodyne liniment; and ordered wine, beer, and nutritious food. The patient, being a teetotaller, was with great difficulty persuaded to take any fermented liquors. I obtained a specimen of the morning and evening urine before my next interview, when I had a consultation with a leading physician on the case. Both specimens had a specific gravity of 1035, were strongly acid, exhibited no increase of phosphates, and contained neither sugar nor albumen. There were no crystalline forms in either, and specifically no oxalates: but both specimens almost solidified on the addition of equal volumes of nitric acid, by the formation of nitrate of urea. The joint examination elicited no proof of any organic disease; and it was agreed that the case was one of azoturia. The quantity of urine never was large that was passed in the twenty-four hours; it rarely attained to three pints. I find that on one occasion I had the advantage of obtaining a volumetric analysis from Dr. Ringer; and the estimate was seven drachms and a half in the twenty-four hours—an amount considerably above what Dr. Parkes considers the average in the healthy adult. The gentleman remained under my treatment for three weeks; and improved materially under the administration of mineral acids, opiates, and tonics of various kinds. It was not till after twelve days' observation that I discovered a few oxalates in his urine. In a medical report which I gave the patient at his request, on leaving town, I stated that I regarded the azoturia as the source of debility; and that the drain caused by a persistent waste of tissue, indicated by an excess of urea in the urine, sufficiently accounted for the anomalous nerve-symptoms to which he was liable.

I saw the same gentleman again at the end of 1862, when he was passing through London after he had, without medical advice, been taking the waters of Kissingen, and thereby brought on severe diarrhœa. He was naturally weak in consequence; but the urine presented a specific gravity of 1025—lower than I had ever seen it formerly, and I have a memorandum that it contained but little urea.

There are many insidious morbid conditions for which we may find an adequate explanation in the urinary secretion, with important hints as to treatment and *régime*. The insidiousness of degenerative renal disease as indicated by albumen, is a point that can scarcely be too often mooted; and it is surprising how often and how long patients continue ailing and complaining of feebleness and want of power without presenting any very tangible symptoms, when an examination of the urine reveals the cause of the anæmia, the weakness, and the apnœa, and at once suggests the proper indication. Although albuminria is a much more frequent cause of these insidious symptoms than azoturia, still I am satisfied that there are numerous cases of chronic disease going through a weary life, for which the undue waste of the nitrogenous tissues, indicated by a high specific gravity of the urine and an excessive proportion of its urea, affords a satisfactory explanation.—*British Med. Journal, June* 3, 1865, *p.* 557.

DISEASES OF THE URINARY ORGAN.

32.—ON THE PATHOLOGY OF DIABETES.

By Dr. GEORGE HARLEY, F. R. S., Professor in University College, and of University College Hospital.

[It is well known that cerebral lesions, especially when in the neighbourhood of the fourth ventricle, are not unfrequently accompanied with saccharine urine. Epilepsy is also occasionally accompanied by diabetes.]

Over-mental exertion is among the causes of saccharine urine. An interesting example of this kind has recently fallen under my notice in the case of one of our students. During three weeks of mental excitement, sugar was daily detected in his urine. After a fortnight's rest, it entirely disappeared, but recurred a few days after the resumption of the mental labour, again to disappear almost as soon as the labour was discontinued. I have seen again and again mental labour increase the sugar in diabetic patients. In the case of a clergyman, to which I shall subsequently have occasion to refer, on several occasions I found the daily amount of sugar double itself when he was preparing for his College examinations, and again gradually return to the usual average almost as soon as they were passed, without either medicine or diet being altered.

Irritation of the pneumogastric or of the sympathetic nerves may give rise to diabetes.

Mr. Nyman relates the case of a professional gentleman who had long suffered from diabetes mellitus. "After we had examined," he says, "the above-named organs (chest, abdomen, and brain), we commenced the dissection of the nervi vagi. On the left nothing abnormal could be found, but on examining the right, we met in the thorax immediately behind the bifurcation of the bronchii a calculous mass (tumour) the size of a hazel-nut, under the entire trunk of the nerve, which appeared to have exercised considerable pressure upon it. He adds that it is the third time that he has met with this morbid condition in persons who have died from diabetes. The urine of this patient contained 6 or 7 per cent. of sugar.

One case is also recorded in which disease of the sympathetic caused diabetes. In a report of the cases occurring in the Royal Infirmary, Edinburgh, under the care of Dr. Duncan, it is noticed that in one case of diabetes there was found, after death, to be an hypertrophy of the lumbar sympathetic.

Saccharine urine may also arise as the result of indirect nerve irritation; thus it is met with in tumours of the abdominal organs and in pregnancy, in which cases the only way to account for it appears to be by the irritation caused by pressure or otherwise to the abdominal sympathetic, perhaps the cœliac plexus or some of its branches.

For further information regarding the effects of nerve lesions and irritation in the production of diabetes, reference must be made to the physiological views previously expressed.

We now come to the consideration of disorders of the digestive function as a cause of diabetes. And, first of all, we must notice those cases which arise apparently from the use of stimulants. It is a well-known fact that cases of diabetes are much more common in Great Britain than on the continent, and I, like many others, attribute this to the fact of a much greater quantity of alcoholic drinks being used among us than among European nations. A few years ago (August, 1861), while on a visit to Munich, in the course of conversation, Dr. Pfeufer, the Professor of Clinical Medicine, told me that he had only had one case of diabetes in the Hospital during the whole six years he had been in Munich; whereas, while Professor in Heidelberg, although the Hospital was much smaller, he had on an average four or five diabetic cases in the course of each year. Now, the only way to account for this difference was that the Munich Hospital draws its supply from a beer (Bavarian) drinking district, while the Heidelberg Hospital

obtains its supply from the wine-drinking districts of the Rhine, &c. And, curious enough, it turned out that even the single cases of diabetes Pfeüfer had in Munich, he added, came from the Palatinate.

There can be no difficulty in explaining this action of stimulants, for, as said in the physiological part of the lecture, diabetes can be artificially induced in healthy animals by injecting small quantities of alcohol, ether, chloroform, or even ammonia, into the portal circulation.

For example, I injected 10 cubic centimetres of sulphuric ether diluted with 30 cubic centimetres of water into one of the branches of the portal vein of a full-grown Newfoundland dog half an hour after he had been fed. When he rose up after the operation, he appeared intoxicated, and staggered a little as he moved about. This effect, however, soon disappeared, and in a few hours the animal looked as if nothing had been done to him. In two hours after the injection was made, I passed a catheter into his bladder; but did not obtain sufficient urine to enable me to satisfy myself whether it contained sugar. Some hours afterwards, when I had obtained enough urine, I found that it readily reduced the copper in Barreswil's liquid, thus indicating the presence of saccharine matter. To assure myself that this effect was not due to the presence of any other substance, I boiled the urine in order to coagulate the albumen, of which it contained a little, then evaporated it almost to dryness, dissolved the residue in boiling alcohol, and filtered. The filtered liquid was next evaporated to drive off the alcohol, and an aqueous solution made. On testing the latter for sugar with the sulphate of copper solution, its presence was clearly indicated. Although by this method the existence of saccharine matter was rendered almost undeniable, I still wished to convince myself of its presence by some other means. The urine which the dog passed the next day was therefore fermented, and carbonic acid gas and a trace of alcohol were obtained, thus placing beyond a doubt the existence of sugar.

The following case shows how ammonia has the same power as ether in causing the liver to secrete an abnormal amount of saccharine matter:—

Into the portal vein of a good sized dog in full digestion, I injected fifteen drops of liquor ammoniæ diluted with forty cubic centimetres of water. In twenty hours afterwards, on the animal being killed, his bladder was found enormously distended with urine, which not only reduced the copper in the liquid of Barreswil, but fermented rapidly.

These two experiments are selected from a number of others, some of which I had the honour of performing in 1853 at the College of France before a commission appointed by the Société de Biologie; but as the results then obtained are identical it is unnecessary to cite them. I cannot refrain from mentioning with what pleasure I perused a communication of Bernard's, entitled "On the Influence of Alcohol and Ether on the Secretions of the Digestive Canal, of the Pancreas, and of the Liver," read before the Société de Biologie. M. Bernard, instead of putting the alcohol and ether, as I had done, directly into the portal vein, introduced them, by means of a long œsophagus tube, into the duodenum of dogs, and allowed them to be absorbed through the walls of the intestine, into the portal circulation. The result, as might *à priori* have been anticipated, was identical with what I had previously obtained. Bernard, in fact, found that six centimetres of alcohol, mixed with an equal amount of water, were sufficient to excite the liver to secrete a large quantity of sugar, even while the animal was fasting. With ether employed in a similar manner, he obtained no less successful results.

Rosenstein has still further confirmed the value of the physiological data I obtained by a very complete series of experiments on the influence of different kinds of drink upon the quantity of sugar, salt, and urea, daily eliminated by a diabetic patient. He performed the experiments, he says, with the view of ascertaining if the conclusions I arrived at were equally applicable to the human subject as to the animals operated upon, and he came to the conclusion that they are identical from his having obtained the following results:—1. Coffee, while it diminished the elimination of urea, increased

that of the chloride of sodium and sugar. 2. Bavarian beer had a precisely similar effect. 3. The influence of wine was also similar—namely, to decrease the elimination of urea, and augment that of the chloride of sodium and sugar. Lastly, he found that vinegar exerted a similar influence on the elimination of these substances.

If any one doubt the truth of the assertion that stimulants excite diabetes, let him select a case of that form of the disease arising from excessive formation, and after having carefully estimated the daily amount of sugar eliminated by the patient, allow him to drink a few glasses of wine during the next few days, and watch the result. He will soon find that the ingestion of stimulants is followed by an increase of sugar, and if stimulants increase the amount of saccharine matter in the urine of the diabetic, we can easily understand how their excessive use may induce the disease in individuals *predisposed* to it. The effect of stimulants introduced into the portal circulation also explains to us how a disordered digestion is not unfrequently followed by saccharine urine. I may here relate a curious fact in illustration of the latter remark. In 1852, at a time when I was much occupied in studying the physiology of diabetes, I regularly tested my urine twice a-day, and on one occasion I found it to contain a small quantity of sugar. On the day in question I had partaken freely of asparagus salad; and, thinking that this might perhaps be the cause of the presence of the sugar, I determined to try the effects of a greater quantity. The following day, the sugar having entirely disappeared from the urine, I again partook of the same kind of salad, both in the morning and afternoon. In the evening, on testing the urine, I found very distinct indications of sugar. As the observation was to me one of great interest, I determined to make some further experiments on the subject, in order to discover how many hours this state of saccharine urine would continue. During two days I ate large quantities of asparagus salad, taking care to have it made as stimulating as possible with vinegar and pepper. The result was far beyond my expectations, for, instead of the sugar disappearing from the urine in a few hours after I had ceased partaking of the diet in question, it continued to be secreted during several days, until I at last became very much alarmed lest the disease had been permanently induced. On the evening of the fourth day the sugar had almost entirely disappeared, but on the fifth it returned in increased quantity, so much so that a drop of urine falling on a boot left a distinct white spot. I could not account for the recurrence of the disease, as I had been particularly careful in my diet during the two previous days.

I have mentioned this experiment because it appears to me that if a flow of saccharine urine be induced in a healthy person, as I consider myself to be, by disordering the digestion and over-exciting the liver, it is very probable that a cause insignificant in itself, but operating upon a predisposed constitution, may tend to produce the disease. Sugar in the urine has been found after eating cheese and other indigestible substances. It is worthy of remark that Dr. Jessen, of Dorpat, has rendered horses diabetic by feeding them with hay damaged by moisture. M. Léconte has also found sugar in the urine of dogs after he had administered to them the nitrate of uranium. Several other substances have the same effect, and I have no doubt but that a greater number of stimulants will be afterwards found to produce similar results.—*Medical Times and Gazette, Oct.* 21, 1865, *p.* 437.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

33.—ON CERTAIN ERRORS IN THE TREATMENT OF JOINT-DISEASE.

By R. Barwell, Esq., Assistant Surgeon, and Lecturer on Comparative Anatomy, Charing-cross Hospital.

[The disease to which the author refers in this paper is strumous synovitis, and the error he impugns is taught in most hospitals, and is based upon high authority.]

I must be allowed to give some description of the morbid processes, which constitute, and the morbid growths, which are constituted by a strumous synovitis. When an inflammation, be it simple, traumatic, rheumatic, or strumous, attacks a synovial membrane, there is set up, besides the mere increase of vascularity with redness, pain, &c., &c.—there is set up, I say, a brisk action in the tissue itself, both on its inner surface and surrounding parts. This action is a very rapid growth of cells. All areolar, fibrous, basement membranes, in fact, all connective tissues, are composed of cells, somewhat elongated, stellated, or otherwise changed in form; but still possessing their nuclei and procreative power. Inflammation of such tissues essentially consists in the rapid production by these cells and nuclei of a numerous progeny, which lies heaped up in the meshes of the parent structure, or, at least, in as much of it as has not melted away in their production.

Now, it is on the ultimate fate of this progeny that the issue of the attack, that which in surgical lectures we were taught to call the "termination of inflammation," depends. If these cells evolve themselves healthily into new tissue, the inflammation terminates by resolution, leaving thickened structures behind. If the cells become pus-cells, the attack terminates by suppuration, and so on. This process of cell formation is exactly the same action as that which is produced to fill up an open wound, and which is called in such circumstances granulation. Now, whether they be situated externally as in an open wound, or internally as under the yet unbroken skin, such granulations have, under certain conditions of constitution, a great tendency to become indolent and sluggish—*i. e.*, the cells incline simply to reproduction and not to metamorphosis into tissue. We see many open sores, from which project large, soft, almost gelatinous granulations, which do not grow firmer, scarcely unite among each other, and make no progress towards forming a new tissue. At some period or periods of the disease a portion or all of this mass becomes pale, almost green in hue, its discharge thinner, and presently that district of jelly-like tissue disappears very rapidly, in part by suppuration, in part by a kind of slough or degeneration. This cause may be sufficient to make the wound, instead of projecting, hollow; but it will soon refill itself, and these changes may succeed each other again and again without any increase of tissue or advance towards healing; on the contrary, the granulating process has a tendency to invade more and more sound tissue of the neighbourhood.

The natural course with an inflamed joint, as with a wound, is, that the granulations should form new fibrous tissue, and the part recover with a certain amount of thickening according to the severity and duration of the disease; but it frequently happens, and especially in strumous constitutions,

that the granulations fall into the sluggish state described. Parts of them even will suppurate, degenerate, and disappear, so that abscesses, cavities, &c., are produced in the thickness of the mass itself, and these conditions may continue indefinitely without tendency to heal; on the contrary, more and more fresh tissue is constantly becoming involved. The white swelling, a name which has been translated into every European language, is a term properly confined to this sluggish disease. The *tumeur fongueuse* of certain French writers denotes the same malady. This is the disease which Sir B. Brodie termed "a morbid change of structure of the synovial membrane, and which he considered analogous to tubercle and fungus hæmatodes" ("Diseases of Joints," p. 85). When we have an opportunity of examining pathologically the condition left by this disease, we find it the following:—The tissues, forming and surrounding the joint, including bones and cartilages, are infiltrated with, or become converted into a soft, yellowish, or pinkish brown material, of a translucent nature, through which long sinuous vessels meander. The cavity of the joint, filled with pus, is much encroached upon by this material, which creeps from the side over the cartilages, and adheres to them in spots, where they are themselves converted into a similar structure. In fact, the joint has now become an abscess, surrounded by a thick, new structure—a transition tissue, which, examined either macroscopically, or microscopically, is found to be nothing else than granulation tissue, formed of round, fusiform, and fibrous cells—the offspring of the articular and periarticular tissues.

The course of the joint-disease for months, perhaps years, ay, indeed, the ultimate fate of the joint itself, depends upon the evolvement of this tissue, either towards completion or towards decay, exactly as the different conditions of an ulcer depend upon the development into new tissue or upon the degeneration of its cells.

The phases of a strumous synovitis are these:—

1st. Inflammation and active granulation.

2nd. Indolence, with languid cell-growth.

3rd. Development or degeneration of the granulations into fibrous tissue or into pus and sloughs respectively.

The degeneration, *i. e.*, the suppuration and death of the jelly-like granulation involve generally only portions of the substance, and form localised abscesses in the thickness of the mass, or an enlarged area of joint-cavity filled with pus. This central cavity opens after a time externally, the localised abscesses form communication with that area and with the outward air.

The external aspect and condition of the joint itself differ essentially in each of the above phases. In the first, the inflammatory, it is swollen, hot, tender, often reddened; the patient is in a state of pyrexia. In the second, the joint is more swollen, and its form, if superficial, is rounded, shapeless, dumpling-like; it is cool, and *not* tender, or but very slightly so; the patient's health is depressed, and the condition is not feverish. This phase is *par excellence* that of *white swelling*. Of the third stage we will speak in the sequel.

Now, the common and authorized mismanagement, which I deprecate, takes place in the second, the torpid phase of the disease. Infirmary, hospital, or private patients are, as a rule, ordered to remain in bed or at complete rest, with the limb on a splint, for months, even for years. We find these patients pale, exsanguine, etiolated, having a large, shapeless joint, cool, white, uninflamed, perhaps, with a great issue in it and a poultice over it. Thus, inactivity is added to sluggishness, enforced indolence to the passivity of disease, and with such treatment it is not to be wondered at that but few such joints recover; that Sir B. Brodie, whose one *panacea* for articular disease was rest, continuous rest, should have ascribed an almost malignant character to this malady, and should have been led to say, "In every case, in which I have had it in my power to watch its progress, the complaint has advanced slowly and sometimes has remained in an indolent state during a very long period, but ultimately it has always terminated in the destruction of the joint."

Deeply impressed with the want of success of the practice as above described, I have been led to adopt an opposite course of treatment in the second, the sluggish phase of the disease; and with such success as to encourage me to continue the same method and to bring it prominently before the profession. As soon as the inflammatory stage has passed, I prescribe movement—at first passive, then active—friction, pressure, and a series of exercises destined to restore form and flexibility. I will not now enter into minutiæ of such methods, since the one or two appended cases, copied from the many in my note-book, will suffice to show how and when one or the other should be used. But there remain essential points for consideration:—Firstly. What are the signs that we may safely remove a diseased joint from the splints, cease all remedies against inflammation, and adopt the treatment above described? Secondly, and this is very important. What are the signs that degenerative actions have already set in and advanced so far that such measures would be not merely useless but hurtful?

I will endeavour to answer these as plainly as I possibly can, but I must observe that the response to the former question is far easier than to the latter.

The signs that inflammation has so far ceased as to warrant the use of movement, &c., are coolness and whiteness of the joint, and, above all, a cessation of tenderness—not merely of a general more or less superficial tenderness (which often subsists in a splinted joint long after inflammation has ceased), but of the tenderness in the special locality appertaining to each joint. The fact of a special locality for pain is very peculiar, and requires an explanation. It is well known that, in every acute inflammation of joints, the pain of the disease is referred especially to one part of the organ; even although the whole limb may be painful, there is always one special point of which the patient more particularly complains. This fact, taken in itself, is singular, but practically unimportant, save that it throws a curious light upon the circumstance to which I would call attention, namely, that in chronic joint-diseases, in which pain during rest no longer exists, there yet remains a certain point for each articulation which continues tender as long as any inflammation persists. This point is in all cases that same place which in acute diseases is especially painful.

These are—for the knee, a small space over the inner trochanter and inside the patella; for the elbow, the posterior part of the junction between the radius and humerus; for the wrist, the union between the scaphoid and ulna; for the ankle, a spot just outside the extensor longus tendon. For the shoulder, this choice spot of tenderness is situated at the back and external aspect, where the posterior fibres of the deltoid cross the joint; at the hip, the test place is just behind the trochanter. It may appear to many a dangerous doctrine to assert, that, tenderness being absent in this choice seat of pain, inflammatory action in the joint itself has ceased. No surgeon would ever, I presume, examine a joint without feeling it all over; but the especial attention, the indication for treatment, is to be taken from the condition of this point.

Let us, then, suppose a case to come before us. A patient has been for some months affected with strumous synovitis; the knee is round, soft, having a pasty feeling, and is white and cool; there is a little dull sense of pain on pressure with the tip of the finger all over the joint; but rub it with the palm of the hand slowly and with some pressure, and this slight tenderness, which is merely the effect of the skin having been kept from the air by bandages or poultices, will soon cease. Press now with some firmness over indifferent parts of the joint—*i. e.*, everywhere but on the point to be interrogated; see that all this superficial sensitiveness has disappeared, or that, at all events, you can appreciate its amount; then, and not till then, make with the thumb pressure, at first gentle but gradually increasing, until it becomes pretty considerable. If no expression of pain be elicited, you may feel sure that joint inflammation has ceased.

Now, to continue rest, antiphlogistic treatment, poultices, or the like, is the mismanagement I have so frequently mentioned. Such treatment di-

rectly aids in the slow, sluggish destruction of the limb, which, under such circumstances, is so certain, that a man with all the experience of Sir B. Brodie considered it inevitable. At the present day even, I have no hesitation in saying that nearly all surgeons would still order such a joint to be kept at rest, to be blistered, to have issues in it and poultices applied, although innumerable examples show how worse than futile is such a course. It is because I hold this conviction that I have ventured to bring forward in so detailed a manner a subject which is not perfectly new. I pointed out in 1860, in my work "On Joint Diseases," the false practice of this continual rest, and I have since published lectures on the subject; I also detailed the choice seat of tenderness for each joint. Since then my experience has impressed upon me even more fully the value of the practice which I then inculcated, and how essential to a joint in this condition is the stimulus of movement, of pressure, &c., to cause the granulation cells to contract and form themselves into new tissue, instead of remaining sluggish, until they degenerate into fatty cells and sloughs.

There is yet another question to be answered—viz., after degeneration has once set in, when there are abscesses in the thick tissues surrounding the joint which open and discharge externally, when there is reason to suppose that the cartilages have disappeared totally or in part, are we to keep the limb still at rest in the hope of producing anchylosis, or are we to inculcate movement, &c.? My answer most decidedly is, that in cases where the health is not very much broken, where destruction has not invaded all the tissues, where the central abscess is not larger than the thickened tissues themselves, and where there is no grating, carefully-arranged pressure, followed by passive movement, will do more to consolidate the tissues and cause the abscesses to heal than can be imagined by those who have not tried the method.

One point, however, must be noticed—viz., the fact that when a limb has been kept at rest for some time, whether for fracture, joint-disease, or other cause, movement will always produce pain; and this is so often believed to be pain in the joint from continuance of the inflammatory action, that the salutary remedy is checked. The surgeon must carefully distinguish the one from the other—a diagnosis in which he will have but little difficulty, if he carefully compare the position of limb in which the pain chiefly occurs and the action of the muscles.—*Medical Mirror, June*, 1865, *p.* 334.

34.—ON AMPUTATION BY SINGLE FLAP.

By Henery D. Carden, Esq., Consulting Surgeon to the Worcester General Infirmary.

In 1845, my attention was drawn to a few of my old cases of amputation; and I was much struck by the fact that, where double muscular flaps had been employed to cover the end of bone, the cicatrix had been drawn gradually over the sawn part, rendering the stump incapable of bearing the least pressure; whereas, in the two cases referred to, the flaps were plump and painless, and capable of bearing very considerable pressure on the end without flinching.

Under these circumstances, I determined to try the mode of amputation by a single flap of skin; and I have been so well satisfied with the results, that I have never since that time used any other.

The operation consists in reflecting a rounded or semi-oval flap of skin and fat from the front of the joint; dividing everything else straight down to the bone; and sawing the bone slightly above the plane of the muscles; thus forming a flat-faced stump with a bonnet of integument to fall over it.

The operation is simple; and is performed easily in two ways.

The operator, standing on the right side of the limb, seizes it between his left forefinger and thumb at the spot selected for the base of the flap, and enters the point of the close knife to his finger, bringing it round through skin and fat below the patella to the spot pressed by his thumb; then, turning the edge downwards at a right angle with the line of the limb, he passes

it through to the spot where it first entered, cutting outwards through everything behind the bone. The flap is then reflected, and the remainder of the soft parts divided straight down to the bone; the muscles are then slightly cleared upwards, and the saw is applied.

Or, the limb being held as before, the hand and knife may be brought round under the limb, as in the circular operation, and the blade entered near the thumb and drawn round to the opposite side, when the ham may be cut across by turning the edge of the knife upwards, and the operation completed as before. This requires rather more dexterity and flexibility of wrist than the first method.

Or the flap may be reflected first, and the knee examined, particularly if the operator be undetermined between resection and amputation. In amputating through the condyles, the patella is drawn down by flexing the knee to a right angle before dividing the soft parts in front of the bone; or if that be inconvenient the patella may be reflected downwards.

In this operation, the arteries are easily found and secured, without loosening their tourniquet; but there is a point worth noticing whilst tying the popliteal artery in this situation; namely, its close proximity to the vein, and the tendency to secondary hemorrhage from the vein in consequence. This occurred to such an extent in two of the early cases, that I had to re-open the dressings, and found the closure of the vein prevented by the stroke of the artery. This was immediately checked, and afterwards always prevented, by gently separating the vessels for an inch upwards. The flap falls easily over the end of the bone; and, when united to the posterior integuments by a few pins or sutures, is drawn strongly upwards and backwards by the greatly retracted flexors, and has a somewhat puckered and redundant appearance at first. This is no disadvantage.

The dressing may be of the lightest and simplest kind; a few strips of soap-plaster between the sutures, and a light bandage perhaps. I have often been content to use a cap of soft porous linen, drawn over the end of the stump, and secured above by a broad circular strip of plaster. Whatever dressing be used, it is of great importance to loosen it and examine the stump early, and to carefully provide for the free escape of serum. I remove the pins or sutures at from twenty-four to forty hours at furthest, and never find that I have opened the dressings too soon.

The principal advantages obtained by this plan appear to me to be the following:—the facility afforded for amputating, either through the knee or through any part of the lower end of the femur; thus proportionately avoiding the shock of higher amputation, and the risks of exfoliation, pyæmia, &c., attendant on sawing through the cylinder of the bone; the simplicity of the whole proceeding, and small extent of the wound; the favourable position of the flap for dressing and for the escape of discharge; painlessness and quietude of the stump, the principal nerves being divided high up and drawn out of reach of pressure or exposure; the fitness of the stump to stand and walk upon; the bearing being broad, and the skin employed being accustomed to bear the weight of the body in kneeling; the cicatrix being drawn clear of the point of the bone, out of the reach of pressure.

Having been frequently urged, by friends who had witnessed the result of my method, to publish the foregoing cases, I had prepared the table in 1858, accompanied by the sketches and photographs, with that intention, when Mr. Teale's valuable work on *Amputation* appeared, and seemed to me, at first sight, to render anything further on the subject unnecessary on my part. Mr. Teale's method of operating with a long and a short rectangular flap appears to provide an excellent stump to walk upon, when the materials for forming it are attainable. The evidence of Messrs. Grossmith and Heather Bigg is conclusive as to the great superiority of Mr. Teale's operation in this respect over the double flap and circular plans. His rate of mortality, also, is greatly in favour of his plan, compared with the usual methods. It appears to me, however, that an eight-inch muscular flap is not always to be had, with due regard to the essential principle of removing the point of severance as far as possible from the trunk. (Erichsen's *Science and Art of Surgery*,

second edition, page 20, lines 33, 34.) The extent of wound is certainly great, being thirty-two inches in a circumference of sixteen; whilst the divisions of muscles longitudinally for eight inches can scarcely be accomplished at all times without risk of bad consequences, particularly by surgeons less skilful than Mr. Teale.

Mr. Lister's observations on Teale's method, in his essay on Amputation in the third volume of Holmes's *System of Surgery*, lately published, bear so directly on the subject of this paper, that I may be allowed to quote his opinion. He says, page 162: "Thus, in a particular instance, where the development was by no means extraordinary, the dimensions were such that, supposing the anterior transverse incision made at the level of the upper border of the patella, it would have been necessary, in order to preserve Mr. Teale's proportions, to saw the bone eleven inches further up, or full five inches higher than if the modified circular operation were performed. This must, *cæteris paribus*, very seriously increase the risk, which is always greater the nearer the seat of amputation is to the trunk. The cut surface is also very extensive; and this would give rise to a correspondingly large amount of suppuration, if immediate union failed. It therefore becomes an important question, whether the advantages of Mr. Teale's plan may not be obtained in some less objectionable manner."

He then proceeds to propose a modification by an anterior flap of six inches in front, without any posterior flap at all, retracting the muscles two inches before dividing the bone; and observes: "That such a procedure would answer well is not, however, a mere matter of inference; for, since the appearance of Mr. Teale's work, Mr. Spence of Edinburgh has performed some amputations of the thigh according to his directions, and others by cutting from without inwards a large rounded anterior flap, dividing the posterior parts at the level of its base by a perpendicular sweep of the knife, and retracting the muscles before applying the saw. I have had the opportunity of comparing the fulness of the cushion in both cases; and Mr. Spence tells me that, where it has been formed after the latter mode, it has proved capable of bearing a large proportion of the weight of the body.

"Still, this proceeding would involve as high a division of the bone as Mr. Teale's, if the anterior flap had the length above mentioned: and the question yet remains, whether it may not be curtailed, and eked out with a short posterior flap, without impairing the usefulness of the stump. Provided the covering of the bone be sufficiently full, the essential thing to be attained is, that the tender cicatrix shall be so placed as to be free from any chance of being squeezed between the bone and the bottom of the socket of the limb. Now, it fortunately happens . . . that the bone lies forward among the muscles, so that even its posterior surface is considerably anterior in position to the longitudinal axis of the limb. Hence a flap half the diameter of the limb would more than cover the bone; and one as long as two-thirds of that diameter would insure the scar being considerably behind the point of pressure; especially as the absence of the two inches of bone above the base of the flap would allow it to drop lower than it otherwise would. Also, in the further progress of the case, the contraction of the muscles will draw the cicatrix still further back; an effect long since noticed by Alanson after his circular operation."

Mr. Lister then proposes that, "in order to compensate for the diminution of the anterior flap, a posterior one must be made of rather more than half its length, and of rounded shape, for adaptation to it; and the integument of this flap should be dissected up before the posterior muscles are divided, so as to set it free from the effects of their contraction. On the other hand, the anterior flap, after being marked out by carrying the knife through the skin and fat, should be raised so as to contain a good deal of muscle, which will be useful, both by insuring the vitality of the rather long flap, and also by increasing the thickness of the cushion below the bone; while any tendency to contraction that it possesses (small compared with that of the posterior muscles) will be counteracted by the force of gravity, through which it will naturally tend to occupy its proper place.

"By this mode of amputating, the bone will be divided about as low as in the modified circular operation; and, though I have not yet had much opportunity of trying it on the living subject, I feel satisfied from the above considerations, and from experiments on the dead body, that it will attain the essential objects of Mr. Teale's plan, while it will occupy less time than either of the methods alluded to. The flaps, when brought together, will be free from tension; and the stump, if properly dressed, most favorably circumstanced for safe and speedy healing."

In his observations on dressing, Mr. Lister lays great stress on providing a free escape for serum. This, I can say from experience, is a most important point, and one that cannot be too strongly urged.

It will be obvious from the above extract, that Mr. Lister still considers that no plan hitherto adopted has fulfilled the requisite conditions in amputations of the thigh; namely, that of obtaining a long and useful stump in this situation by a sufficiently safe and simple operation.—*Mr. Carden's Pamphlet on Amputation.*

35.—ON AMPUTATION.

By Wm. Fergusson, Esq., F. R. S., Surgeon to King's College Hospital, Professor of Surgery in the Royal College of Surgeons, and Surgeon Extraordinary to H. M. the Queen.

[There is a rising feeling to preserve a long anterior and short posterior flap, in order to keep the cicatrix at the back of the stump and to facilitate the escape of matter, whilst the patient lies on his back.]

A compromise, I imagine, goes far to settle what may be deemed the best method of amputation in such important places as the arm and thigh. A little of the flap and a little of the circular constitute, I believe, the most perfect operation; and in my own practice I have long followed out that idea.

Of the two operations, I have always thought that the circular is the most difficult. The comparative difficulty of drawing up the skin and other tissues, so as to make bare the bone at a sufficient height, is considerable, and a great contrast to the easy way in which this is accomplished by flaps. The facility of exposing the bone high up is comparatively so great with flaps that I am impressed with the idea that more is often done in this way than is actually required; and, if I am correct in this, I believe that a larger wound is the result than if the bone were not so freely exposed. If in the transfixion of the limb—that being the usual way of forming the flap—the knife crosses almost as high as where the bone is to be divided, there is a greater wound made than if the transfixion were kept considerably lower: the skin on opposite sides, being cut, is wounded where it might be left entire; for after flaps of moderate extent are cut, they, with the tissues above, may be so pulled upwards that, with some circular sweeps of the knife, the bone may be denuded an inch or two higher, and so there will be a smaller opening in the skin.

Now this is that kind of compromise to which I have referred in the preceding remarks; and if, in transfixing, the flaps be made purposely short, and then, retraction being made, the knife be carried round the exposed tissues which cover the bone, a cone will be formed resembling that in the ordinary circular operation, and thus by a combination—a compromise of the two methods—a covering to the end of the bone, in other words a stump, will be left superior, in my estimation, to any other.

Yet this is only an individual opinion, and there are many who hold to the ordinary method by flap, many who still hold by the old circular, and some in recent days who carry the flap system beyond what was ever contemplated by its early advocates. Thus Mr. Carden and the Worcester surgeons take especial care to exclude other soft tissues, so that skin only shall cover the bone or bones.

Irrespective of either circular, flap, ovoid, or any other sort that may have been fancifully named, I think it must be admitted that amputation by

almost any fashion has made progress during the present century. I take this opportunity, however, of entering my protest against that of burning or wringing a limb off by any kind of apparatus whatever. Cheselden gave us the case of the miller who had his arm and scapula torn off by machinery, and yet survived. Several instances of a similar kind have since been met with. We have good data as to how the bleeding is arrested in these cases. Hands and arms, feet and legs, have been torn off by accidental rude force. A chain has been put round a rabbit's leg and drawn so tight that the part has dropped off within a few minutes, and the like has been done on a human thigh; but I sincerely hope that these processes may never appear as part of the surgery of this or any other century. Let Cheselden and others have due credit for treating successfully enormous and unheard-of wounds, but let a due distinction be drawn between that which is possible and that which is proper.

With the exceptions alluded to, I conceive that there has been much beneficial progress in amputation in recent times, and it is part of my object in this lecture to give illustrations to that effect.

As may have already been remarked, I do not attach so much importance to the question of flap or circular as many do; and whilst giving the preference, as a general practice, to the mixed manner above referred to, I believe that with a well-performed operation in any of these ways, or with any zig-zag which circumstances or the surgeon's fancy may dictate, a stump can be produced which shall defy adverse criticism; while by any of these methods, badly executed, any or all of the evils pertaining to bad stumps may be the result.

In my younger days, the grand effort of all operators, whatever the kind of operation, was to have an abundance, or I might say a superabundance of soft material to cover the end of the bone, and make what was called a fleshy stump. The bugbear in those days seemed to be the risk of scantiness in this respect, and hence every substance was looked to which might afford the needful amount of soft materials. Every now and then it was evident that mistakes were made in this direction: a greater length of soft parts was left than was needful; and occasionally, when the length seemed perfect, the flap was actually too thick to bend readily and properly up against the end of the bone or bones. This was often seen particularly marked in the flap operation in the leg, a little below the knee—a proceeding at that period mostly in fashion. If the operation were done for an accident—for example, on an athletic navvy—a flap being formed by transfixion from the muscular calf often proved a troublesome one to treat. The skin by this method of operation, although in reality cut lower down than the muscles, retracted more; and although that retraction was seemingly overcome in the dressing, as stitches were applied, the subsequent swelling of the muscles often burst the superficial union, and, shooting between the margins of skin, presented for weeks or months a large granulating sore, which healed but slowly, much to the distress of the patient and vexation of the surgeon. I remember well that Mr. Liston was so much annoyed in this way that he latterly made the posterior flap in such an operation much thinner than on former occasions; and I can speak freely of my own experience in the same operation. It is long indeed since I came to the conclusion that it was unwise to leave a thick muscular flap. Part of the fault in such cases arose, I believe, in the almost uniform practice of transfixing and cutting outwards. I am by no means convinced that this is invariably the best method of performing the flap operation, and I feel assured that cutting from without inwards is in many instances followed by the best results. But I may say that I have seen the two extremes with regard to the substance of flaps, for in latter years some of our ablest surgeons have advocated the practice of carefully excluding all material but skin, cellular tissue, and fat. We are greatly indebted to the surgeons of the Worcester Infirmary, Messrs. Carden, Sheppard, Budd, and Walsh, for our knowledge of upwards of sixty such cases, wherein the practice has given much satisfaction.

Some of these recent views I attribute largely to the proposals of Mr.

Teale, of Leeds, who, in this as in other respects, has contributed so much to the established reputation of that school of British surgery. In a series of examples he has endeavoured to show the advantages of long flaps from one side of a limb, wherewith to cover the ends of bones, and short flaps in which the main vessels and nerves are preserved.

I should be untrue to the position I hold were I to decry amputation either at the shoulder or the hip-joints. On the contrary, I consider them as amongst the best achievements in surgery when judiciously applied. But I believe that there is still much to be impressed on the surgical mind in regard to amputation at other joints; and with a few examples of the kind I shall close this lecture.

Happily for the character of surgery, amputation in the arm for disease of the elbow may now be considered obsolete. I feel inclined to say as much for injuries of the elbow strictly local. There may be an exception every now and then to that general rule, as I hope it may now be considered. If such there be, why should the surgeon almost invariably get into the middle of the arm for his incisions? Why should he not take the covering of the end of the humerus from the forearm? If he wants skin only, there will most likely be plenty of it; if muscle, he has the whole fleshy mass at the head and front of the forearm to deal with. I feel convinced that amputation near the middle of the arm has often been done when this more conservative process might have sufficed.

Then at the wrist. How often, may I ask, has amputation been done at this joint? I know that it has been done, but I have never seen it, excepting in my own practice. Every now and then good results of resection of the wrist are effected, as has been proved years ago by Mr. Stanley, and more recently by Mr. Butcher and Mr. Lister; but from my personal experience after repeated trials I am far from being sanguine of the general applicability of excision in this locality. Yet I can speak in unmeasured terms of approval of amputation at this part in preference to amputation higher up in the forearm.

Again, look to the knee as the seat of amputation. Twelve months ago I asked in this theatre, how many surgeons in Britain had performed this operation. I was aware at the time how much had been done by Mr. Syme, Mr. Liston, Mr. Lane, Mr. Erichsen, and a few others, but particularly by Mr. Carden, of Worcester, whose doings, in my opinion, reflect vast credit on modern surgery. In the British Medical Journal for 16th of April, 1864, Mr. Carden published the particulars of thirty-one cases in which he had performed this operation, with an expression of opinion that it was likely to be more successful, and that the stumps were better than in ordinary amputation in the thigh. Of that I have long had no doubt. More than twenty years ago I had satisfied myself on that score; and had it not been that I fancied I struck a richer vein in surgery by the revival of excision of the knee, I am certain that most of those cases which I referred to in my lectures on this operation last season would have been brought before you now as illustrations of amputation at the knee. Before I commenced excision I had amputated frequently at this part, and with the most gratifying results. One case I have recorded in my book on Surgery of a man who walked ninety miles in three days on such a stump; and from all I have seen and know of this operation I am greatly astonished that it has been performed so rarely. No doubt excision turned attention in another direction. But it might be asked, Why have those who object to excision, who have adhered to the old slashing system of amputation, not taken to this? I imagine that the ulcers, the sinuses, the abscesses, the disorganized synovial membranes, have all deterred surgeons as much from amputation on such questionable grounds as it has deterred them from excision. Yet in excision how worthless have such objections been proved!—and have we not similar experience of amputation at the ankle-joint with disease in that articulation and the soft tissues around?

In this operation, I am not aware that any teacher has, more than myself, so strenuously drawn attention to one particular feature wherein it differs so much and so importantly from ordinary amputations in the limbs. In the

forearm or arm, the leg or thigh, the coverings to form the stump are invariably shaped from the soft parts in these regions. But in amputation at the knee, the covering to the end of the femur, that which is to form so important a part of the stump, is preserved from the leg—a piece of surgery which I consider supremely conservative. Supposing that excision of the knee should yet be abandoned, why should not this operation take the place of the larger mutilation six or nine inches higher up.

When the condyles of the femur are sawn off in this operation, Mr. Syme has stated his opinion that there is greater risk of serious inflammation in the broad cancellated surface of the bone than when it is exposed higher up in the ordinary operation. But whilst admitting some such cases, there is no positive proof from numbers of the correctness of this view, and it is quite contradicted by the successful practice of that gentleman in amputation at the ankle-joint (where a broad surface of tibia is almost always exposed by the saw), as also by the usual good results of excision. Moreover, no such evil seems to have occurred in the numerous examples recorded by Mr. Carden. But in some of these cases the cancelli need not be exposed at all. The condyles and articular cartilage, being healthy, may be left. And here I cannot, as a zealous surgeon, but express my thanks and admiration to Mr. Lane for having displayed what an admirable stump may be made in some instances by reserving the condyles and cartilages untouched, and covering them with a flap of skin merely from the front of the leg. Previously, in as far as I am aware, most operators had reserved a covering for the end of the femur from the calf of the leg, but few had ventured to take the calf away and leave only a flap of skin. This, however, is an important feature in Mr. Carden's practice.

It remains to be seen whether such a thin covering or the fleshy substance of the calf will prove the best stump. It will also be an interesting matter for future observation whether a stump with the entire femur or one with the condyles sawn off will be best. No doubt, if this operation comes much into use, there will be a necessity in the majority of instances to remove the condyles; but if it were proved that a stump with the condyles is better, it would be the duty of the surgeon to preserve them on all proper occasions. I am not prepared to give a decisive opinion on such interesting and important points.

As a strong indication of how this operation is estimated by some of the active spirits of the day, I may refer to the opinions of Mr. Butcher, of Dublin, who, in his recent work on "Operative and Conservative Surgery," speaks of it with all that enthusiasm which characterizes the writings of that very able and enterprising surgeon.

Lastly, as one of the greatest improvements in modern surgery, let me refer to amputation at the ankle-joint.

It is now about a quarter of a century since Mr. Syme proposed a method of removing a diseased or damaged foot at the ankle, different in all respects from any previous proceeding. His idea was to preserve the ordinary tissue covering the lower surface of the os calcis, so that it might be brought up against the end of the tibia, and leave a pad almost equal to the normal substance, whereby a stump might be retained superior to any that could be procured from the leg, or from any of those flaps which had previously been proposed from the sides or surroundings of the ankle-joint. At this date it is hardly needful to show how this operation has taken with the profession. Like many others in surgery, it has its faults; but, take it with evil and good, I know of no amputation—no style or kind of amputation which deserves more high consideration. There is everything associated with it to lead to perfection in our modern estimation of such a proceeding; a long stump, and a perfect covering to the end—a covering more perfect than that of any other stump; for the reason that the very bit of soft material on which we naturally stand is still preserved for the future basis of this support. Stumps in the upper extremity are put to little trial or strain, as compared with those in the lower; for these are always looked to as supporters of the weight of the body. The soft tissue under the os calcis is the only part of the lower extremity

which has been designed by Nature for this purpose; and, with a happy idea, Mr. Syme proposed to reserve this tissue.

The general success of this operation has been highly satisfactory. Evils have followed in various instances; and in what operation, I may ask, do they not? As a whole, looking to the small part of the body necessarily removed, the great length of limb preserved, and the natural character of the end of the stump, I most cordially express my opinion that this is the greatest addition to amputation in modern times.

In saying this I do not overlook the proposal of Pirogoff to retain the end of the os calcis. I consider the operation of the Russian surgeon a decided improvement on that of Mr. Syme. Yet it is, in my opinion, only a modification, which, even should it ultimately be allowed by all to be an improvement, by no means detracts from the merit of Mr. Syme for reserving a flap from the sole of the foot. The idea—one of the happiest in modern amputations—corresponds with that which I have insisted on as an important feature in amputation of the leg, where a flap or covering for the stump of the thigh is preserved from the leg.

It is not so much my object at present to compare one operation with another, as to mark the modern development of surgery; and, if I am not mistaken, this operation may, in regard to amputations, be considered as far ahead of any other in this department, and I therefore refer to it specially as one which pleasingly characterizes the progress of surgery in the present century.—*Lancet, July* 8, 1865, *p.* 29.

36.—AMPUTATION OF THE THIGH.

By Prof. Fergusson, at King's College Hospital.

[In the following case there are some interesting and novel points in the mode of operating. There was a large tumour springing from the inner condyle of the right tibia.]

The operation was performed as follows. The patient being chloroformed, an assistant compressed the femoral artery as it passed out under Poupart's ligament, no tourniquet being used. Mr. Fergusson raised the diseased limb nearly at a right angle with the patient's body for about two minutes; he then transfixed the thigh directly in front of and close to the femur, about three inches from the patella, bringing the knife out obliquely downwards. He proceeded in a similar manner at the back of the thigh, making as it were two short flaps. He then divided the tissues close to the bone, while an assistant made retraction. The bone was sawn off about three inches higher than where the knife first passed in; the vessels were then secured, and the flaps brought together with silk ligatures. The wound made but one line, as in the circular operation; and very little blood was lost. Strips of wet lint were then applied to the stump, and a roller over all.

Mr Fergusson made the following remarks. He said that, after a long experience, he thought it better, in amputation of thigh, to depend on compression of the main artery by the finger of an assistant, than to trust to a tourniquet. It might have been also observed that he raised the limb for a short time, so as to unload the vessels, as it was of importance that as little blood as possible should be lost by the patient in her weak condition. The consequence of this proceeding was satisfactory; not more than an ounce of blood was lost. There were two modes of operation for amputation of the thigh, usually styled the flap and the circular; and he was of opinion for some time that a modification of each could be used, as in the present case, with very great advantage. It may have been observed that he transfixed the limb as in the flap operation, bringing the knife out obliquely downwards; and that he divided the tissues adherent to the bone, as in the circular operation; while retraction was made by an assistant. The consequence was apparent when the flaps were brought together. We had a wound in one straight line, getting rid of the side wounds, which very often extended

three inches up the stump in the ordinary flap operation. He thought that this mode of operating would be found to be a great improvement on the old methods.—*British Med. Journal, June* 3, 1865, *p.* 556.

37.—ON FRACTURES.

By Dr. William Lyon, Surgeon to the Glasgow Royal Infirmary.

In the Glasgow Royal Infirmary fractures are very frequently at once put up in pasteboard splints, sometimes in a starch bandage. In this way, I believe, the patient suffers much less pain, and the practitioner a great deal less annoyance. Allowance is made for the occurrence of swelling, but if this happen, to any extent, it is easily relieved by slitting up the bandages. By this method of treatment patients are often able to leave their beds in the course of two or three weeks. While in the simple fractures there is hardly any danger to life, we have found it a very different affair with the compound fractures. Out of four cases of compound fracture that I had lately in the hospital I lost three, and one still remains under treatment. In one amputation was performed, and the patient died of pyæmia. Removal of the bone was practised in another, where amputation would not be submitted to, and perhaps this was fortunate, as pyæmia probably existed before the proposition of amputation was made. A third patient, who suffered compound fracture by his limb passing between the spokes of a wheel of a railway engine in rapid motion, and in whom there was much bruising and laceration of the soft parts, was thought, nevertheless, to have a chance of recovery without amputation. He went on very well for a week. Very high inflammation then set in, and this was followed by very copious suppuration. A thorough examination (an examination which could not have been instituted with any propriety at the time of his reception) was now made, and it was believed that his only chance was by amputation. It was performed, but he was seized with traumatic delirium and sank. In fact, so dangerous did we find compound fractures, that a remark was made by one of my colleagues (and I am almost inclined to coincide in it), that if in cases of compound fracture the universal rule was to amputate, a greater number of recoveries would take place. At all events I am deeply impressed with this, that in all cases of compound fracture it is of the very utmost importance to make a thorough examination on admission, to decide whether amputation should be performed or not; for if secondary symptoms are allowed to come on and to progress to a great extent, even though amputation be performed, there is little or no chance of recovery. And therefore, in such cases, I have established the rule with myself to put the patient under chloroform, to ascertain by a careful examination of the parts whether an attempt should be made to save the limb, or amputation at once performed.

We have had cases of compound fracture and dislocation at the elbow, at the ankle, and very frequently in the fingers. You are all aware of the great danger of a large open articulation. Possibly you are not quite so much alive to the danger when the open articulation is a small one. All that time will permit me to do is to remind you of the great importance of performing excision in all cases of open articulations, whether large or small, as by this you diminish very much the hazard of the accident. A woman fell from her chair, and dislocated and fractured the end of the humerus, throwing it upon the front of the forearm. We performed excision, and in the course of a little while she recovered, with the limb in a condition very little the worse for the accident. In the case, too, of a man whose ankle was excised, he left with a useful foot in a short time. A similar result was obtained in a little girl, in whom the soft parts in front of the ankle were completely divided. At the time of her dismissal from hospital her foot promised to become exceedingly useful. I remember a very striking example of the efficacy of this mode of practice, and the danger of the contrary. A patient was admitted with an injury of the elbow, such as I have described to you, and I perform-

ed excision. One of my colleagues had a boy admitted under similar circumstances, about the same time, and he decided to reduce the dislocation and to stitch the wound. My patient, a man of 50, recovered perfectly; the boy was dead in a few days. A man was admitted with compound dislocation of the thumb. It was reduced, but shortly afterwards cellulitis set in, which extended rapidly up to the shoulder, and the patient sank under it. I blamed myself very much indeed for not following up my usual practice, in all such cases, by excising the joint—an operation which I have good reason to extol.—*Glasgow Medical Journal, July*, 1865, *p.* 139.

38.—ON OPERATIONS ON THE JAWS,

By William Ferguson, Esq., F. R. S., Surgeon to King's College Hospital, Professor of Surgery in the Royal College of Surgeons, and Surgeon Extraordinary to H. M. the Queen.

[Thirty or forty years ago little or nothing was done in cases of tumour of the jaws, nature being permitted to take her course, and years of misery and a lingering death were the result.]

Of all innovations the operations for removal of tumours of the jaws have created the greatest impression on my mind. There is little active excitement associated with the operation of lithotrity; and ovariotomy, although involving the life of the patient, and demanding both energy and courage on the part of the surgeon, cannot, as regards performance, be considered a high-class operation. But for the perfect removal of these tumours, I am inclined to think that the highest requirements of operative surgery are called forth. There is such variety of manipulation, such necessity for caution, yet such boldness in action, that in my opinion neither removal of ovarian tumours, nor of the most formidable growths in the scrotum, can at all compare in scientific accuracy with those upon tumours of the jaws.

[The presence of the teeth in the jaw bones forms an element for mischief such as is absent in all other bones. When tumours are present a question is often raised as to the injurious influence of certain teeth,]

The abstraction of a tooth in such a case is merely fencing with the outside of the disease. In irritation of a socket this doubtless may do good, although at a sacrifice which time might possibly save, but I cannot say that I have ever seen the removal of a tooth produce any substantial benefit in the cases under consideration. The tumour itself must be removed if good is to come from surgical interference. I trust that I am not less acquainted than my neighbours with common-sense surgery, but I never saw a tumour of the jaws dispelled by constitutional treatment. In doubtful cases, and where there has been much derangement of health, I have seen favourable changes, in time and through judicious management. I have even seen a case, where to all appearance, a tumour was malignant, deep-seated, and beyond all hope of a cure either by nature or surgical interference;—such an opinion was given by one of the best surgeons of the day, and such I confess was my own;—yet in the end it proved to be only a chronic deep-seated abscess, which burst, and got well spontaneously. Such mistakes do little credit to surgical diagnosis, and let us hope they are of rare occurrence.

But I wish now to refer to such cases as are beyond the power of hygiene, and where a process of removal is decided on. Caustics are of little value, ligatures out of the question; in short, whatever enthusiasts may think of their skill in treatment constitutionally or locally, I wish to speak solely of those requiring cutting for their removal—I wish to speak of excisions of portions or of the whole of the jaws.

I have selected this theme on the present occasion, partly because it is illustrative of the progress of surgery in the present century, and partly because I imagine that my favorite conservative practice may be as usefully developed here as in any other region of the body or in other cases of surgery.

The first and early operations for the removal of tumours in or of the jaws, initiated by Dupuytren, Gensoul, Hodgson, Wardrop, Lizars, Syme, and others, produced great sensation in the surgical world. In cleverness of

conception there seems scarcely a doubt regarding them in the present day; and in vigor of execution they have not yet been surpassed. Yet, even here, it may be doubted if perfection has been achieved; and I venture to make—indeed, I may say, reiterate—my humble contribution towards it, for most that I am now about to say has been stated over and over again in my clinical teaching.

I have no doubt that in diseases of the jaws operations have been performed when they were not warranted; and I am equally of opinion that they have been neglected when they might have proved of the utmost value. Here, as in other departments of surgery, perfection will probably never be achieved. Mistakes and improprieties will occur even in the best regulated minds and hospitals. I make no pretensions to be above them myself; but, by way of originality, I shall plead for a share of conservatism, even here, where it has been comparatively little thought of.

Happily, cancerous tumours of the lower jaw are somewhat rare; but cancerous ulceration, beginning in the gums, or more probably extending to them from the cheek or lips, is by no means uncommon. When the surgeon can encompass the disease in the cheek or gums with the knife, he may clip away the alveolar ridges with good prospect of a satisfactory result, provided the disease be of the kind called in modern days "epithelial." If there be no glandular affection in the neck, the operation is likely to succeed, just as with cancer of the lower lip; but where that kind of cancer has eaten away the lower lip, and laid hold extensively of the bone itself, I deem excision a misapplication of surgery. There is not the smallest chance of a permanently successful issue. I have known this done, and even a flap of skin brought from the neck to fill up the gap; but I doubt the wisdom of the proceeding.

I look upon the lower jaw as giving the most forcible examples of the value of a doctrine which I have long advocated—albeit, contrary to the opinions of many, possibly even against the ordinary doctrines of surgery. In one of my lectures last year I referred to this. Tumours of the lower jaw are often removed by vertical section, and both ends, so to speak, of the bone are left. Everyone knows how successful these operations usually are. When a section is made in the healthy part, it may truly be said that disease seldom if ever returns. Yet this feature is totally lost sight of in the bones in other parts; and so if a tibia or fibula be affected with tumour, nothing but total ablation will satisfy. Amputation in the thigh is the step. If the femur is affected, the hip-joint only will suffice for the sweep of the amputating knife; and a like pathology prevails in regard to other long bones. But in the lower jaw the surgeon will cut out an inch or two, or two-thirds, of the bone by transverse section, and the pathologist, very properly in my opinion, says not a word against the proceeding. I had great pleasure last year in referring to Mr. Syme's views on this subject, whilst alluding to his novel and bold operations on the scapula and head of the humerus, which he performed with a view to preserve the greater part of the upper extremity; and my own impression is so strong in regard to this, that I actually advocate horizontal sections of the lower jaw when it appears practicable. If only as much, in a state of health, can be retained as shall in some measure preserve the shape of the lower part of the face, so as to prevent the collapse which takes place when the whole mental portion is taken away, I fancy that the surgeon enacts a good example of conservatism.

Again, in operations here, it has been much the custom to make the incisions run into the mouth, thereby involving division of the lower lip. Now, although I do not mean to deny the occasional necessity for this extensive wound, I am strongly of opinion, from my experience, that there is no urgent necessity for division of the prolabium. The mobility of the lips is such, that if the mucous membrane of the cheek, which runs to the gums, be divided, the labial orifice may be moved extensively,—so extensively that any reasonable manipulation may be effected on the jaw itself. I know that this limitation has been occasionally resorted to, but it has not been specially

referred to as an advantage. I am, however, convinced of this, that even in the removal of the largest tumours there is no necessity for the extensive incision referred to. It may be asked—What does that extensiveness imply? It may be only a quarter of or half an inch! and the question is just such as I desire to answer. Besides saving the lip entire, the principal blood-vessels —the labial artery and vein—are untouched; and so there is neither trouble as regards re-adjustment, nor hemorrhage, as when the lip is cut. If it be needful to cut vertically through the whole thickness of the bone, I fancy that the operation can be done by a lunated incision, just below the lower margin of the bone, with ends reaching upwards, almost as readily as if the mouth were opened at the lips,—the semilunar flap being so easily turned upwards, whilst the division of the mucous membrane will sufficiently relieve the cheek as to let saws and cutting forceps be applied to the bone. If a tumour involves much of the base, division of the facial or external maxillary artery is a necessity. It is from this vessel alone that severe hemorrhage is to be expected, but by tying it at once, or by judicious temporary pressure, there need be no fear on this point. When a ligature is used, I strongly advise that both ends of the vessel be tied, for in one instance I had great trouble from secondary bleeding from the upper end of the vessel eight or ten days after the operation.

But I am more anxious to refer to operations on the upper jaw than on the lower; for I fancy that I can say fully more that is original in regard to these than those on the lower. I am as strongly conservative here as in other parts of the body; and in this locality I use the term in a double sense, both because I think that parts may be retained which have generally been removed, and because I think that even the features may be more effectually preserved by certain steps than by others.

The modern idea with regard to the removal of tumours in the upper jaw has been associated with excision of the whole of that bone; and the operations of Gensoul, Lizars, Syme, and others, who were the earliest advocates of this proceeding, seemed to imply the absolute necessity of removing the whole of it. The essence of the operation, so to call it, consisted in isolating and cutting through parts of the extreme points or circumference of the bone —even the sacrifice of the malar bone by dividing the zygoma. The round bulging part of the bone behind at the pterygo-maxillary fissure, the orbital plate, its margin or whole extent, the nasal surface, and the palatine plate, were all marked out for removal in excision of the upper jaw. Now, whilst not inclined to call in question the propriety of what was done and advocated by these early operators, I fancy that a better style of surgery has made such sweeping proceedings scarcely needful. It does not appear that much was done in former times for the removal of such growths. A few rare cases have been recorded on which operations were performed; but such proceedings were far between, and had no position in the roll of our operations. So when modern surgeons began the excisions now so extensively recognised, tumours of a large size were more frequently met with than in the present time; and hence, perhaps, the necessity of reaching those outside points that I have just referred to. But in recent days the surgeon interferes at an earlier date, and before a tumour has implicated the bone extensively. It is in such instances that I believe there is room for improvement both in diagnosis and practice, and it is here that I make so bold as to propose that which I conceive to be different from ordinary accredited proceedings.

In operations on this bone, as on the lower jaw, and as with bones in other parts of the body, I take the liberty to protest against the doctrine that the whole bone must be taken away when there is tumour present. Indeed it is largely in consequence of what I have seen in the maxillæ that I have come to the practical conclusion that total excision is not always needful in the case of tumours.

Again I express my conviction that, in removing diseases of the upper jaw by the extensive separations referred to, the modern surgeon has been amply justified; but I feel equally confident that in many cases there is, or has been, no need for such destructive work. The malar bone, for example (separated,

be it observed, by an articulation from the maxilla, and therefore not within the scope of certain so-called physiological or anatomical laws), has often been removed, although there has been no trace of disease upon it; but all for the sake of making sure of the total extirpation of the jaw-bone. Now, I make bold to say that all this destructive surgery may in many instances be avoided, and that partial excision will prove, on the whole, as effective here as I am convinced it does in other parts. In certain instances the malar bone may be saved; in others the surface next the pterygo-maxillary fissure need not be interfered with; in many the floor of the orbit need not be touched, nor the nasal surface, nor the palatine plate. Much regarding these views will depend on the individual character of the disease; but of this I am convinced, that in the majority of such cases one or other of these parts may be saved. It is scarcely possible to overrate the advantage of saving one or more of them, nor do I doubt that a general feeling must be on my side in this opinion. But the question may be asked, How can such parts be saved? My answer is, to let them alone when they do not seem to be involved. But again, it may be asked, How can you let them alone if division is to be effected at such extreme points as the zygoma, at the outer side of the orbit, the apex of the nasal process, the junction with the other maxilla in the mesial line at the alveoli below the septum, along the roof of the mouth, and also at the palatine junction with the palate bone? Hard questions, seemingly! yet, in my opinion, as easily answered as anything associated with conservative surgery. Just take away that which is in disease, and leave that untouched which is in health.

But, supposing the doctrine which I advocate admitted, how can this be

FIG. 1.

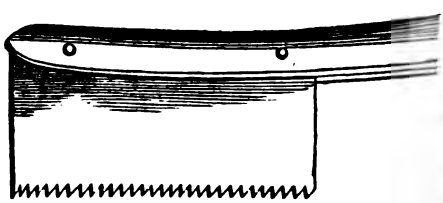


FIG. 4.

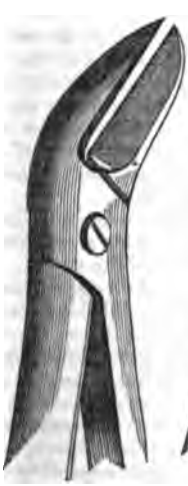


FIG. 2.

FIG. 3.

done effectually? And here I come to one of the chief objects of this lecture. My view is, that instead of attacking disease of the upper jaw at its circumference, as has been the almost invariable practice since Gensoul's proposal to excise the whole bone, it is better to get into the disease as it were, and cut from the centre to the circumference, making sure that in doing so, that circumference shall be thoroughly encompassed. But where, it may be asked, are the instruments to do this? It was an old practice with the chisel and mallet, but it did not answer! These are the instruments (Figs. 1, 2, 3, and 4), small saw, bent forceps, and gouge. The saw (Fig. 1) commonly called Hey's, these bent forceps (Figs. 2 and 3), and the gouge (Fig. 4), are almost essential to the process I am now referring to. The saw may be dispensed with, but the forceps and gouge are well-nigh absolutely requisite. My opinion is, that in such operations, if the great mass can be removed by adequate cuttings at convenient parts, any remaining portions may be readily clipped away by such forceps as these, or scooped out with the gouge; the rule being to clip or gouge until healthy surfaces are reached; and if this cannot be done, for fear of going too deep, then the surgeon may conclude that the disease has gone further than he calculated, and that it is beyond the reach of operation. By such a course as I now have indicated, I have frequently cleared the upper jaw of its diseased deposits, and left both orbital and palatine plates entire, and, as a matter of course, the posterior surface. The nasal side of the bone I have never taken much into account in these cases, for I think it of little consequence compared with the intention and magnitude of the operation.

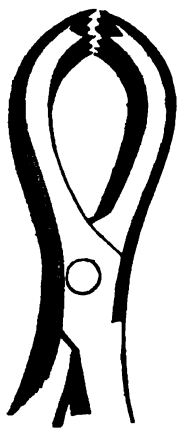
After all my experience and repeated trials, I have latterly formed a strong opinion that the features of the face may be better preserved than as yet by the generality of surgeons; and my anxiety to impress these views is certainly not the least object of this lecture. First, I consider that many tumours of the upper jaw may be summarily removed without cutting the lips or cheek at all; and next, should more space be needful, it may be gained at less cost of feature than has generally been supposed. In dealing with the upper lip for removal of tumours of the upper jaw, I greatly object to any other incision than one in the mesial line, which must be run into one, or both nostrils if required. There are two advantages of great importance, in my opinion, associated with this incision. First, the wound being made in the furrow below the columna, and exactly in the middle of the lip, is less observable than on any other part; and next, there is an inch in length gained by the natural opening of the nostril. The ala of the nose is so easily raised, and with the tip can be so easily moved according to the will and wish of the operator, and the cheek can be so readily dissected of the tumour as high as the margin of the orbit and as far out as the malar bone, that a large space for operation on the anterior surface of the maxilla is easily made. Since 1848 I have never made any other incision in the upper lip; and I have no hesitation in stating, from experience of twenty cases or more, that in a number of operations of moderate-sized tumours there is no need for more extensive incisions on the outer surface of the face.

Should it so happen that, the tumours being large, more room is required, I am further led from my experience to prefer an incision alongside the nose, and a horizontal line, to those of Gensoul or Lizars on the cheek. By these incisions through the lip, up the side of the nose, and along the lower eyelid, as far out as may be needful, say even to the zygoma, all the room required for the removal of a large tumour may be secured, and the most conspicuous part of the cheek may be left untouched. Another great advantage which I claim for these incisions is that the chief vessels of the surface are all divided at their narrowest points, and thus hemorrhage is less severe than when the facial artery is divided in the middle of the cheek, as in the common incision.

I am unable from want of time to dwell longer on this subject; but, before concluding, I must state my impression that the views regarding operations on the upper jaw, to which I have now drawn attention, and which, whether for good or for evil, I must claim as my own, cannot be carried out excepting by the use of such instruments as are here shown (Figs. 1, 2, 3, 4). The

Hey's saw and the gouge were familiar to surgeons before my time. The straight cutting forceps, depicted by Scultetus in former years, and brought into fashion in modern surgery by Liston, are of limited use in such instances; and where prejudice or ignorance does not prevail, they may be said to be entirely set aside by these angular ones (Fig. 2), which effect all, and even more than the straight ones. But in particular these semicircular clippers (Fig. 3) will be invaluable; and, with curves of different circles, the largest tumours may be circumscribed by them. If even some of the tumour should not come away with the mass, the blades will enable the surgeon to remove the whole, and clip upon the healthy surface.

FIG. 5.



Whilst recommending these angular and semicircular forceps, which I claim to have originated for surgical purposes, I cannot omit referring to these additional blades (Fig. 5) as being of the greatest imaginable value in the removal of some tumours in the upper jaw. A quarter of a century ago, in imitation of an older custom, small hook-beaked blades were used for seizing arteries on stumps and other open surfaces, when ligatures were required. They were likewise of use in seizing and holding small tumours during removal. It was Mr. Liston who, I believe, gave them the name of the bulldog forceps. In my earlier experience in operations on tumours of the upper jaw, I had observed the difficulty of separating them, even after a free use of saw and forceps; and this instrument was a device of my own to facilitate that step. The commanding size and strength of these blades give facilities for wrenching out a tumour of the upper maxilla previously unknown.. Their grasp is such that, in comparison with the others, I call them the lion forceps; and they are now well known under that name, although not so well by my own, for it is not long ago that a London hospital surgeon, who, being provided with one of them for an operation he was about to perform, asked me if I had ever seen the instrument, and kindly directed my attention to it as one of peculiar merit! In operations on the lower jaw it will be found of wonderful service in keeping all steady during the application of the saw, or in disarticulating; and in various operations elsewhere, where a stronger catch than the fingers can give is required, their use in my hands has added largely to the value of my digital resources.

In the preparation of this lecture I had sketched the chief features of diseases associated with the jaws, and particularly those associated with the antrum; but I found it impossible to keep all within the appropriate limits, and, being anxious during such an opportunity as this to state my views and practice when operations are required in such cases, I have thought it best to omit that which in my estimation had less of the aspect of novelty than the portion which I have now submitted to your notice. You may have observed that here, as in certain other departments of pathology and operative surgery, my mind takes a strong local bias, and I advocate what some may think or call restricted measures, in preference to those of a more sweeping kind, which involve the destruction and loss of a considerable amount of sound substance around. I trust that I shall not be misunderstood here. I advocate the removal of all disease when an operation is really undertaken for tumours. If there is no malignancy, there is, according to rule, no need for taking away more than the disease. If a sound surface is left, that is all that the surgeon need care about. If the disease be malignant, I have great doubt if cutting widely beyond it makes much, if any, difference as to its return; and this pathology I apply particularly to malignant tumours in bone, for very generally, when there is a return, it shows in the soft parts more than in bone. Of course I willingly admit that every now and then the renewed mischief really does come in the bone; but that feature should, in my opinion, contribute to form the exception rather than the rule for such operations as we have

had under consideration to-day. In addition, and in some degree to give reasons why I advocate conservatism in their performance, I may state that I entertain views as to the original and early seat of many of these tumours about the antrum which are in some respects peculiar. Most surgeons, I believe, have an impression that when a person is said to have a tumour in the upper jaw, or, to be even more precise, in the antrum, the whole of this cavity, with its walls, is so involved, that in any operation for removal there is an absolute necessity that this circumference—the walls of that cavity—must be taken away. Now, it is my opinion that disease in the antrum, beginning in the cavity as it were, is much more rare than most people think. My impression is, that in many, if not most, instances the wall of the antrum is the part first affected, and that its cavity is gradually filled by the growth, and then perhaps expanded. Actually, in some instances, so strictly local is the disease that the antrum may be in a manner displaced and compressed, whilst its mucous lining remains without indication of disease. This I have particularly noticed in tumours which have had their origin in the alveoli; and I have seen frequently growths of considerable size here which have projected forwards, downwards into the mouth, and even upwards, and yet have left the antrum scarcely if at all involved. Any part of the circumference of the antrum may be the original seat of a tumour, and if such tumour be attacked by an operation at an early date, I maintain that it may be removed, and the rest of the antrum or jaw be left. If it so happens that the tumour is chiefly associated with the posterior wall or part of the antrum, the diagnosis will be more difficult, and if an operation is performed the destruction of comparatively sound parts in front must be extensive in proportion; but if the tumour happens to be at any other surface, it is in my opinion of great importance to reserve as much as possible by removing only the offending part. It is unfortunately too often needful, owing to the extensive development of the disease, to remove all between the tongue and the eyeball; but cases are frequently met with of a more limited extent, and if the surgeon follows the practice which I have ventured to characterize as a modern improvement—viz, to interfere at a reasonably early time, so as to arrest the tumour in its onward progress, he may remove the disease, and yet save the greater part of the upper maxilla. In one case the sacrifice of only the inner or nasal wall of the antrum will suffice; in another the front may alone require removal; again, probably the floor of the orbit or roof of the antrum may be involved, or possibly only the lower part of the antrum—that is, the roof of the mouth, with, perhaps, the alveoli. To save the floor of the orbit, as happily may often be done, is in my opinion of great importance; but of all these local operations that I am now advocating, that of removing disease, and at the same time preserving the roof of the mouth, is the one of most importance in my estimation, and, where the extent of disease will permit, the surgeon should make every effort to do so. I have known a sound alveolar ridge, a perfect set of teeth, and one side of the roof of the mouth all sacrificed to get at a comparatively small tumour in the antrum, which could readily have been removed with the sacrifice of only the front wall of the cavity.

But time tells that I must cease, and I shall do so after a few words more.

The dread of hemorrhage was great in the early days of these operations, particularly when the upper jaw was affected, and it was the custom for a time to begin by tying the common carotid; but that practice was soon given up. Vigour and rapidity of action are the best safeguards against this danger.

Every now and then one hears objections to chloroform in such operations. For my part I have none. I have used it invariably since the introduction of anæsthesia, and I have never had reason to trace evil to it. It has seemed to me a greater boon to a patient in such an operation than in almost any other, for there are few more severe or frightful in the whole range of surgery.

In offering these observations, I speak from a personal experience of between thirty and forty cases. Twenty of them have been performed in King's College Hospital. I did not scruple in my lecture on Lithotomy to call the fatality of that proceeding in my own hands; nor need I hesitate here

to remark upon the seeming comparative immunity to life of operations on the jaws. There have been returns of disease and ultimate deaths, as with cancers and fibro-recurrent tumours elsewhere; but of the whole list of my cases I have lost only five—a success which, as compared with lithotomy, seems remarkable, particularly when we take into consideration the huge wound in the face as contrasted with the limited incisions in the pelvis.—*Lancet, July* 1, 1865, *p.* 1.

39.—ON TREPHINING FOR COMPRESSION OF THE BRAIN, FROM FRACTURE OF THE SKULL, WITH REMARKS.

By Bamlet W. Switzer, Esq., Assistant-Surgeon Bengal Medical Service.

It must sometimes be a perplexing thing to a surgeon when he purposes to trepan to know where he ought to make his incisions in an unwounded scalp, with nothing external to guide him, and the whole scalp so swollen as to render manual examination useless. From a number of cases of this kind which I have seen, I may safely say that in nine out of twelve the left parietal is the fractured bone. A man striking another with a stick holds it in his right hand; it is, therefore, brought down on the left side of his antagonist facing him—the reverse, of course, if the blow is given from behind. In these cases the bone generally gives way where struck. Fracture by *contre coup*, I do not think, takes place except by a fall. Cases are recorded of men falling on the head, fracturing the skull opposite to the place of contact, or fracturing the base; but I do not remember to have seen any case of *contre coup* fracture from a direct blow of stick or stone. The rule I would therefore deduce is that, in the absence of any symptom or sign to guide the surgeon, the history of the case would point out the practice. In the case of two men fighting with sticks, the fracture is to be sought on the left side; if it is a man found insensible on the road, and who, most likely, has been struck from behind, I would try the right side. Hemiplegia is in general a safe guide, but should never be one when the fracture can be felt. Trephining at the fracture is the practice, even when the paralysis is at the same side, for the compression causing it is, doubtless, blood, that a free opening into the skull, even at a little distance from it, will evacuate. It must have been a very small part, indeed, of the clot on this man's dura mater that I was able to remove, but the prising up of the bone took the pressure off immediately. The large amount of blood this man lost was most salutary; no sign of inflammation occurred through his progress to recovery. Except for the first few days, when he was on tartar emetic mixture (carefully watched to prevent emesis) and an occasional purgative he required no further treatment. One cause of the non-success of trephining, I believe, is its being delayed too long: pressure on the brain for a few hours is productive of far more mischief than an early operation is likely to cause; besides, the patient is better able for it early than late. I need not digress on the operation itself; it is exceedingly simple and easy of performance. Tetanus is said to have resulted from jagging the dura mater with the saw, but that can be readily avoided by plumbing the depth of the cut by a pointed quill, and cutting with a light hand when through the diploe. I would always encourage bleeding from within the skull when not coming *per saltum*. Nothing tends more to relieve the brain or prevent after bad consequences. Even had the operation eventually proved fatal from secondary causes, it might in many cases be of great present use in restoring temporary consciousness, and thereby allowing a man to settle his worldly affairs or prepare for the last great change.—*Medical Times and Gazette, Aug.* 26, 1865, *p.* 220.

40.—ON ACUPRESSURE.

By Dr. William Pirrie, Professor of Surgery in the University of Aberdeen; Surgeon to the Royal Infirmary of Aberdeen.

[Professor Pirrie's experience of acupressure as a hæmostatic agent, comprehends its use in five amputations in the middle third of the thigh, one

amputation of the leg below the knee, two cases of removal of the mamma, and one of excision of the elbow joint; also several cases in private practice.]

Before giving a brief record of my experience of this hæmostatic agent, it may be advisable to describe very shortly the principal methods of acupressure hitherto employed. They are seven in number.

The first method, to use the words of Professor Simpson, "consists in passing a needle through the flaps or sides of the wound, so as to cross over and compress the mouth of the bleeding artery or its tube, just in the same way as, in fastening a flower in the lapelle of our coat, we cross over and compress the stalk of it with the pin which fixes it, and with this view push the pin twice through the lapelle. The only portion of the needle which is left exposed internally on the fresh surface of the wound is the middle portion of it, which bridges over and compresses the arterial tube at its bleeding mouth, or a line or two or more on the cardiac side of it. And if it were a matter of any moment, this part need not always be left bare, for the needle could be often passed a few lines higher up, between the vessel and the cut surface, and without emerging on that surface. More or less of both extremities of the needle—viz., its head and point, are exposed externally on the cutaneous surface of the side or flap of the wound. When passing the needle in this method, the surgeon usually places the point of his left forefinger or of his thumb upon the mouth of the bleeding vessel, and with his right hand introduces the needle from the cutaneous surface, and passes it right through the whole thickness of the flap till its point projects for a couple of lines or so from the surface of the wound, a little to the right side of the tube of the vessel. Then, by forcibly inclining the head of the needle towards his right, he brings the projecting portion of its point firmly down upon the site of the vessel, and after seeing that it thus quite shuts the artery, he makes it re-enter the flap as near as possible to the left side of the vessel, and pushes on the needle till its point comes out again at the cutaneous surface. In this mode we use the cutaneous walls and component substance of the flap as a resisting medium, against which we compress and close the arterial tube. But in some wounds a neighbouring bone or other firm unyielding texture forms the best and readiest point of resistance against which to pin and compress the artery by the acupressure needle."

The second method consists in inserting the needle in the fresh surface at a little distance from the vessel, pushing it on, causing its point to rise up as near to the artery as possible, bridging over and compressing it, dipping the point of the needle into the raw surface of the wound on the other side of the vessel, forcing it on, and causing the needle to emerge a second time on the wound. The needle is threaded with a passive iron wire, by which it can be easily withdrawn.

The third method consists in entering the needle on one side of the artery, pushing it behind, causing its point to emerge on the opposite side of the vessel, passing a loop of inelastic iron wire over its point, bringing the wire over the track of the artery and behind the stem of the eye-end of the needle, drawing it sufficiently to close the vessel, and fixing it by a twist or half a twist around the needle. The wire with which the needle is threaded should be twisted that it may be readily distinguished. By means of this twisted wire the needle can be pulled out, after which the loop of wire is liberated, and can be easily withdrawn.

The fourth method, or that by a long pin and a loop of passive iron wire, is a modification of the third, and differs from it only in a long pin, with a glass head, for facilitating the insertion, being substituted for the common sewing needle threaded with iron wire. Perhaps of all methods the third and fourth are the most secure. The principle in each is the same, but I like the modification of using long pins when convenient from the form of the wound, as they can be so quickly introduced, so readily withdrawn, and all wriggling and entanglement of different kinds of wires with each other avoided.

The fifth method, or that by the twist, may be varied according to the extent

of rotation of the needle, whether to a-half or quarter rotation. The operator has, on the cessation of bleeding, a reliable proof that a sufficient degree of rotation has been given to the needle. This method may be practised with a long pin or with a threaded sewing-needle, and with either it can be very quickly done, but of all methods of acupressure that by the twist with a long pin is the quickest.

In acupressure by the twist to the extent of a half rotation of the needle, the three first movements given to the needle are precisely the same as in the third method above described, namely, it is entered on one side, pushed behind the artery, and its point is made to emerge on the opposite side. The needle is then twisted over the artery and fixed on the parts beyond. In this method the artery is, to a certain degree, both twisted and compressed. The first time I tried this method by the twist, a half rotation was given to the needle; but as so little pressure, when direct, is sufficient to arrest hemorrhage, in other cases a quarter rotation was only made by it. The needle was sent by the side of the artery, a quarter rotation was given to it, and then secured by sending its point into the soft parts. This is the simplest, quickest, and easiest of all the methods by the twist, more especially when done with a long pin, and, as far as experience in the Aberdeen Hospital warrants opinion, perfectly efficient. The gentlemen who were present in the theatre of the Hospital at the first operation where I tried acupressure by the twist were particularly struck with the great simplicity of this method as well as the facility with which it could be employed; but as the manœuvre with the small needle could not be distinctly seen from a distance, they were curious to see, after the removal of the patient, a demonstration I gave of this mode, in which I used a large needle and the mouth of the femoral artery in the amputated limb.

Professor Simpson used this method early in the history of acupressure in cases operated on by Dr. Handyside and Mr. Edwards, but he had published no account of it when the same method occurred as a perfectly original idea to the mind of Dr. Knowles, formerly House-Surgeon of the Aberdeen Hospital, who suggested this method without the knowledge that it had been devised by Professor Simpson, and I felt anxious to give it a trial. I did so in the Aberdeen Hospital on June 29, 1864, in an amputation of the thigh. This was the first instance in which the femoral artery was secured in this way, and the first time this method had ever been tried. It is no part of the fifth, or Aberdeen method by the twist to transfix the artery; on the contrary, great care is taken to avoid doing so. It is important to be aware of this circumstance, that there may be no confusion between this method and the sixth, in which the artery is transfixed. In practising the Aberdeen method by the twist, the first movement of the needle may be made to consist of its insertion by the side or underneath the artery, as may be most convenient for having its head towards the edge of the wound when the quarter rotation has been made.—*Med. Times and Gazette, July* 1, 1865, *p.* 5.

41.—CASES IN WHICH, AFTER AMPUTATION, ACUPRESSURE WAS EMPLOYED FOR THE ARREST OF ARTERIAL HEMORRHAGE IN THE ABERDEEN ROYAL INFIRMARY.

By Dr. William Keith, Lecturer on Clinical Surgery and Senior Surgeon to the Royal Infirmary, Aberdeen.

Case 1.—*Leg—Flap Operation.*—William F., aged 14. Diseased ankle-joint. On January 6, 1864, amputated the leg at mid-length; three needles employed, looped over. Two of the needles were removed at the end of forty-eight hours, and the third one at the end of seventy-two hours. No oozing after the operation, and no hemorrhage at or after the removal of the needles. The wound was closed by wire sutures, and healed by first intention.

Case 2.—*Thigh—Flap Operation.*—Mrs. M., aged 29. Diseased knee-

joint of three years' standing. On November 16, 1864, amputation at the middle of the thigh by the flap operation. Four long bead-headed shawl-pins with wire loops were employed for securing the larger vessels, and two small needles for muscular branches. Wound closed by wire stitches. No hemorrhage followed; the after ooze seemed less than usual. Seventy-two hours after, the two small needles were removed; at the end of other twenty-four hours, three of the long pins were withdrawn; and, lastly, the long pin controlling the femoral artery was taken away one hundred and twenty hours after the operation. Not a drop of blood followed the removal of any of the pins. The wire sutures, causing no irritation, were not removed till December 4, eighteen days after insertion, by which time the wound was whole. It adds interest to this case to state that it was announced beforehand that acupressure was to be put on trial, and the case was consequently closely watched from its commencement to its close.

Case 3.—*Forearm—Flap Operation.*—Peter P., seaman, aged 54, recurrent fibrous tumour, extending from the palm of the hand up into the forearm. On January 21, 1865, amputation performed by flap operation, six inches above the wrist joint. The bleeding from three vessels completely controlled by a long pin and one needle. Erysipelas, present in the ward at the time, affected the stump for a few days, but in no way hindered the removal of the needles or the union of the wound. In four weeks he went home to Shetland sound and whole.

Case 4.—*Leg—Flap Operation.*—Lauchlan G., aged 18, railway porter. Had his left foot crushed to pulp by the wheels of a loaded wagon having passed over it whilst lying on the metal rail. This happened at 1 p.m. on January 24, 1865; he was brought a distance of thirty-six miles, and arrived at the Royal Infirmary, Aberdeen, at 8 p.m. the same night in a very exhausted condition. Reaction established, by the end of sixteen hours after admission, the leg was amputated a little below the middle at 12 noon on January 25. Four arteries required to be stopped, and this was done by Simpson's pins with wire loops, and the wound was closed by wire sutures. On account of the molecular injury which the tissues forming the flaps were feared to have sustained (no retraction whatever taking place in the gastrocnemius and soleus muscles when cut), the stump was at once laid in a warm poultice.

January 26th. Stump a little swollen and red, the swelling extending up the thigh. The patient otherwise as well as could be expected. Continue the poultice.

27th. Stump and thigh considerably swollen. Continue the poultice. The four pins were this day removed forty-eight hours after insertion, and not a drop of blood followed. The patient's state satisfactory, but the pulse is today more frequent than hitherto.

28th, 12 noon. Stump looks well; swelling much relieved; patient's condition much as yesterday. At 8 p.m. he had a severe rigor, followed by great prostration. Pulse scarcely to be counted; intense thirst, and in a state of approaching coma. Mustard cataplasms to the nape of neck, and wine.

29th. Patient sensibly improved. Takes sustenance, but still drowsy, and feels very faint.

31st. In spite of every effort he sank at 11.30 a.m. this day.

Inspection of Body Twenty-four Hours after Death revealed the evidence of acute phlebitis in the saphena major vein of the injured limb; the coats of the vessel were thickened, and it was filled with a white, thin, sero-purulent looking fluid, not even tinged with blood. The deep-seated veins were all sound and healthy. The kidneys were greatly injected and friable. The stump was made the subject of very careful dissection. The flaps were easily separated; no coagulum of blood was in the wound; one red stain, the size of a four-penny piece, tinged the flesh at the end of the posterior tibial artery, and that seemed all the effusion of blood that had followed the closing up of the wound. The mouths of the anterior tibial, the peroneal, and the posterior tibial arteries lay open to view. Their ends were abrupt,

solid looking, not much retracted, nor in any great degree contracted, but filled up by organized fibrine or lymph, as solid to the look, and almost as much so to the feel, as the coats of the vessels. The popliteal and these three vessels were dissected out together, and showed, on trial, as perfect occlusion at the cut ends as I have ever witnessed. On slitting these vessels from above downwards the following observations were made:—The peroneal artery was firmly plugged at its cut end for one-fourth of an inch by a colourless, fibrous-like tissue indissolubly united to the lining arterial coat. No conical continuation of the plug extended farther up that vessel. The anterior tibial was in like manner completely occluded; the vessel, when slit up, revealed a short conical continuation of fibrine, pointing upwards, its further progress having no doubt been hindered by two small branches in close proximity passing off from the main vessel just above the tip of the clot. The posterior tibial was very effectually closed by a well-plugged mouth, and inside by a conical extension of the plug upwards to fully half-an-inch; the tissue composing it slightly tinged with blood, the plug and the cone being one and the same mass. The vasa vasorum had evidently maintained the vitality of the arterial coats, so as at once to commence the vital and healing process of exudation of lymph at the cut ends, all the more readily that the cellular attachments around had never been disturbed.

Case 5.—*Thigh—Flap Operation.*—March 6. George R., aged 12, parish of Newhills, labours under necrosis of the tibia of right leg; extensive abscess of the thigh surrounding the knee-joint and diagnosed to communicate with the joint; the boy greatly reduced in flesh and strength. The collections of pus were one after another freely emptied by incisions, and his strength recruited as much as possible by rest and generous diet, but it was the 3rd of May before matters were quiet enough about the limb to admit of amputation with any chance of success.

At 12 noon of May 3 the limb was removed at mid-thigh, under chloroform, and five arteries were speedily pinned under and looped over by a wire loop. The wound was closed by wire stitches; no dressing applied; not an ooze has followed the closure of wound. The pinheads varied in colour so that each vessel was individualised by the protruding pinhead, while the length of each pin selected was regulated by the distance of the artery from the margin of the wound, the head visible outside, the free point for steadiness being stuck into any adjoining muscle. At 8 p.m. the same day not a drop of blood had exuded from the wound, although it was then discovered that he had drawn out three of the pins and one of those securing the femoral.

May 5. Wound adherent throughout; not an ooze of blood from the wound, or even serum; boy in good spirits. To have meat diet, and four ounces of wine daily.

7th. The remaining two pins were removed to-day, Some pus followed the withdrawal of each, but not a tinge of blood. Had considerable headache yesterday, which was removed by free action of the bowels.

8th. Going on favourably, but very weak. To be allowed 6 oz. of wine daily.

18th. Last of stitches removed yesterday; wound united with little suppuration.

24th. Stump quite whole. Boy is up every day.

June 20th. The stump has been whole for a long time; but he has been kept in the house on account of his exhausted condition. Discharged to-day quite cured, forty-eight days after the amputation.

Case 6.—*Arm—Circular Operation.*—G. W., boiler-maker, aged 45, was admitted on June 8 with lacerated arm. About 10 o'clock the same day, while working at a foundry, he was caught by a belt and carried round a revolving shaft near the ceiling, between which and the shaft he was pinned. The machinery was immediately stopped and the man extricated. The only injured part was the left arm, which was almost torn through at two places. The flexor muscles of the forearm were severely lacerated, the ulna and radius fractured, and the broken ends of the bones protruded through the tissues. At the elbow there was similar laceration of the soft parts for some

distance above and below the joint, and the forearm was completely torn away from the humerus. Amputation by the circular method was performed about six inches below the acromion process. Six arteries bled. The brachial was secured from outside the skin by Simpson's first method, using simply the long pin. Two others of some size were secured by the third method, that of needle and loop, and three lesser by the needle twist, or Aberdeen method, known as the fifth mode. The stump was left exposed.

On the 11th the stump was tumid, and a livid patch appeared on the front. Four of the needles were removed. He was ordered 8 oz. of wine.

On the 12th the pin compressing the brachial artery was removed.

On the 14th all the stitches were removed; the stump was still red, but less tense, and was discharging pus freely.

On the 18th the slough which had formed separated, and was taken away; the sixth pin, the head of which had been concealed by the tumefaction of the stump, was removed; he continued to progress favourably, and on July 15 was dismissed, sound and whole.

Case 7.—Arm—Flap Operation.—John G., aged 29, labourer, admitted July 6, 1865, for a crushed elbow. About 11 o'clock on the same forenoon, while carrying a large stone on his back, his shirt became so tightened about his neck, in consequence of the stone dragging it down, that he fell down in a state of insensibility, and the stone fell on the left elbow. The elbow was so crushed that amputation by the flap operation, about nine inches below the acromion, was resorted to. Four arteries were acupressed—the brachial by the pin and loop and other three by the twisted needle. The stump was left exposed. No blood oozed from the wound. On the 8th one needle was removed without a drop of blood, and on the 9th another, followed by three drops of blood. On the 10th all the stitches were removed, and on the 11th the pin controlling the brachial artery, with the loss of two drops of blood. The wound suppurated a little, but he went on favourably, and on August 3 was dismissed cured.

Dr. Greig, of Dundee, was the first surgeon who trusted entirely to acupressure in a case of amputation. On January 6, 1864, I followed his example in the operating theatre at Aberdeen on a leg; ligatures were ready in case of need, but no need occurred; the case speaks for itself. On the occurrence of the second case—amputation of a thigh—I gave previous notice to my clinical class and the hospital staff that I meant to put acupressure on trial and trust entirely to it. The result was most satisfactory and conclusive, and led to its adoption most cordially by my colleagues Drs. Pirrie and Fiddes. The former has spoken for himself, and for the latter I have his authority to say that his reliance on the practice is entire, but more especially on the Aberdeen mode known as Knowles' needle twist. In my third case—of forearm—the simplicity of needle pressure by Simpson's first mode was striking. Entering from without through the skin, under the radial, out through the skin, crossing a portion of skin, and passing under the ulnar artery with the same needle in an exactly similar way, left only the interosseous to be secured, which was readily effected by a small needle and the half twist, or fifth method.

The fourth case presents many features of interest. The injury sustained by the patient was severe; his journey was long and fatiguing, and he lost much blood by the way, all tending to place his life in jeopardy, and lessening surprise at his death in the end. Yet he was got into a fit enough state for operation, bore it well, and gave promise for some days of getting through. He survived till the eighth day, affording ample time for the complete occlusion of the arteries—four in number—cut and secured at the operation, a long pin and wire loop (according to Simpson's fourth method) having been employed for each artery; and at the end of forty-eight hours these were all removed, not a drop of blood following them or afterwards. The vital and effective closure of the vessels during life was demonstrated by the dissection. The process of lymph plugging, so familiar to all who have followed Jones through his experiments on the arrest of hemorrhage, is well seen in the preparation made of the vessels of the leg in Case 4.

In the fifth case (amputation of the thigh in a boy aged 12) an occurrence happened not at all desirable, but still instructive. The restless boy, amusing himself with the pin heads protruding at the angles of the wound, had, before four hours had elapsed, withdrawn three of them; yet no hemorrhage took place. I applied no substitute, and yet all did well. It would, therefore, appear that four hours' pressure would suffice to effect coagulation sufficiently to shut any artery; for the femoral was one of those relieved of its supporting pin. Although no one will, I presume, advocate such an early removal of a useful means of support and control, yet it gives great strength to the affirmation that at the end of forty-eight hours the needles and wire loops may be safely and confidently removed. In the other cases, it need only be noticed that the method of needle pressure varied according to the position of the vessel—at times a long pin through the skin, over the vessel and out again, without a loop; at others, a short needle with a wire attached to withdraw it by, and the vessel looped over by a wire; and at times, again, the Aberdeen twist, which is truly a simple and secure mode of proceeding, and has this additional to recommend it, that it can be so easily withdrawn without disturbing the vessels or surrounding tissues in the very slightest degree. Whether a short needle with wire attached to its eye is employed, or a long bead-headed pin with which to secure the vessel by the twist, you have, on withdrawing it, only to pull gently in the line of direction in which the needle or pin already lies, assisting the pin with a slight rotation before beginning to draw; and as nothing really grips it, no difficulty is ever felt in the proceeding. The same can hardly be said when a wire loop has been employed. The loop to secure the vessel passes first over the pin point after the pin has passed under the artery, then the wire passing over the artery is made to embrace the pin stalk below its head, and that pretty securely too—all very right, and in some degree necessary; but when the hour comes for withdrawing the pin or needle, it is not found to come quite so easily away. Of course it comes, but with a little drag, and when the needle has been withdrawn, the wire loop has yet to come. As it had an eye at its further end, was bent a little over the vessel, and twisted not a little round the shank of the pin or needle, there remains a crooked wire to bring away after the pin or needle is out. It also comes away with a drag, usually paining the patient a little, and what I feel always most anxious about is, lest the wire should in its progress out drag the vessel so as to loosen its closing plug. No such thing has happened in any degree within my own experience or observation of others: yet I deem it fair to point attention to the circumstances that all will have to encounter, and I adduce it as one chief reason why my colleague, Dr. Fiddes, all but invariably gives the preference to Dr. Simpson's fifth method—viz., by the quarter or half twist without wire loop pressure at all. When the loop is employed, the objection I have just stated is most easily obviated by using a wire simply doubled without being at all twisted, and by undoing the closing twist fully before withdrawing the needle. In all cases reference should be had in the selection of pins and needles to the size of the limb about to be operated on, so that when the pin head is outside the flaps, the point may in no case protrude at the other side, and if the point shows, better clip it off. While inserting the needles, it is also advisable to regulate so that all may present at one or other angle of the wound (this is not absolutely necessary, but will be found convenient), and that a knowledge of each vessel may be distinctly preserved, pins with different colored heads might be employed, or, if needles, silver or iron wire attached as required.

A little tact is no doubt necessary in introducing the pins or needles, whether through the skin or only through flesh and cellular tissue, but both are greatly facilitated in their movements by being provided with spear points. In penetrating from without, the index finger of the left hand laid over the vessel on the cut sides guides the pin point with great precision, at the same time preventing any flow of blood, and in passing a needle behind an artery of any great size on the cut surface of a flap it will be found very useful if an assistant would seize the mouth of the vessel and draw it a little out, steadying it while the operator passes the needle under it and while he

applies and secures the wire loop over it. By this mode the proceeding becomes momentary indeed; much or little tissue may be allowed to intervene between the coats of the artery, and the surface of the pin or needle employed as the operator pleases. In the first method the artery should be crossed by the needle at a point where its sheath is entire, hindering, therefore, contact between the two; in the second, third, and fourth methods not only should the same be allowed, but a moderate portion of muscular fibre or cellular tissue will be usefully included to act as a cushion while compressing the vessel. In the fifth method less, but still a portion of the adjoining tissue should be entangled in the twist along with the artery, and the twist secured entirely by plunging the needle point deeply into the firmest soft tissue within reach, always providing by the lie of the needle for its safe and easy withdrawal when desired. It is unnecessary to repeat that inelastic wire is now exclusively used to close all amputation wounds; the stitches are allowed to remain in until the wound is healed, and no dressing is applied whatever.

The pins and needles employed should be soft-tempered, so as to bend rather than break, should be flat, sharp, spear-pointed, round in the stem, and of various lengths. Mr. Young, the Edinburgh cutler, is familiar with their construction, and Mr. R. B. Turner, of Redditch, is highly commended by Dr. John Burton of Walsall. At a very moderate cost, Mr. Turner supplies the needles electro-plated with gold—a process that will, by preventing oxidation, preserve the polish and purity of the needle for future use, but what is of still more importance, will hinder any gumming between the wire and needle, and so facilitate the withdrawal of both.

We desire to suppress hemorrhage promptly, effectually, permanently, after every surgical operation. Acupressure is equal to do all this, and in every emergency. Having secured the bleeding vessels and allowed time for the surface of the wound to glaze, and every ooze of blood to cease, the wound is closed over an unirritating substance, so arranged as to be capable of being withdrawn at any moment whenever its work is deemed to have been accomplished, leaving a clean glazed wound already adherent face to face by forty-eight hours of undisturbed repose, and now relieved from the presence of any foreign matter whatever. What need hinder union by first intention? Yet we have been recently told by a surgeon of some eminence that this mode of treating wounds and wounded vessels is "a proposal that ought to be received with regret," and as the newest important suggestion in surgery, he enjoins the "leaving both ends of the silk ligatures with which the arteries in a stump have been secured protruding from the wound, to maintain an outlet for the discharge of fluids that might otherwise accumulate in the cavity." What cavity? The surfaces are carefully approximated with a view to healing by primary intention. What kind of fluids? All the vessels are closed, the ooze had ceased, and before any irritation is created the wire ligatures are withdrawn. I again ask, Where are the fluids to come from? To be sure, if after amputation of an adult thigh from five to eleven double silk ligatures are left attached, each pair to the strangulated end of an artery, and their ends traversing the length or breadth of the wound, as may be, and that for many days in succession—in truth, until time or chance liberate the ligature by the dead end of the vessel sloughing off—one need not wonder at what may follow in the way of morbid action or copious secretion either; but every unprejudiced person will see and acknowledge that the cause of all that is the presence of the silk ligatures.

In dealing with this question of needle pressure I am conscious of no thirst for innovation on the part of my colleagues or myself; and certainly we have not rushed into print with any undue haste; we calmly thought over it, coolly tried it fairly, and now, at the solicitation of others, give our unqualified approval of it as quite safe and quite sufficient for arresting arterial hemorrhage, having fewer after drawbacks than the mode by silk ligature. We do not need to condemn the use of silk thread more than do people condemn stage coaches where other conveyance cannot be had; but where a locomotive is available I suppose most people now-a-days would

prefer it, and so with silk thread and acupressure.—*Medical Times and Gazette, Sept.* 9, 1865, *p.* 278.

42.—ON THE EMPLOYMENT OF ACUPRESSURE AS A HÆMOSTATIC AGENT.

By Dr. PATRICK HERON WATSON, Surgeon to the Royal Infirmary and Chalmers' Hospital, Edinburgh.

[Dr. W. relates nine cases in which acupressure was used for the arrest of hemorrhage after operations.]

At first sight, looking at these cases, it is apparent that there are nine cases of acupressure, with three deaths; and according to this common method of estimating the success of certain great operative procedures, such a result would be tabulated as a fatality in cases of acupressure of 33 per cent. But as hardly the most vehement opponents of this means of checking hemorrhage are inclined to go the length of charging upon it such disastrous consequences, it is unnecessary for me to say more on this aspect of the fact, than merely to show that this fatality was in every instance independent of the surgical proceedings resorted to for the relief of urgent disease.

In the first case (that of a strangulated inguinal hernia) the form of the affection—congenital in the adult—the protrusion occurring into the tunica vaginalis testis as its sac, is sufficient to indicate the likelihood of a fatal issue, especially when we consider that the protrusion had taken place on this occasion for the first time, that the apertures were very rigid and but slightly dilated; that the hernia had been incarcerated for certainly eight hours, and strangulated for at least two; that at the time of the operation the sac contained no fluid, and that the bowel was tightly constricted in no less than three situations. Furthermore, the fatal peritonitis was in all probability already established before the operation for the relief of the hernia was undertaken, as turbid fluid escaped from the abdominal cavity after the contents of the sac had been returned.

The second fatal result, in the case of the railway guard, occurring after amputation of both limbs, consequent upon a severe railway injury, attended with great constitutional shock, a long transit with consequently some hours' delay after the accident, and considerable loss of blood, is nothing more than what is usually observed in such circumstances. To the question of the recurrence of hemorrhage after amputation, I shall again advert; but meanwhile I mention it merely to say, that its amount was so insignificant as to be incapable of seriously influencing the ultimate result.

In the last case which proved fatal, that, namely, of the farm-labourer with disease of the elbow-joint, and a large chronic abscess of the forearm and arm—to all appearance the determining cause of death must be found in the epileptic attack, first followed by deep coma, and then succeeded by a violent state of maniacal fury. It was during this period of deep stupor, followed by violence, that the bed-sore formed over the sacral region which originated the erysipelatous affection. That this was the difect cause of the pyæmic affection from which he died was rendered probable from the fact that, while there was no coagulum contained in any of the larger veins of the right arm, the right internal iliac vein was occupied by a coagulum originating in its posterior branches. When the epileptic convulsions, followed by stupor and then by mania, first appeared, I imagined either that this was due to some embolic cerebral lesion or to uræmic poisoning, No cerebal embolism, however, was found to exist in the post-mortem inspection; and the theory of uræmia, although so far borne out by a trace of albumen being discovered in the urine for a day or two, received no confirmation in the condition of the kidneys.

Passing from the consideration of these fatal cases, because, as I have already said, the unfortunate result is in no respect attributable to the employment of acupressure, instead of the ligature, I would wish to consider what bearing these cases have: 1st, Upon the question of acupressure as a

safe hæmostatic agent; 2nd, Upon the utility and application of acupressure as contrasted with ligature and other hæmostatics; and, 3rd, Upon the prospect it affords of expediting the process of healing by the first intention.

1. Is acupressure then a safe hæmostatic agent? This question, so far as my experience goes in its employment, I must answer in the affirmative. I should speak with submission, as no doubt I have never had any opportunity of employing it in any vessel larger than the posterior and anterior tibials, radial and ulnar arteries. My experience, however, such as it is, would lead me to have no hesitation in resorting to its use in the largest arteries which require to be secured in amputation, such as the axillary or femoral. Nay, such is my complete confidence in the trustworthy nature of this measure, that I should be inclined to employ some modification of it in preference to the ligature in the operations upon the continuity of arteries in the treatment of aneurism. Before leaving the hæmostatic capabilities of acupressure, I must say a word in regard to Cases Nos. 3 and 6, which have been alluded to in Mr. Syme's summary as examples of hemorrhage following the use of the needle.

In regard to Case 3—the amputation of the forearm—the bleeding from the flaps was easily stopped by the employment of needles, and when the surfaces were brought in contact there was nothing but a scanty oozing, such as must occur in every wound of similar extent. To have brought the edges and surfaces of the flaps in contact, however, while even this was going on, would not have facilitated union by the first intention, and, as to obtain this was my chief aim, I left the edges of the flaps gaping in the intervals, between two and three points of suture, which were inserted only for the purpose of equalizing the margins. From the surface of the flaps there was undoubtedly no further bleeding of any moment; but in passing the deep sutures through the flaps, some arterial blood flowed along the track of one of the wires when it was first introduced, and came, I am inclined to believe, from the radial artery, which had been injured in the passage of the long needle. As the flow ceased as soon as the sutures were tightened, and as the trickling stream was arrested, when I saw the patient after his removal to bed, as furthermore, my house-surgeon informs me all bleeding of any kind had entirely ceased some time before the hour of evening visit, and, as certainly no means were deemed necessary to arrest any oozing which did occur, as certainly Mr. Syme's house-surgeon only saw the case—not on account of bleeding—but to give the patient chloroform, while the additional sutures were inserted, I am compelled to conclude that there was no "after bleeding" of any consequence, certainly not more than is usually seen in cases of amputation, certainly nothing which needed interference, and assuredly that which did occur was altogether unconnected with the employment of acupressure.

In Case 6, there was bleeding repeated again and again from trifling vessels which did not bleed at the time of the operation, which were not previously secured by means of needles. This hemorrhage, therefore, was altogether independent, and unconnected with the use of acupressure. In the removal of one needle and its wire loop, some oozing did ensue, and in this case I certainly did re-introduce the needle and reapply the wire, not because this was absolutely required, but because the patient had already manifested such a proneness to bleed from any open surface that I deemed it expedient to secure myself and the patient against even the possibility of any further oozing. Had I deemed the needle insecure and unsatisfactory, it is scarcely likely in such circumstances that I should have re-employed it, instead of resorting to the ligature. But even supposing, in this example, copious hemorrhage had ensued when the needles were removed from the larger vessels, which was not the case, it still would not have prevented me in happier circumstances from resorting to the employment of acupressure; for the nature of the flaps, and the consequent impossibility of obtaining accurate co-aptation of the cut surfaces, rendered the probability of the occurrence of hemorrhage when the needles were removed, or while suppuration was going on, much greater than in the instance of amputation of the forearm, already commented upon. In fact, I regard the case as a very severe test of the safety of the needles as a

hæmostatic agency; and, as it stood this test so well, the dissection of the vessels exhibiting firmly adherent coagula, occupying their extremities, which required very considerable force to displace or to separate them from their attachment. I should be inclined to quote this case, not as an instance of the unsatisfactory nature of acupressure, so far as the arrest of bleeding is concerned, but as an instance of the trustworthy character of this procedure as a hæmostatic agency.

2. As to the applicability and utility of acupressure as contrasted with ligature and other hæmostatics. When acupressure was first proposed, the procedure consisted in introducing a long glass-headed needle from the cutaneous surface of the flap. This, after traversing the textures, compressed the orifice of the vessel, by passing across its track, and again emerging on the cutaneous surface. It, therefore, compressed the artery in a degree proportioned to the thickness and resiliency of the tissues, the directness of the ingress and egress of the needle, together with its own rigidity. Such a method was manifestly ill-suited for general application. Hence, I presume, it was that the statement was very generally accepted, that acupressure was limited in its application to a few situations. But modified as this agency has been in various different ways, I can conceive of no situation in the body where its employment cannot readily be resorted to. Certainly, of all these various methods, that is simplest in which a small-sized glover's needle, threaded with a loop of fine iron wire twisted into a single strand, or a common long hare-lip needle, is employed either to support a compressing loop of wire, or to obstruct the orifice of the vessel by rotation through the arc of a circle. There are situations, too, such as the extirpation of tumours, or in plastic operations on the face, where, while the dissection is proceeded with, the bleeding, arterial and venous, is readily checked by passing a common pin, or, better, a steel pin with a glass head, in at the bleeding orifice, till its head compresses the cut surface. I have in several examples employed this method, and have had much reason to be satisfied with it, because it certainly saved loss of blood, while it avoided the necessity for the fingers of assistants embarrassing the operator, by being employed either mediately or immediately in compressing the bleeding vessels. After completing the operation, these pins are removed, and should the vessels again bleed, one or other of the more permanent methods of acupressure may then be adopted. The needles are of course more readily employed in some situations than in others. In open exposed surfaces, rather than narrow deep wounds, in flap operations performed by transfixion, than in those by the circular method, or where the mouths of the vessels are cut even with the muscular tissues in which they are embedded. But even such circumstances offer no insuperable obstacle to their employment. The interosseous arteries in the forearm, the anterior and posterior tibials and peroneal in the amputation in the leg, certainly gave me no trouble in employing acupressure. There is one situation in which bleeding is sometimes troublesome, and where a needle seems to afford a satisfactory means of stanching the arterial flow. I mean when the hemorrhage obstinately comes from the nutrient artery of a bone, or from an unusually active branch of the vessel. In such circumstances a needle affords an easy means of stopping the bleeding, and may surely be admitted to be a very preferable hæmostatic to the actual cautery, a compress of lint, or a peg of wood attached to a ligature, all of which I have in such circumstances seen employed. The use of the needle, however, has appeared to me to be much more easy even in cases where the ligature can easily be employed. To use a ligature properly, the mouth of the bleeding vessel should be seized by the forceps or tenaculum, and secured by the ligature, including in its noose as little of the extraneous soft parts as possible. No such accuracy is required in the use of the needle in any of the methods recommended for its application; while, in acupressure, the degree of compression of the tissues is so slight, and its necessary duration so short, that no ill can arise from it, whatever be the nature of the textures included. I do not think, therefore, that difficulty of application is an objection to acupressure likely to arise in the mind of any one who has had an opportunity of employing it.

In the case of the ankle joint amputation (No. 4), it will be observed, that while the bleeding was very copious, it came from no vessel of such size as to admit of the satisfactory use of a ligature. Possibly it may be said that sponging cold water over the wound, or leaving the flap open for an hour or so, would have sufficed to stop any such bleeding. I cannot say that this has been my experience in similar cases, where the bleeding, though free, came from so many open points as to render the use of ligatures to all of them out of the question. Certain it is, that the needles afford a remarkably easy means of securing the satisfactory stoppage of this continuous and copious flow from a number of extensive patches of surface. Again in sloughing wounds or sores, and in cases of secondary hemorrhage, I think there will be little objection taken to the reasonable anticipation that acupressure will prove more satisfactory than ligature; and, again, in bleeding from the palm of the hand or sole of the foot, either in the form of acupressure or of filo-pressure, by means of a curved needle and wire suture, we possess a means of stopping the flow more easily applied than the ligature, and neither so painful nor so likely to cause suppuration as the graduated compress. As compared with torsion, the necessity for the removal of the needles may be urged as an objection to the use of acupressure; and no doubt it is, to a certain extent, a drawback, because, however gently effected, it must in some degree interfere with the accurate apposition of the surfaces which have been approximated. Should there, however, be any object to be gained by leaving the wound completely undisturbed, this might easily enough be effected, either by leaving the needles in till the wound is healed, or by taking care that in the introduction of the needles they shall lie so completely horizontal with the opposed surfaces of the wound, and so directly in the axis of the opening in the margin through which they are to be withdrawn, as to avoid any undue disturbance in their removal. In this manner, without force, the needles will admit of extraction at any time from a few hours up to days or weeks after the operation, as may be thought most expedient.

In the amputation of the forearm (Case 3), some of the needles were taken out early, some were left untouched till the sutures were removed, and in the latter instance they seemed to create no irritation of any kind, while from the side in which they were left undisturbed there was less discharge than from the other. In all the cases a very moderate amount of attention enabled the needles to be inserted in such an axis as to lay them in a position most suitable for easy removal. Without entering upon the question as to the immediate or remote effect of torsion upon the twisted end of the smaller arteries, there can be no doubt, in the case of vessels the size of those of the forearm or leg, that if the process of torsion is to be really efficacious in arresting all further bleeding, a considerable portion of the extremity of the vessel must be so injured as to ensure its dying and separating as a slough, while in those cases of my own, and others where I have had an opportunity of dissecting the parts after the employment of the needles, no slough whatever appeared to have formed.

The effect of the ligature in producing loss of vitality of the small portion of the vessel, and other soft parts beyond the embrace of its noose, is, I suppose, undeniable; and although it may be of small moment when the ligature remains in the wound till the separation of the little morsel of dead tissue is complete, and it has floated away in the discharge of matter which continues to find its way along the ligature, in the case of the twisted vessel it is far otherwise. Here the portion of vessel which had been injured by the torsion more nearly resembles in its condition that of the noose of a ligature which has had both ends cut off short after the deligation of the artery. In such cases, as is well known, should the wound seem to have healed kindly, a small abscess sooner or later forms in connexion with the foreign body, and, by its seeking the surface, breaks up the union which had already taken place. In considering the effects of torsion as contrasted with acupressure in small vessels, as affecting union by the first intention, we are apt, when torsion is applied, to be deceived by the fair union of the external surface of the wound, and to imagine that it is the same throughout its whole depth, and

thus to conclude that the extremities of the twisted vessels have healed kindly along with the rest of the cut surface. That there may be no sloughing of the extremities of these minute vessels after torsion, I am not prepared to deny, but that it is necessarily so because the union seems so perfect externally, I am not prepared to admit. In those very cases where the union by the first intention is most readily obtained, and is most perfect in appearance upon the surface, it is so just because there is another surface of the wound in which the union is much less perfect, and from which a ready escape is afforded for blood and serum in the first instance, and for more or less suppuration afterwards. The pus, then finding its way most readily in the direction of the deepest surface, does not interfere with the accuracy of union on the cutaneous surface. If, in such cases, torsion of the bleeding vessels has been employed, and sloughing of their extremities ensues, the small fragment of detached tissue escapes inwards along with the pus, and leaves the surface unaffected. The union, then, in such cases by the first intention is rather apparent than complete. The surface aspect of the wound does so heal, the deeper portion suppurates and granulates, I will not say always, but certainly very frequently.

3. The only remaining question to be determined is the influence of acupressure on the union of wounds in the cases described. In determining this result it is essential to understand the use of terms, for many differences of opinion in the matter of union by the first intention, union by adhesion, or primary union, are undoubtedly due to the varying significations attached to these terms. In strict application they should be reserved for wounds which heal without the formation of a particle of pus. From what I have just said under the previous head, it is plain that such union occurs but rarely, and though a part of a wound frequently heals in this manner, it is but a portion, not the whole, and that those instances which are most frequently quoted, as affording a pattern example in this way, are exactly the cases where the suppurating part of the process is least easily observed, and where the union by adhesion is most obvious. I do not quite go the length of some continental authors who deny *in toto* the existence of union by the first intention, except to a most limited extent in most favourable circumstances, but I am inclined to believe that the entire absence of all trace of suppuration has been too readily taken for granted. In the careless and incorrect use of pathological language, which is perhaps too common with us all, cases of plastic operations upon the lips and cheeks are usually referred to as examples of the most satisfactory union we can obtain when we have adhesion without the formation of a drop of matter. But while this is undoubtedly perfectly true as regards the cutaneous aspect of such wounds, it is, I am convinced from observation, not true as regards the mucous aspect of the wound from which matter does escape, but mingling with the saliva, and being swallowed, is apt to elude our notice. In the instance of any incised wound, where there is no great tension, nothing is more easy than to obtain a similar amount of primary union, if we bring the cutaneous edges together with equal care; but here, there being no second surface corresponding to the buccal cavity in wounds of the cheek and lips from which the pus may escape unperceived, the consequence is, that it is pent up within and forms an abscess. When, then, we wish to obtain union by the first intention to as great a degree as possible, in the case of large flesh wounds or amputations, our principal object, it appears to me, should be to regard the cutaneous surface as the secondary one which should be sacrificed to the accurate adhesion of the deeper parts, so that while they heal, from it the blood and serum, and afterwards the pus—should it form—may obtain a free escape. It was with this view that in Cases 2, 3, 5, and 9, I adopted measures calculated to obtain such results. The progress in these cases was certainly to my mind, and to that of all experienced observers who saw them along with me, very different from what I had ever previously obtained; and in my description of the progress of these cases my object has been not to set them in as favourable a light as possible, but to give the facts which might be described as unfavourable as great a prominence as possible. In these cases I have no

hesitation in saying that union occurred as thoroughly by the first intention as is ever seen in plastic operations upon the lips and cheeks, and certainly with an entire absence of anything that could be described as characteristic of the local or constitutional symptoms of inflammatory action. As to the other cases, where undoubtedly suppuration occurred, and in some copiously, the result need excite no surprise, as a few words will suffice to demonstrate.

In Case 1, of hernia, the parts operated on and cut through were acutely inflamed; nothing short of suppuration could therefore have been expected.

In Case 4, of amputation at the ankle, the hollow form of flap in this amputation does not admit of accurate apposition in any circumstances, and therefore, for that reason alone, suppuration must have taken place, but especially so in this instance was it likely to occur, where, from the extensive formation of matter in connexion with the disease of bone, a part, and that a considerable part, of the interior of the flap had already suppurated. The sloughing of a portion of the flap which ensued was where the soft parts had been undermined by the progress of the pus towards the surface during the advance of the necrosis.

In Case 6, amputation below the knee and at the ankle, suppuration occurred simply from the want of accurate apposition of the surfaces of the flaps with the parts with which they were brought in contact; this apposition being impossible from the site of amputation and the nature of the flaps. The sloughing of a portion of the flap in the amputation below the knee being manifestly due to its being involved in the original injury, although at the time of the amputation there was no symptom of any such extension of the effects of the accident as to render it justifiable to resort to amputation of the thigh rather than attempt to save the knee by operating on the leg.

In Case 7, excision of the elbow-joint, that suppuration after the operation should occur where a large abscess sac already existed in connexion with the disease of the articulation, and when the patient had, for some days, tossed the limb about during convulsive and maniacal seizures, is certainly not more than might have been anticipated, and may, I think, in such circumstances, find a more easy explanation than in the fact that needles were employed to check the bleeding.

In Case 8, suppuration certainly took place in a most limited area of the hiatus left by the removal of the cystic tumour of the breast, and was largely due to the want, on our part, of that accurate application of pressure to keep the flaps steadily fixed in their position upon the surface of the pectoral muscles, by which alone any hope of perfect primary union could be realized.

The sum of the whole matter, it seems to me, may be expressed in the following statements:—

1. Acupressure may be employed as a hæmostatic agency in the instance of vessels as large as the posterior tibials without risk, so far as bleeding, immediate, consecutive, or secondary, is concerned.

2. Acupressure is easy of application, while its adaptation to situation and circumstances is as great, or it may even be greater, than that of the ligature.

3. Acupressure is more trustworthy and satisfactory than either torsion, compression, or the use of the cautery.

4. Where primary union can take place, acupressure is likely to favour its occurrence; and, in three of the cases described, seemed of material service in this respect.

5. In the cases described, where suppuration or sloughing occurred, this was altogether independent of the use of acupressure, or the absence of the employment of the ligature' and referable to causes inherent in the individual cases.

6. In my opinion, the employment of acupressure in one or other of the methods suggested by Professor Simpson is safe, satisfactory, and well worthy of an extended trial.—*Edinburgh Medical Journal, July*, 1865, *p.* 45.

43.—ON ACUPRESSURE FOR CONTROLLING HEMORRHAGE.

By Professor RAWDON MACNAMARA.

(Abstract of a paper read before the Surgical Society of Ireland.)

[The patient, aged 40, had suffered for several years from a chronic abscess on the outer side of the leg, which continually discharged a thick offensive matter mixed with blood. The whole leg, when he came to the Meath Hospital, appeared to be one mass of disease. Numerous sinuses communicated with the carious tibia, behind which there was evidently a large abscess. The leg was removed through the condyles as recommended by Mr. Syme.]

Along with Mr. Syme's operation through the condyle, he had combined Professor Simpson's method of acupressure, which was an interesting circumstance in the present aspect of affairs. He found not the slightest difficulty in controlling the hemorrhage. The popliteal artery, the principal vessel to be secured, was of unusually large diameter. It was as large as the femoral artery. It was at once and most effectually controlled by the form of acupressure which he brought before the Society that day month, and which is known as the third method—recommended by Dr. Simpson—that is inserting the needle under the vessel, and then putting a loop of wire across it and turning it under the needle. It at once and in the most satisfactory manner controlled the popliteal artery. Four other vessels required acupressure, and scarcely any blood was lost. The operation was performed on Friday, and on the following Monday he would have removed the needles could he have done so that day in the presence of the students. Indeed he might have removed them safely on Sunday, but that being a *dies non* so far as the pupils were concerned, he delayed doing so until Tuesday, when, in the presence of the class and of his colleagues, he removed the needles, and there was not the slightest discharge of blood, nor had there been the slightest appearance of hemorrhage from that period to the present. He saw the man at two o'clock that day, and everything looked most promising for his recovery. He might be asked what did he gain by acupressure in these cases? He was not putting himself forward as an advocate for acupressure; his experience had been too limited on the subject to permit him to do so, and he was merely bringing before the Society the cases in which he had employed that method of controlling hemorrhage. That night fortnight he brought forward a case where he had removed a woman's breast. He stated that two hours after the operation he removed the three needles which he had used, and that two of the vessels were as perfectly sealed as ever vessels were, and the third vessel spouted in his face. He reapplied the acupressure, and in a couple of days afterwards he removed the third needle without the loss of a drop of blood. What did he gain in that case? All he could state was this, that the whole wound united by the first intention. It appeared to him that his patient would have gone back to Clare, from whence she came, within a week, but unfortunately she accidentally caught erysipelas from another patient, and the lower and upper portions of the incision suppurated slightly; the intermediate portion, however, remained firm. The whole breast had gone on most satisfactorily, and the patient had returned to the country within one month, the breast perfectly united. When he brought this case before the Society on a former occasion, he was asked why he did not remove the glands in the axilla, and he replied that he did not remove them because it was so decided on consultation, the opinion being entertained that they might have been enlarged from simple irritation and not from malignant action. The diagnosis and prognosis regarding the glands were borne out by the fact, that the woman had gone down to Clare without any enlarged glands in the axilla. In Donohoe's case, what did he gain by the adoption of acupressure? In an assemblage of surgeons he need not enter into a detail of this operation, and he would only say that a considerable part of the wound united by first intention. Where tendinous structure existed there was some sloughing, but he was so perfectly convinced of the merits of acupressure that he intended to employ it in two amputations which he anticipated having to perform next week. In a recent case of

extirpation of the testicle Mr. Porter had used acupressure. He drew the ligature round the cord on Simpson's method, and not a drop of blood was lost. One of the arguments used against acupressure was, that pressure on nerves would be attended with pain; yet this man had acupressure applied to the cord, and the whole of the hemorrhage arrested by that means, and he did not require any sedative after the operation. He complained of nothing to interfere with his comforts to any extent. He saw Mr. Porter withdraw the needle from the cord, and not a single drop of blood followed. He happened to be in Edinburgh some time ago, and was entertained at Professor Simpson's hospitable table. The Professor showed him on that occasion a nice model, which he had made to explain the system of acupressure. He (Mr. Macnamara) said to him—"There is no doubt you have carried out acupressure in a most practical manner, but allow me to tell you that that is simply the treatment of aneurism by compression, which originated in the City of Dublin." He referred Dr. Simpson to the "Medico-Chirurgical Proceedings," vol. xlii., in which would be found a paper on the subject by his distinguished master, Sir Philip Crampton. This was published in 1816, and it appeared that Sir Philip Crampton led the way for acupressure. He remembered when it was said that it was necessary for securing a vessel that the internal and middle coats should be divided by the ligature. He was addressing gentlemen who had been taught that, and who had taught it themselves to others; and yet so early as the year 1816 Sir Philip Crampton stated that it was perfectly unnecessary for the middle and internal coats to be divided; all that was required was to bring the walls of the vessels together, and adhesion would most probably be the result. He remembered distinctly going round the wards of the Meath Hospital with the late Surgeon Porter—a man whose name must always be mentioned with reverence in that place for his eminent services to surgery—and he had often heard him say, "Ah, that nasty ligature —what is it doing here?" He remembered being horrified at the manner in which Surgeon Porter would pull at the ligature, and yet he never saw a drop of blood follow its removal. Here was another man, distinguished for his services in respect to aneurism and surgery generally, who seemed to be on the right road to acupressure, and no doubt Sir Philip Crampton, who taught Mr. Porter, and Mr. Porter also, had laid the foundation for acupressure as a mode of controlling hemorrhage. He thought acupressure was so important a branch of the profession, and was likely so completely to revolutionize surgical manipulation, that every surgeon ought to bring forward the cases which had fallen into his hands—well if fortunate, more important still if unfortunate. If any unfortunate instance occurred in his practice he would not be less quick in bringing it forward than he had been with respect to the cases he had already brought under their notice. He thought acupressure deserved to be taken up by the Dublin surgeons, and subjected to the *experimentum crucis*. If it did one half of what Professor Simpson claimed for it, a great boon would be conferred on medical science. That could be only ascertained by testing it. They knew how the treatment of aneurism by compression was pooh-poohed on the other side of the water. Let them not follow a bad example and pooh-pooh acupressure, which I repeat, if it did only one half of what Dr. Simpson said it would effect, would be a great boon to operative surgery.—*Dublin Medical Press, June 7*, 1865, *p.* 533.

ORGANS OF CIRCULATION.

44.—ANEURISM OF THE POPLITEAL ARTERY CURED BY DIGITAL PRESSURE IN TWENTY-SIX HOURS AND FORTY-FIVE MINUTES.

By Dr. GEORGE H. PORTER, Senior-Surgeon to the Meath Hospital and County of Dublin Infirmary, &c.

[The patient was 26 years of age. He had a pulsating tumour, the size of a small hen egg, in the right popliteal space.]

Considering this a very suitable case to try digital pressure, I commenced it on the morning of the 19th April, at 11 o'clock, a. m., a number of the pupils of the hospital kindly volunteering their assistance, and each gentleman becoming responsible for two hours' pressure at a time, up to 9 o'clock, p. m. Having applied a flannel roller from the foot to the knee, and shown the pupils how to make pressure by placing the points of the middle and index fingers of the left hand over the femoral artery where it crosses the brim of the pelvis, then to fix and increase the pressure by placing the middle and ring fingers of the right hand on those already over the vessel, I desired that the pulsation in the tumour should be arrested as much as possible. I also taught the patient himself to press the vessel, and throughout the treatment of the case he occasionally gave assistance to the young gentlemen in charge. I visited him at 2 o'clock, p. m., April the 19th, and learned that the compression had been carefully sustained and was well borne by the patient. The tumour was certainly more solid than before, but the pulsation remained undiminished whilst the leg and foot affected were warmer than the other. Again, at 5 o'clock, p. m., of the same day I saw him. The solidity of the aneurism had by this time evidently increased; but as yet no apparent change had taken place in the force of pulsation. 9 o'clock, p. m. The tumour now was much more solid. The pressure was then discontinued for the night, and thirty drops of Battley's sedative liquor of opium administered in a draught.

April 20th, 9:30 o'clock, a. m. The man had passed a tolerably good night; awoke once about midnight with a sharp pain in the tumour. This was the first time he had ever felt pain in the aneurism, but it did not last more than half an hour, when he fell asleep. Compression was resumed this day, by gentlemen who came forward to offer their assistance, as others had done on the day before. 12 o'clock, p. m. The tumour now was very solid, and the pulsation was greatly diminished. 1:45 o'clock, p. m. Pulsation had now ceased completely, and the sac was firm and solid. The patient complained of slight numbness along the back, and outside of his leg, but there was no œdema whatever in leg or foot. The pressure was continued in a slight degree up to 7 o'clock that evening, when it was finally discontinued. The beating of the femoral artery could be traced down at this period to about four inches from the knee. He got the opiate, as before, at 9 o'clock, p. m.

April 21st. He had passed an excellent night; no pain or swelling in the limb. The superior internal articular artery was evidently enlarged, and pulsating strongly. I kept him in bed for a fortnight from this date, with the limb rolled in flannel, and the tumour gently rubbed, every night, with a liniment, consisting of 2 oz. of soap liniment and 15 grains of iodide of potassium. He was then allowed to get up, and move about the ward on crutches.

May 29th. I tapped his hydrocele; and on the 3rd of June he was discharged the Hospital.

Thus, in the short time mentioned, the formidable disease was perfectly cured by finger pressure. The point where the fingers rested was only slightly reddened, but did not become sore throughout the treatment; and

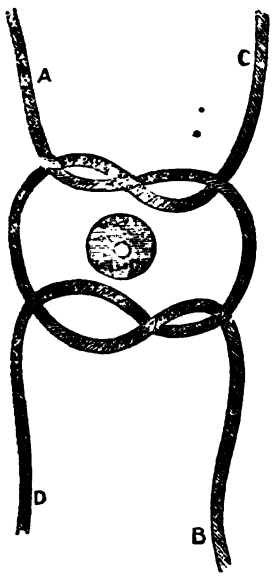
he never complained that the pressure gave him pain. This contrasts strongly with instrumental arrest of the flow of blood through an aneurism; but no matter how carefully applied, it cannot be borne so long as can the finger top; and further, the artery cannot be isolated so completely from its accompanying vein by mechanical as by digital compression, and therefore, œdema of the limb is more likely to ensue. There is less likelihood of ulceration or sloughing of the skin occurring under the point of pressure than when an instrument is used, inasmuch as by being so direct, a smaller amount of force suffices to control the current of blood through the artery; and, as the point of pressure can with such ease be changed to a higher or lower position. But in addition to all this, the rapidity of the cure in cases of digital treatment contrasts most favourably with any mechanical pressure, as in one case a few hours usually accomplish what in the latter requires days. With respect to the rapidity of cure in cases of aneurism treated by compression of any kind, I cannot more appropriately conclude this account of mine, than by citing some remarks of my esteemed friend and very accomplished surgeon, Mr. L'Estrange, at a recent meeting of the Surgical Society of Ireland. They struck me forcibly at the time, and are well worth the attentive consideration of every practical surgeon. I quote from the Dublin Medical Press, June 7th, 1865, p. 536. He said: "I would wish to observe that it appears to me that there must be some peculiar state of the blood, which admits of quicker coagulation in some individuals than in others, placed under similar circumstances and under similar treatment. For instance, we have had in this city, within this last few months, three cases of aneurism treated by external compression. In one, cure by coagulation of the contents of the sac was effected in the short space of five hours and three-quarters (Dr. Mapother); the second was effected in twenty-five hours and a-half (Mr. Porter)—(here he slightly erred); and in the third, coagulation could not be effected although the external compression was steadily kept up, and well borne by the patient for many weeks. It would be of great importance to ascertain this physiological and pathological fact, as to the coagulation of the blood in the aneurismal sac under compression. This I should think might be ascertained, or great light thrown on the subject, by taking a small quantity of blood from each of these patients—Dr. Mapother's, Mr. Porter's, and the one in the City of Dublin Hospital, in which coagulation would not take place by external compression; and by submitting their blood to chemical and microscopical tests, much information might be obtained, which would conduce materially to the scientific treatment of aneurism."—*Dublin Quarterly Journal, Aug.*, 1865, *p.* 11.

46.—ON A NEW MODE OF TYING ARTERIES, ETC.

By Frederick Churchill, Esq.

The very rapid success which has attended the introduction of acupressure as a hæmostatic agent in surgical operations, and its speedy adoption by many of our most distinguished surgeons, is sufficient proof that the ligature, though employed with such success for a number of years, is not the best mode of arresting arterial hemorrhage. Still the very fact that the advocates of acupressure have hitherto failed to produce a single valid argument to prove the advantages of acupressure over those of the ligature, except as a temporary compressor of the bleeding point, is sufficient to induce us to give further consideration to the claims of the latter. With this view I began to consider how we might combine the advantages of each, avoiding all the disadvantages, or, in other words, to secure the artery against the risks of secondary hemorrhage by a ligature removable at will. The result of my investigation is the knot I am about to describe. Take two ligatures of the ordinary length, and tie upon them a reef-knot. This may be either prepared beforehand, and kept slack, so as to be slipped over the head of the

forceps when the artery is seized, or, with the help of an assistant, it may be easily tied at the time of the operation. The form of knot may be better understood by referring to the wood-cut. In the centre of it is a transverse section of the gutta percha tube described below. When the knot is made fast, cut close either A and B or C and D, the remaining ends being arranged on each side of the flap. Where several ligatures are employed, it is advisable to tie the corresponding ends over the face of the stump. When the artery is occluded, which, I believe, will prove to be in a much shorter time than with acupressure, a gentle and equable pull of the corresponding ends in a straight line will untie the knot. The fact that the middle coats when divided will retract renders it certain that the risks of secondary hemorrhage are less than when the needle is used, and, consequently, an earlier removal of the ligature can be effected with safety.



Should the objection be raised that "in sponging the flap you may untie the knot,"—I have found by experiment that it holds as firm as the old ligature, except when counter-traction is made upon the corresponding cords, which never would be the case when sponging; or that "in removing the knot you disturb the artery,"—I maintain, on the contrary, that the removal of the knot *may* tend to occlude the artery more effectually, by twisting it upon itself, and cause the formation of an external clot within the sheath to support the internal coagulum. In order to test the resisting power of the knot to the current of blood in the artery, I made a number of experiments which prove that the single reef is quite sufficient to hold any artery. For instance, I took a piece of thick elastic tubing about three inches long, to one end of which I fastened my knot, and into the other end I inserted the nozzle of a very powerful syringe, the piston-rod being an inch in diameter. I then handed it to a sceptical friend, who injected water into the tube, but with all his force he failed to dilate the knot. This he acknowledged quite convinced him. Still, in order to combat, if possible, all the objections that may be raised, I experimented with the double reef—i. e., a reef-knot on either side of the artery, which has of course double the strength of the old ligature; and I removed this also with comparative ease, though not quite so readily as the single reef.

I am therefore sanguine that, should my future experiments on living animals confirm those I have already made, my plan will become generally adopted. With these few remarks, I beg to leave the suggestion in the hands of the profession, who I have no doubt will give it an unbiassed reception, if it prove worthy of recognition at all.—*Lancet, Nov.* 4, 1865, *p.* 510.

ALIMENTARY CANAL.

46.—ON THE RADICAL CURE OF INGUINAL HERNIA.

By Dr. J. Fayrer, Surgeon, Medical College Hospital, Calcutta.

The following method of operating for the cure of inguinal hernia I have found to be more simple and successful than that I had previously practised. It has, so far, been unattended with any dangerous consequences, and, though it has failed in some cases to give complete relief, it has improved

the patient's condition by enabling him to control the hernia more thoroughly by aid of a truss. It appears to have the advantage of not confining the patient very long to his bed, the treatment seldom extending beyond five or six weeks, and the latter part of that not necessarily requiring confinement. The only disagreeable results that I have yet observed, and that only in one or two cases, was suppuration extending towards the thorax, between the abdominal muscles, giving rise to irritative fever and exhausting discharges; but this has not endangered life, and has quickly subsided on free counter incisions being made to give exit to the pus.

Sloughing of the integument at the seat of operation rarely occurs, beyond the death of a minute portion of it just where the needle emerged and the ligatures are tied; and in no case have I seen peritonitis to a dangerous extent take place; nor has hemorrhage occurred, though it is difficult to understand how the epigastric artery always escapes.

The object of the operation is, as in the other methods, to invaginate a portion of the scrotum within the inguinal canal and secure it there. But I must state my impression that the success of the operation does not depend so much on retention of the invaginated scrotum within the canal as on the formation there of a quantity of exudation and cicatrix tissue. I have frequently remarked that the success of the case did not depend on the invagination remaining in the canal, for I have seen successful results when the invagination had completely descended, and I have also not unfrequently seen the converse of this.

My impression of the operation is, that it is very often successful, but frequently unsuccessful, and that we are not able to speak so positively as to the result as we are in other operations, so much depending on the accident of how the exudation may be poured out, and in what direction, and how the cicatrix will form.

The chief element of success appears to me to consist in the careful introduction of the plug into the canal, that the end of it may press well against the internal abdominal ring. Much, also, depends on the subsequent dressing, and care on the part of the patient, not by any premature effort or over-exertion to force down the protrusion whilst the process of cicatrisation is yet incomplete.

The instruments with which I operate are very simple,—two plugs of wood, silk ligatures, and a curved needle; one plug of wood about six inches long, rounded and compressed at the end and lengthways, and about the circumference of a man's thumb. This may be made of wood, ivory, or bone. Any wood will do, though, perhaps, ebony is the best. At one end it is pierced obliquely, and threaded with two strong ligatures made of the strongest ligature silk. These, before being used, should be waxed. A needle, made of strong steel, curved and inserted into a firm handle, with an eye at the point, through which the ligatures in the plug have to be passed. The curve of the needle amounts to about half a circle. In addition to these, a small blade or plug of hard wood or ivory about an inch and a half long, three-quarters of an inch in diameter, and rounded, is required to tie the ligatures firmly over when the plug has been inserted into the inguinal canal.

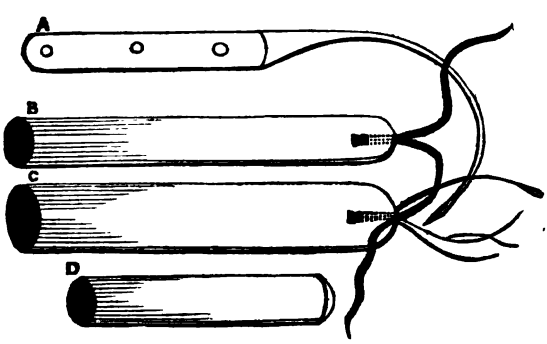
The mode of operating is as follows:—The patient is prepared by having the bowels opened the night before. The pubes and scrotum are shaved and the bladder emptied just before the operation, which may be performed under chloroform if the patient dreads the pain. The forefinger of the left hand is then introduced within the external abdominal ring, pushing before it an invagination of the scrotum. Having introduced it as far into the canal as possible, the needle is threaded with one of the silk ligatures, and being held in the operator's right hand is gradually insinuated along the palmar aspect of the left forefinger until it reaches the extremity of the invagination; it is then thrust boldly through the tissues lying over the finger, and emerges about one and a-half inches interiorly to the anterior superior spine of the ilium. The needle is then unthreaded and withdrawn. Again threaded with the second ligature, the process is repeated, taking care to pass the needle through the tissues, not quite at the same point in the canal as the first, but

bringing the ligatures out at the same aperture in the integument. This is easily effected by drawing the yielding integument over the point of the needle until it merges at the original point of exit. The needle is again withdrawn, and now the plug is insinuated into the canal whence the finger has just been withdrawn, and is tied tightly in the canal by the two ligatures being firmly knotted over the small piece of wood provided for the purpose. The plug, it is to be observed, should be well oiled and introduced as the finger is withdrawn. Until recently I was in the habit of using various sized plugs, fitted to the size of the abdominal ring in each particular case; but I now find that to be unnecessary, and use only one size of plug. Its object is to support the invagination at the upper extremity of the inguinal canal, and not to exert any lateral pressure. It is to be understood, however, that the canal is to be sufficiently occluded to prevent any descent of the hernia by the side of the plug.

The subsequent treatment is equally simple. When free suppuration makes its appearance either by the side of the plug or round the ligatures, the plug should be withdrawn. The invagination is then supported by a pad, after sponging away the discharge and dressing the wound with simple dressing and a spine bandage. The patient should be kept carefully in the recumbent posture. The bowels seldom act for several days after the operation, but the patient should be warned against straining if they should do so. An opiate is desirable after the operation to tranquillise the patient, and also to prevent any tendency in the bowels to act. It frequently happens that there is retention of urine. This is relieved by fomentation, or, if necessary, by the catheter. Abdominal tenderness requires hot fomentations; and if any indication of peritonitis present itself, frequent and full doses of opium will be needed, but this in my experience is very rare.

In ordinary cases the wounds, scrotal and abdominal, heal rapidly, and all that is needed is careful dressing to prevent descent of the hernia. The patient should not walk until the wounds have healed, and then only with a well-fitting pad and bandage. When the wounds have thoroughly healed, the patient may take exercise with a well-fitting truss, which should not be left off for three or four months, when cicatrisation may be presumed to be completed.

These are the instruments with which I operate. The plugs are made either of ebony or teak wood. B is the plug in ordinary use; C is used in



A.—Needle.
B.—Ordinary plug.
C.—Larger plug for large herniæ.
D.—Block to tie the ligatures over.

some of the larger cases of scrotal hernia, where the abdominal ring is very large.—*Med. Times and Gazette, August* 19, 1865, *p.* 193.

47.—ON THE COMPLICATIONS OF INTERNAL HEMORRHOIDS AND THEIR TREATMENT.

By W. Allingham, Esq., Surgeon to the Great Northern Hospital, &c.

[The treatment of hemorrhoids followed by Mr. Henry Smith, viz., the clamp, excision, and the actual cautery, are now well known, and do not require review to render the following intelligible.]

Let us examine for a moment Mr. Smith's statement as to the superiority of the cautery over the ligature. He says:—"It is not possible that tetanus or pyæmia, the two most formidable results of the ligature, can occur after this operation." Allow me to observe that on this point Mr. Smith gives very little evidence; the few cases operated by him and others can form no satisfactory standard of comparison with the results of the ligature.

From 1763 operations upon hemorrhoids with the ligature performed at St. Mark's Hospital, five cases of tetanus resulted. It is remarkable that four of these occurred in the spring of 1858 (two in March and two in April), a year in which I find tetanus was very much more frequent than usual in other hospitals. From 1858 to the present time there has been no case of tetanus, and rather over 800 operations for hemorrhoids have been performed. Although Mr. Smith asserts that it is not possible for tetanus to follow his operation, I cannot myself see why it should not; in fact, I think most surgeons would bear me out that a burn has been a frequent cause of tetanus. In the year 1857 I removed a very vascular tumour from the entrance of the vagina of a young woman. The hemorrhage was arrested by the actual cautery: no ligature was used; tetanus supervened in five days.

With regard to pyæmia resulting from the ligaturing of hemorrhoids, I will again refer to the statistics of St. Mark's Hospital, which show that no case of pyæmia after the operation has ever occurred in that institution.

And now as to the final advantage claimed by Mr. Smith—viz., "That patients are not half the time in bed, and the convalescence is much more rapid."

This, I am willing to admit, would be a great superiority over the ligature if it could be safely guaranteed; but it is in this particular that I have been most disappointed, for in none of the twelve cases I have operated upon with the clamp and cautery was the convalescence materially shortened.

After the ligature, I keep my patients lying down until it separates, which usually takes place on the sixth or seventh day. They then get up; but I interdict any active exertion, and recommend the recumbent posture for a few days longer, or until the wounds are nearly healed. I am quite aware that patients might get about much earlier, but I think there would be danger in doing so; and I should anticipate as the probable result chronic ulceration, possibly ending in more or less stricture. I operated some time ago upon a gentleman who had bad hemorrhoids, to which I applied five ligatures, and, through an unexpected turn of business, he was obliged to get up on the fourth day after the operation and go to town where he remained standing and sitting for some hours (as he assured me) with less discomfort than he had frequently experienced before the operation. The case did none the worse for it, but I thought it a very dangerous experiment, which I should not advise any one to repeat. Now let me ask, what is the condition of the rectum of patients operated upon by the clamp and cautery at the end of five or six days? From a careful examination of my patients, both by the finger and the speculum, I can assert that they have more or less extensive wounds where the piles have been excised and the slough produced by the cautery has separated. I do not for one moment doubt that they could resume their usual avocations, but I submit that it would not be sound surgery to allow them to do so. Active exertion and the erect posture maintained for any length of time so soon after the operation is very inimical to cicatrisation, for the veins of the rectum in these cases are dilated, and, moreover, they are destitute of valves, so that the sore much resembles that condition called varicose, and, therefore, I believe the recumbent position is most advantageous. This point I have frequently seen exemplified in simple ulcer

of the rectum, which has resisted all kinds of treatment as long as the patient went about, but has rapidly healed with a little opiate injection when the horizontal position has been maintained for a week or two.

I cannot help believing that if this attempt to save time after the operation by excision and the cautery be persevered in, it will prove disastrous to many patients. One of my cases after that operation got about on the fourth day, and then for three consecutive days lost as much as a quarter of a pint of arterial blood on going to stool, and this was checked by confining him to his bed again for a week.

In cases of hemorrhoids complicated by fistula the sinus should be laid open, and at the same operation the hemorrhoids ligatured. Even if the fistula be a very severe one, I should prefer doing the operation all at once. Of course, the time of recovery is delayed, but in this respect also a saving is effected in the end. If the fistula be divided and the hemorrhoids left to a future operation, they come down more, and are a constant source of annoyance to the patient and irritation to the wound, which may thus be prevented from healing.

I saw a case of this kind some time ago, in which the fistula remained unhealed four months after the operation. I think in these difficult cases it is advisable, not only to slit up the sinus, but to make a tolerably free incision through the floor of it, so as to divide any fibres of the sphincter that may be beneath the fistula, as well as the indurated tissues which usually form its base. The incision, also, should, I am of opinion, be carried beyond the external orifice of the fistula into the healthy tissues, by which means you may avoid a collection of matter, which burrows under the integument, and gives trouble, and possibly may necessitate another incision.

I must here enter a protest against too much dressing and the use of ointments in these cases; a little dry cotton wool inserted gently between the edges of the wound is all that is required. I am sure I have many times seen fistulæ remain unhealed in consequence of the interference of the surgeon, who had daily put lint smeared with a stimulating ointment to the bottom of the wound. In cases where severe ulceration and stricture co-exists with hemorrhoids, only palliative measures can be adopted. The use of sedative injections, the occasional introduction of a bougie, rest in the horizontal position, and a careful regulation of the bowels may do an immense deal, in some cases even effect a cure; but, failing that, the patient may be made very comfortable, and life, which was previously a burden, be rendered quite endurable. Any operation upon the piles must, as in the case I related in my last paper, only aggravate the already deplorable condition of the patient.—*Medical Times and Gazette, Sept.* 2, 1865, *p.* 250.

48.—ON THE ALLEGED DANGERS OF THE LIGATURE IN THE OPERATION FOR HEMORRHOIDS; AND THE COMPARATIVE MERITS OF THE TREATMENT BY THE CLAMP AND BY THE LIGATURE.

By James R. Lane, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mark's Hospital for Diseases of the Rectum.

There is perhaps no operation in the whole range of surgery which affords so much relief at so small a cost of suffering and risk as the removal of hemorrhoidal tumours by the well-known and long-established operation with the ligature. Nevertheless there appears still to be, on the part of the public, and even of many members of the medical profession, a sort of undefined dread of ill consequences arising from it; the result of which is, that many persons are induced to linger on for years in misery and discomfort, when surgery is capable of affording them complete and permanent relief, at a risk scarcely greater than they are constantly incurring in the ordinary affairs of life.

A good illustration of what I state has been recently afforded me in the case

of a young officer, who had spent some years in India, and who was suffering from the disease in its most aggravated form. It completely incapacitated him for his duties, and compelled him to return to this country for treatment. When I saw him he had been here more than a year, but had had nothing done, because he had been told by a physician of eminence, whom he consulted, that it would cost him his life if he allowed himself to be meddled with. He was otherwise in fair health, and I could discover nothing in his case to contra-indicate an operation. I therefore persuaded him to submit, and the consequence was that he recovered without any untoward symptom, and was quite well at the end of a fortnight.

I have no doubt that to the existence of this vague impression might be traced the introduction of nitric acid in the treatment of this disease; and now, since experience has shown this remedy to be quite inadequate to the cure of the complaint in its severer forms, and only applicable to a small and exceptional class of cases, I believe we owe to a similar cause the introduction of a new operation with a clamp and the actual cautery. This method has been persistently vaunted during the last two or three years, as affording a certain means of cure combined with a complete immunity from all those indefinite ills to which the ligature has been stated to give rise. I have no wish to depreciate this new operation, which is ingenious enough, and may, I doubt not, be found effectual, though I do not believe it to be better, and there is not a shadow of proof that it is safer than the methods previously in use. I cannot, however, refrain from expressing my opinion that its author, Mr. H. Smith, has exceeded the limits even of parental partiality, in the way in which he has attempted to build up its reputation at the expense of the fair fame of our old and tried friend, the ligature.

The operation to which I allude is performed in this wise. The hemorrhoidal tumours being protruded, they are seized and their bases firmly compressed with the clamp. They are then cut off, and, to prevent bleeding, the cut surface is cauterized, either with nitric acid or the hot iron, the latter having been found the more effectual. The clamp is then removed, and the cut surface, covered of course by the layer of slough produced by the cautery, is allowed to recede into its place within the rectum. Any external piles which may happen to be present are then cut away with the knife or scissors.

What is the *real* state of the case as regards the consequences of the operation with the ligature, deducible from the experience of those who have had the largest opportunities of forming a judgment on such a matter? The following quotation from the writings of Sir B. Brodie must commend itself to every one by its plain common sense and sound judgment. He says:—"I conceive this is not only one of the most effectual, but one of the safest operations in surgery. I should think I must have performed or seen it performed between 200 and 300 times. I saw one patient who died after the operation, in consequence of diffuse inflammation of the cellular membrane running up the inside of the gut as high as the mesentery; but that was a patient whose constitution was broken down by long-continued hemorrhage, and in whom any slight accident might have produced equally bad consequences. I saw another patient who, a week after the operation, and having been quite well in the interval, had an attack of pain in the abdomen and shivering, attended with fever, and died..... With the exception of these two cases, I never knew any ill consequences arise. I contend therefore that the operation is as safe as any operation can be expected to be. You are not to suppose that even the slightest operations in surgery are absolutely in all cases free from every particle of danger, any more than the slightest accident. I have known two patients die after the extraction of a tooth, and I have known several die in consequence of venesection of the arm, or an accidental prick of the finger. The chance of danger from this operation is at any rate so trifling that you need not calculate upon it. If you were to calculate upon so small a chance as this, you would scarcely be able to do anything in the ordinary affairs of life." Again, what is the opinion of the leading surgeon of the northern capital, who is well known to have had for many years a most extensive practical experience in such cases. Mr. Syme says:

"I feel warranted, after very extensive employment of the ligature, to state that it may be used without the slightest risk of any serious inconvenience. Indeed, in the whole course of my practice, I never met with a case which terminated fatally, or threatened to do so, when the ligature simply was employed."

My own experience of the operation with the ligature amounts now to as many as 427 cases. In the immense majority the progress towards recovery has been singularly uniform. It has been rare indeed that I have met with any untoward symptoms, or that the healing has been much delayed; and I have never seen any cause for serious alarm, except in two cases, which occurred more than seven years ago, and which have already been made public. The two cases to which I allude proved fatal from tetanus. They were lodged in the same ward, and were seized with tetanus on the same day, at a time when that complaint was epidemic in the hospitals of London to a remarkable, and I believe almost an unprecedented extent. Out of the whole number of 427 cases which I have treated, I have never seen an instance of pyæmia, of erysipelas, or of diffuse inflammation. Mr. Gowlland, my colleague at St. Mark's Hospital, must have operated on as nearly as possible the same number, his appointment to that institution having been made at the same time as my own, and I know that his experience is almost identical with mine on this question. He has never met with a case of pyæmia, and has had no fatal result, excepting, strange to say, two cases of tetanus, which occurred within a short time of those to which I have already referred, and were fairly attributable to the influence of the same epidemic.

Tetanus and pyæmia, as we have seen, are alleged to be "the two most formidable results of the operation with the ligature." With respect to tetanus, there is no doubt it will occasionally follow this, as it will also occasionally follow every conceivable surgical proceeding. By careful search in the medical journals it would not be difficult to find cases in support of such an allegation against almost every operation in surgery, whether small or great. But I contend that there is no evidence that the ligature of hemorrhoids has any special liability to produce this disease, and that there is no ground whatever for the statement that the inclusion of tissues in a ligature is a more probable exciting cause than the cauterization of the same tissues with hot iron. In point of fact, burns have long been rather unfavourably known for their tendency to be followed by tetanus; therefore, in contrasting the cautery with the ligature, the inference to be drawn is certainly not to to the disadvantage of the latter. The assertion that it is *not possible* for tetanus to follow the operation with the clamp and cautery is too absurd to require refutation, and equally so is the argument that an operation is exempt from an exceptional complication like this because twenty, fifty, or even a hundred cases have not furnished an example of it.

Again, with respect to pyæmia, it is to be feared that this is an occurrence from which no surgical operation will ever be able to claim complete exemption, especially in large hospitals. But all our knowledge on the subject tends to show that it depends on the health and condition of the individual patient, and on his atmospheric and other surroundings, rather than on the nature of the operation. It will be time enough to argue against the singular theory, that a slough produced by ligature and a slough produced by cautery are materially different in their predisposing influence, when any reliable facts are adduced in its support.

But I am in a position to assert confidently, and on positive grounds, that the operation with the ligature does *not* possess any peculiar liability to pyæmic infection. On the contrary, the reverse according to my experience has been remarkably the case, so much so that I have long been surprised at the exemption. I have the authentic testimony of 427 cases occurring in my own practice, and reliable information of about an equal number in the practice of my colleague, thus making a total of about 850, and out of this large number not a single instance of pyæmia has been observed.

Having now shown, I trust conclusively, that there is no ground, either in reason or fact, for the assertions which have been made in favor of the clamp

at the expense of the ligature, as regards immediate danger to life, I would ask to be permitted to examine some of the other grounds on which it has been pressed into public notice. We are told by the author of the operation (see the Lancet, Aug. 19, 1865, p. 215) "that it is not only on account of its far greater safety that he practises and strongly recommends the employment of his clamp in cases of internal hæmorrhoids and prolapse, but he adopts it for the additional and important reason, that the period of convalescence after this operation is rendered so brief that the patients are enabled to be up and about their business *in one-third of the time* which is of necessity consumed after the ordinary operation with the ligature." The majority of the patients are stated to have been "moving about" in four or five days, though in some it is admitted that the convalescence was protracted over a period of a fortnight or three weeks.

I am unable to speak from direct personal experience of the comparative merits of the two operations. I have not yet employed the clamp, in consequence of a scepticism, for which I trust I shall be pardoned, with regard to its alleged extraordinary merits, and also because it appears to me to possess the disadvantage of being more painful and protracted at the time, and of being, in severe cases, by no means certainly free from a liability to hemorrhage subsequently. This liability, indeed, is evident from Mr. Smith's account of his own cases. I have, however, had the advantage of repeatedly seeing it performed by my colleague, Mr. Gowlland, who has used it in I believe about twenty cases. Having witnessed most of that gentleman's operations, I can testify to the careful and complete manner in which every detail was carried out. I have seen most of these cases after the operation, I have made particular inquiry respecting their progress, and I have found that there has not been, as a rule, less pain than after the other operation, nor has the convalescence been in any degree accelerated. They have not been considered fit to be discharged from the hospital in a less time than the average of other cases. My colleague, Mr. Allingham, informs me that he has operated with the clamp and cautery on fourteen cases, and he has arrived at precisely similar conclusions. I therefore take the liberty of demurring to the statement that the recovery will be complete in one-third of the time, and that patients will, as a rule, be able to go about their business in four or five days.

But I must also demur to that statement for the following additional reasons:—In the great majority of cases requiring operation there is more or less redundancy of the outer skin, "external piles," requiring removal. These must be cut away with the knife or scissors, whichever operation is adopted; and the sores resulting from their removal will render the majority of patients extremely averse from "moving about" more than they can possibly help, at the end of four or five days. But where the outer skin is not inflamed or redundant, and does not require to be interfered with, but the disease is altogether internal, and the parts admit of being restored to their natural position within the sphincter as soon as the operation is concluded,—in such cases, I contend, the patient will be quite as well able to "move about" after the one operation as after the other. Whether it is prudent to allow him to move about is another question. I am strongly of opinion that it is not; indeed in private practice I not unfrequently find it necessary to repress the tendency to do so, because I believe that where there are large sloughing sores in the interior of the rectum (whether produced by ligature or cautery it cannot matter one straw) it is of the first importance that rest and the recumbent position should be observed, at all events until healthy granulation has commenced. If this salutary rule is largely departed from, I venture to predict that untoward results will soon become more frequent, whether the operation be performed by the clamp or by the ligature.

But it has been also alleged as an argument against the ligature and in favor of the clamp, that sometimes "the wounds resulting from the separation of the ligatures would not heal up for a long period, and the patient would be subjected to much painful suffering, necessitating, perhaps, some other operation." According to my experience, cases of this kind are very uncommon, though I admit, of course, that they may occur; indeed, after an operation,

out of a large number of cases, delay and obstinacy in the healing process must always be occasionally met with; but I would ask, on what conceivable surgical principle should such a result not be equally likely to follow the operation with the clamp and cautery. Wounds made with the cautery, on the outside of the body at all events, have never been noted for any peculiar rapidity in cicatrization.

I repeat that I have no desire to depreciate the operation with the clamp. I believe it to be a sufficiently effectual method of removing the disease, but I consider it to be in many respects inferior, and in no respect to have the advantage over, the operation with the ligature. It is, in truth, only another mode, but a less certain and complete mode, of doing the same thing. I admit, however, that it is an immense step in advance over the treatment by nitric acid, with which the name of its author was some years since so much identified. I should never have thought of saying a word on the subject had the new operation been left to stand upon its own merits, and had not comparisons, which I consider to be unjustifiable and unfair, been made between the two operations. I should be delighted to welcome any real improvement in the treatment of this disease, but I cannot admit that this is likely to be effected by the clamp. On the other hand, I consider that the operation with the ligature has done incalculable service to humanity, and my belief is that it will continue to do so long after the clamp, like many other ingenious novelties, has been laid aside and forgotten.

In conclusion, I would add a few words respecting the best mode of operating with the ligature. The plan which I invariably adopt is that invented by Mr. Salmon, which is, in my opinion, far superior to any other. By means of it most of the objections which can fairly be urged against the ligature are obviated. The hemorrhoidal tumours being protruded, they are seized in succession with a hook or vulsellum, and are separated with the scissors from the subjacent parts along the line of union between skin and mucous membrane. The cut made with the scissors should be sufficiently deep and free to detach the tumour from the muscular tissue on which it rests to an extent equal to about three-fourths of its base, leaving it attached by its upper fourth only. The ligature is placed in the deep groove thus made, and is tied tightly round the upper attached part. The danger of hemorrhage is securely provided against, because the vessels which supply the tumour do not enter it indiscriminately at its base, but descend from above beneath the mucous membrane, and their trunks are therefore necessarily included in the ligature. When the ligatures have been tied, the bulk of the tumours may be cut away, care being taken to leave sufficient tissue beyond the ligature to prevent it from slipping, and the parts may then be returned within the sphincter. Any redundancy of external skin which is apparent *after the parts have been returned* must be cut off with the scissors. This plan of operation is far preferable to the usual method of strangulating the whole base of the tumour by transfixing it with the needle, and tying it in two halves—first, because it is completed in half the time; secondly, because scarcely more than one-fourth the amount of tissue is strangulated by the ligature—indeed the ligature need not include much more than the trunks of the vessels supplying the tumour; thirdly, because the ligature is tied round mucous membrane at some distance from the anus, where the sensibility is much less acute, and consequently the after pain and irritation are very greatly diminished. Instead of the whole attached base of the tumour being converted into a sloughing wound, as when the whole of it is included in the ligature (and equally, I might add, when the whole of it is burnt with the hot iron), three-fourths of it is made into a simple incised wound with the scissors, and the sloughing process is confined to the upper ourth only.—*Lancet, Sept.* 2, 1865, *p.* 256.

49.—ON POLYPOID GROWTHS IN THE RECTUM, AND THEIR OCCASIONAL ASSOCIATION WITH ANAL FISSURES.

By JAMES R. LANE, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mark's Hospital for Diseases of the Rectum.

[The object of the present paper is to call attention to the association of the rarer diseases of polypus of the rectum with anal fissure; the two affections apparently holding the relation to each other of cause and effect.]

In the cases before considered—namely, of hemorrhoids combined with fissure—the outward evidence of the former disease is so much more prominent and striking, that the latter is in danger of being overlooked, and of being disregarded in the treatment. The reverse, however, is more likely to happen in the cases now under consideration; for in them the fissure will, in all probability, be readily discovered,—while its cause—the polypoid growth within the rectum—is very likely to escape notice. The following is a good example of this class of cases. I could adduce many more, but one will be sufficient for the purpose.

Isabella B., aged 40, needlewoman, unmarried, was admitted into St. Mark's Hospital, December 17th. She had had some uneasiness and sense of irritation about the rectum for eighteen months, with occasional loss of a small quantity of blood with her motions. For the last six months something about the size of the tip of her finger had been occasionally protruded at stool, and had caused severe pain till it had been drawn back again, which was only effected by degrees and with great difficulty. For eight weeks, however, she had suffered pain of the most acute character during and after every action of the bowels, and independent of the protrusion alluded to, which was not constant. The pain lasted nearly the whole of the day; it compelled her to keep in bed during its continuance, and almost entirely prevented her following her occupation. The dread of it caused her to avoid passing a motion as long as she could, and the accumulation of hardened fæces thus occasioned of course greatly increased her distress. She had the anxious and haggard expression so often seen in persons suffering from fissure of the rectum—like that of a person labouring under severe organic disease; and she stated that she was "falling away" rapidly.

On examination, I found a well-marked fissure with indurated edges upon the sphincter muscle in the usual situation at the posterior part of the anus. With the finger in the rectum, a polypus, the size of a nut, was readily discovered. It was attached by an elongated narrow pedicle to the posterior part of the bowel; but it had no direct connexion with the fissure, for its pedicle was inserted nearly an inch and a half higher up. The examination was attended with severe pain from the spasmodic contraction of the sphincter muscle.

Dec. 19th. A free incision was made through the fissure. The polypus was then drawn down by a vulsellum, and a ligature was applied at its point of attachment. It was then cut off just beyond the ligature.

The result was complete relief from all the painful symptoms. The ligature came off on the fifth day, and the wound was soundly healed at the end of three weeks, when she was discharged from the hospital cured. A very marked improvement had taken place in her general health and appearance; the anxious expression of face had disappeared, and she had gained flesh considerably. She had had no pain since the operation, except the slight smarting occasioned by the fæcal matters passing over the cut surface.

Since my connexion with St. Mark's Hospital, I have met with as many as nineteen cases similar to the above, illustrating the combination of rectal polypus with anal fissure. In this particular instance the protrusion observed by the patient at once directed attention to the presence of some growth within the rectum; but often no protrusion is noticed, and then the real nature of the case is in greater danger of being overlooked.

In exemplification of this, I may mention the case of a young married lady who was under my care about twelve months ago. She had suffered for

many months acutely with symptoms of fissure, and during that time had miscarried, the miscarriage having been apparently brought on by the severe pain and irritation of the anus. Some weeks before I saw her she had consulted another surgeon, who had discovered a fissure, and had made a slight incision through it, at the same time removing an external pile. She was at first relieved by this operation; but as the wound healed the symptoms returned as severely as ever. When I saw her, the remains of the wound were in a highly painful and irritable condition; and on further investigation. I found a distinct polypoid growth within the rectum, attached about three quarters of an inch higher up. The removal of this, with a repetition of the incision, this time made somewhat freely, completely cured her, and she has had no return of the symptoms. Shortly after the operation she became pregnant, and she has recently been confined at the full period.

Such a mistake as this is very disheartening to the patient and very annoying to the surgeon. It is, however, not unlikely to happen to any one not familiar with the management of such cases; nor is this the only instance which has come under my notice. It is chiefly with the view to point out this source of error that I bring the subject forward, for the co-existence of the two conditions is not very unfrequent. To avoid mistake, therefore, it is well worth while always to examine the interior of the rectum carefully before operating in cases of fissure. The examination, however, must be made carefully and thoroughly, for the polypus, being very movable, is apt to recede before the finger, and may very easily be missed.

A few years since, Mr. Baker Brown published some cases of this kind (see *Lancet*, July 14th, 1860, p. 31), which I read with great interest, for his observations on the subject correspond very closely with my own. I cannot, however, quite agree with him in his conclusion "that a very large number of fissures of the rectum are produced by these little polypoid bodies," and that "they will be found in almost every case if carefully sought for." On the contrary, I am continually meeting with cases of fissure—and indeed they form the great majority—in which nothing of the kind can be discovered after the most careful search. I do, however, not unfrequently find a small excrescence of papilla of mucous membrane at the upper end of the fissure, exactly corresponding to the little inflamed flap of skin so often seen at its outer end; but this is of a different character, and is evidently a subsequent formation—the result, and not the cause, of the local inflammatory action which has been set up. In the cases which I have noted as examples of polypus, and to which I am now alluding, I have only used the term to designate a distinctly pedunculated growth attached to the interior of the bowel, which has had no direct continuity with the fissure, but, on the contrary, has often sprung from the opposite side of the rectum. On enquiring into their history, I have usually found that there has been some irritation about this region for a considerable period, often with loss of blood; but at a later date the acutely painful symptoms have been superadded. The earlier and milder symptoms have without doubt been caused by the polypus; and this, by continually falling against and irritating the anus, has in course of time produced the fissure.

[Mr. HENRY SMITH wishes to protest against the opinion Mr. Lane, in speaking of the treatment of hemorrhoids by the clamp as a new operation, and of Mr. Smith as its author. He says:]

I wish I could lay claim to such an ingenious and useful operation. By referring to page 88 of my Lettsomian Lectures, Mr. Lane can see that I have mentioned that late distinguished Dublin surgeon, Mr. Cusack, as the one who suggested the clamp and cautery thirty years ago, and I have related how in this city Mr. Henry Lee had adopted the operation, and how I was induced by the results of his experience to follow it up. The only credit I claim is of having so altered and improved the clamp hitherto used that it seems to fulfil all the purposes desired, both as regards safety and efficiency. —*Lancet, July 22, and Sept.* 9, 1865, *pp.* 87, 303.

50.—PERSULPHATE OF IRON IN HEMORRHOIDS.

Assistant-Surgeon Cartwright strongly recommends an ointment of persulphate of iron (3ss—3j ad oz.) in hemorrhoids, especially when these are ulcerated, or when the constitution has become debilitated by diarrhœa or excessive fatigue.—*Med. Times and Gazette, Sept.* 2, 1865, *p.* 267.

ORGANS OF URINE AND GENERATION.

51.—ON LITHOTRITY.

By William Fergusson, Esq., F. R. S., Surgeon to King's College Hospital, Professor of Surgery in the Royal College of Surgeons, and Surgeon Extraordinary to H. M. the Queen.

[Lithotrity has lately appeared likely to supersede almost completely the more formidable operation of lithotomy. The life or death of the patient often depends upon the decision whether cutting or crushing shall be selected.]

What are the circumstances which lead us to our determination? There are few questions in surgery more important, and I fancy that I cannot in this lecture address you on a more interesting subject.

Age seems to me to form a most important element, particularly in the male. It may reasonably be doubted if better can be done before fifteen than cutting for stone. The success of lithotomy in young subjects is probably greater than could be secured by lithotrity in similar cases. Taking my own experience, announced last year, of only two deaths in fifty operations, I have no hesitation in stating my conviction that I could not have had equal success with lithotrity in these cases. In some the process would have been well-nigh impossible, such as with stones of this bulk, for here the diameter of the urethra would have precluded the use of a lithotrite of sufficient power. Looking to the size of the urethra in the male before puberty, its length, its irritability; the comparative irritability of the bladder and of the subject generally; and I may add, the conical shape of the bladder, whereby there would be a greater tendency to blockage and retention than in the adult, I feel justified in stating that where the operation might require repetition, it would certainly, although ever so successful, be attended with an amount of distress to the patient, and probable trouble to the surgeon, far greater than any supposed advantage. I confess, however, that I do not think this question has been investigated. Until within these few years there were no instruments that could have been used in such cases. To the best of my knowledge there were none such as I displayed in this theatre last year; but if blades like these [showing a variety] could be brought against small stones such as are usually found in young persons, so as to effectually crush them to sand, I believe that this operation might after all be found more generally applicable in children than it is usually deemed. As an adjunct to this kind of practice, chloroform would be indispensably necessary; without it the needful quietude could not be secured.

In the female, whether under or over puberty, I consider that any other operation than lithotrity should be an exception to the rule. With various-sized instruments sufficient strength of blades can be calculated on; and chloroform will permit the ready use of both crushers and scoops; so that, in accordance with views which I laid before you last year, the operation might be completed in all ordinary cases on one or two occasions.

But stone is found in the adult twice as frequently as in the young person, and, for manifest reasons, its treatment in such patients must be held as of more than double importance. It is of greater interest to consider what is best for this larger class of cases, particularly as the rate of fatality in lithotomy rapidly increases after puberty, and it is chiefly in these instances that the question between lithotrity and lithotomy arises.

There are, I suppose, surgeons in this country who still look upon lithotomy as the rule, and lithotrity as the exception. Yet I believe there is a generally felt impression that the reverse should now be held as the rule; and such is my own conviction. When stone requires mechanical treatment, lithotrity should be the first proposal, and if it cannot be, then lithotomy must be thought of. If such be the predicament, let us see by what process of reasoning the surgeon may have come to such a conclusion. He will have examined his patient, and found the urethra narrow and irritable; the neck of the bladder and mucous membrane particularly sensitive; the prostate somewhat large, and so firm in substance that it has been difficult to pass a sharp-curved instrument; the bladder disposed to throw off its fluid contents; the stone large, perhaps more than one; and possibly withal an irritable temperament, a want of moral and physical courage, with a disposition to oppose, rather than facilitate, all manipulations. These conditions, where present, hold out most unfavourable prospects for the satisfactory or successful issue of lithotrity, and it is to me very doubtful if the operation should be resorted to in such cases. Yet it would be unwise to reject lithotrity in all at a first or second examination. It is remarkable how in some these conditions become modified. The patient may pluck up better spirit, the parts by usage may become less sensitive, the operator by custom may manage to get through the prostate more readily, and the fragments may possibly be more easily dealt with than the entire stone. All this may happen, yet it must be admitted that the conditions referred to induce at first sight serious objections to lithotrity.

It is a good custom, in many instances, to make repeated examinations before a final decision; for whilst the opposing indications may in some be favourably modified, it will occasionally happen that where at a first glance all may have appeared highly propitious to lithotrity, a condition may arise which will at once put an end to all idea of resorting to it. I have seen illustrations frequently of both these changes, and have sometimes been much struck with the effects of what may be called preliminary manipulative treatment. It frequently seems desirable to dilate the urethra with bougies before crushing, and their use is often advisable to relieve irritability—a result of which we have daily cognisance in treating strictures; but as in such cases we every now and then see considerable fever excited, and a train of symptoms particularly alarming to those who have little experience, so in using bougies preliminary to lithotrity, some troublesome symptoms may arise which may induce the surgeon to be unwilling to adopt that procedure. After the bougie the patient may in a few hours experience a chill; shivering, sickness, and vomiting may all ensue to a considerable extent; the surface of the body may become remarkably pale and cold, the pulse will be very feeble, the eye will appear sunken, the voice will be as in a whisper, and it will seem as if rapid sinking had set in. After a few hours the rigors will cease, and so will the sickness; the pulse will improve, and so will respiration. A reaction will take place, almost as alarming to the inexperienced as the previous condition. There will be rapid breathing, a bounding pulse, a flushed surface, particularly of the face, and a racking headache. Profuse perspiration will then set in, and most of the alarming symptoms will disappear. The kidneys and bladder will sympathize. At first there will be scanty and high-coloured urine, and when it becomes more profuse, as it does in the latter stages, it will deposit a thick sediment, and possibly a copious quantity of tenacious mucus, in some instances joined with pus. In eight-and-forty hours the patient may be as well as ever, but in some there will remain a great disposition to a recurrence of most or all of the evils. Such symptoms as these are by no means uncommon after lithotrity, and they cause much distress to the patient and anxiety to all. Every one experienced in stricture cases must be more or less familiar with them. When, a few years ago, perineal section was more in fashion than at present, such symptoms were not unusual, and to the uninitiated caused great alarm; yet, though formidable in aspect, they are not always so in reality. Once passed off, they seldom recurred, chiefly, perhaps, owing to the cause which had

induced them not being repeated. Now, if such evils are brought on by the simple use of a bougie, what might not happen after a single sitting of lithotrity?—what after frequent repetitions? A man could scarcely feel justified in subjecting his patient to lithotrity after such experience as this. I have repeatedly declined it, and have afterwards seen the best results from lithotomy. If a stone be small, and it be possible to make away with it at a single operation, I should not mind advising lithotrity on a very brief acquaintance; but if the operation is likely to require two or more sittings, then I should think it wise of the surgeon to test, as it were, his patient's constitution by a repeated preliminary use of bougie or sound.

As yet I have referred only to the cases likely to be rejected for lithotrity. Let me now speak of those which seem peculiarly favourable for the process. If the urethra be wide and free from marked irritability; the neck of the bladder and mucous membrane of that viscus be not particularly sensitive; if the prostate be of normal or moderate size, and so soft that a sharp-curved sound, or lithotrite, can be readily passed, the case will appear in most respects favourable. If, added to these, the stone be small, then all the essentials to successful lithotrity may be deemed present, and it is not likely that the bladder will be subject to spasm, nor will the patient evince much irritability either of mind or body. Even though the stone should be of considerable magnitude, or should two or more be present, there need be no hesitation in selecting lithotrity. It may be that evil will come after all, but under such circumstances the surgeon may feel satisfied that he has done the best for his patient that human judgment could dictate. Should he in the interval have had, or seen, one or more successful cases of lithotomy, he need not repine and wish that that operation had been selected instead, for had it been, the results might have proved equally disastrous.

When lithotrity is seen in perfection, it certainly is a vast triumph of surgery. A concretion, too large to escape by the natural passage from the bladder, and which, when allowed to remain, almost certainly increases in size, and thereby adds to the daily and hourly sufferings, is broken into fragments so small that they pass away with the ordinary stream of urine; and thus the bladder is left free and in a normal condition. The patient need not be in bed an hour beyond the customary period of rest, and may move about, attending to his ordinary duties as usual. He may walk to the surgeon's house and from it, as if paying a friendly visit, and in a few days or weeks have his mortal foe within his manual grasp, shattered to fragments, and impotent for further evil!

Certain dogmas have prevailed regarding lithotrity which had been hurriedly fixed, but which have now been dispelled by experience. Thus it was said that the operation was scarcely practicable or proper unless eight or twelve ounces of fluid could be retained; indeed, the more the better! This was under the impression that the stone could be more readily seized, and that the distended mucous membrane would be less likely to be nipped between the blades, or between these and the stone. If sufficient urine were not already in the bladder, the first step of the operation was to inject tepid water. Now I think it may be set down as a rule that a capacious bladder and large quantity of water are objectionable. They certainly do not preclude the operation, but they assuredly make it more difficult and uncertain. A stone or fragment moves so freely in a large quantity of water, the slightest touch causing it to change its position, that it readily eludes the blades; and, again, where such a bladder as is here implied is nearly empty, the mucous membrane may possibly be in folds, between which fragments are liable to escape the closest search. To say the truth, however, I have little faith in the presence of so-called folds of the mucous membrane of the bladder. For twenty years or more I have rarely used injections as a preliminary step, and I have often drawn off urine with a catheter when, in accordance with these views, I have fancied the quantity too large. A small proportion of fluid is decidedly better than a large, but care should be taken that the blades of the lithotrite can be used with facility, and without the chance of contusing and abrading the mucous surface.

Paralysis of the bladder such as requires the daily use of the catheter has been generally considered a positive bar to lithotrity. But with the small scoops which I have recommended the condition is positively favourable to that process; for there is generally in such cases an apathetic state of the mucous membrane, which permits a free use of instruments required for the removal of fragments.

Some years ago we were, on nearly the highest authority, positively prohibited from turning the point of a lithotrite downwards behind the prostate gland. If this organ be somewhat enlarged, particularly in its middle lobe, there is always a kind of pouch behind; and even when of its normal size the bladder in old persons is capacious at this part. Doubtless, as a patient stands, the stone usually lies in this place. Even in the horizontal position it keeps there, unless the pelvis be raised. In early days it was the custom to lower the shoulders and elevate the pelvis, for the sake of getting the stone towards the upper part of the viscus, where it was thought it could be more readily and more safely caught. Occasionally even yet this position seems desirable; but the fallacy of not searching behind the prostate has been long since exploded. The best of modern sounds, that with the short curve recommended by Heurteloup, was constructed with the view of being turned point downwards into this pouch; and to catch a small stone or fragment with a lithotrite or scoop, there is no more certain manœuvre.

The attitude of a patient, the question if he was to lie on a bed, a sofa, or on a table made expressly for the purpose, were all thought matters of very great importance. Every now and then some little variety of attitude, from the horizontal to the erect, is desirable; but generally, in private practice, a hard-stuffed bed or sofa is all-sufficient. A pillow under the pelvis is sometimes an advantage; more, however, to keep the handle of the instrument above the mattress than with the intention of influencing the position of the stone. In hospital practice, the ordinary operating table answers all requisites, and that constructed for the special purpose of lithotrity may now-a-days be considered as a remnant of a certain amount of claptrap which was associated with the early history of the operation.

The question as to the propriety of giving chloroform in lithotrity seems still a moot one in certain quarters; but I have none regarding it, and having used it in the greater number of the cases I have had ever since anæsthesia was introduced, I can speak of it in unqualified praise. There are many instances in which it is not requisite, but I am certain that without it the operation would be well nigh impossible in a considerable number.—*Lancet, June* 17, 1865, *p.* 643.

52.—ON LITHOTOMY IN THE ADULT.

By William Fergusson, Esq., F. R. S., Surgeon Extraordinary to H. M. the Queen.

The causes of the successful issue or fatal result, of lithotomy, in cases reasonably well selected and operations reasonably well performed, are problems of deep interest. It is now two-and-thirty years since I first performed lithotomy, and, with a large personal experience, I feel yet unable to offer decided opinions regarding these problems. Hearing, as I have occasionally, of wonderful success, I have had my suspicions that the expression has been used by some to indicate the mere extraction of the stone, and not the final issue of the operation. Breaking a stone in lithotrity, and extracting in lithotomy, have, I fear, been taken by some as the standard of success—the issue has been ignored! There seems to me to be a mystery associated with lithotomy that has not yet been solved. For palpable errors there is an explanation; but when, to all appearance, there has been perfection in the operation, and yet death has been the issue, I confess that I have been puzzled beyond measure. I have performed lithotomy without a shadow of strain, tax, or tension on the parts more than the needful manipulations, yet the issue has been fatal; and again I have been conscious of an amount of rudeness such as has made me tremble for the result, yet an

untoward symptom has never once appeared. I have, indeed, seen badly performed operations where nothing but death could have been anticipated, and where the anticipation was realized; but I have known such an amount of force and haggling end successfully that I have been amazed. I have known several strong men pull at a nine-ounce stone for an hour, when the patient has been put to bed well-nigh exhausted; yet on the stone being extracted eight days afterwards the final result was perfect. With such experience as I have I cannot pretend to explain these seeming mysteries. Working on inanimate material, no doubt precise manipulation must be of the most perfect effect; but when the phenomena of life are afterwards involved, the result seems in most instances to be beyond human control. Cutting or sparing certain tissues or parts seems to me of little moment as regards the grand result; yet, as it may be thought that these doubts and hesitations are scarcely becoming on the part of a Professor of Anatomy and Surgery to the Royal College of Surgeons of England, I shall endeavour to put some of my views in a more palpable shape and aspect.

Much stress has in modern times been laid upon cutting that part of the pelvic fascia which is reflected upwards on the neck of the bladder from about the middle level of the prostate; but I am very doubtful of the pathological views on which the objection is founded. I doubt if such division is often made, and supposing it is made, I doubt much the result which has been said to follow as a matter of course—viz., infiltration of urine, leading to suppuration and death. Infiltration I believe to have been the bugbear of modern lithotomists. With my own experience, and all that I have observed otherwise, I declare that I have never seen a genuine case of urinary infiltration. I have seen cases where the surface of the wound has evidently been irritated by the urine, but I doubt if any serious evil has ever arisen on that account. If there has been unhealthy or fatal inflammation, I fancy that it has come concurrently from some other cause—some unhealthy and unforeseen state of the constitution. There is such resemblance between infiltration and diffuse inflammation of cellular tissue, that they are, I believe, often confounded. Serous and lymphatic effusion so much resembles urinary infiltration, that when it occurs in the track of the wound in lithotomy, such a mistake (as I consider it) as that I allude to is not unlikely.

Infiltration of urine, I maintain, never takes place when there is such a free passage as that after lithotomy. The diffuse swelling in cellular tissue, so characteristic of infiltration, is never present after lithotomy, even in fatal cases. Surgeons are familiar with ordinary infiltration, and how readily the urine escapes when incisions are made in such cases. All my experience leads me to say that infiltration is one of the rarest causes of death after lithotomy.

Whilst making this statement I am not disposed to refer lightly to that free incision of the prostate which implies a division of the reflected portion of the pelvic fascia. On the contrary, I advise that it should be left untouched, and declare my conviction that stones of great magnitude may be removed without such free use of the knife. There are those who in this operation have come well up to *Mercutio's* idea of a fatal wound: "Not so deep as a well, nor so wide as a church door." Yet withal the gap has been big enough to be fatal; and both depth and width have often, I fancy, been the cause of evil in this wound. Depth which has gone through the prostate, wounded the bladder when needless, and even penetrated the rectum, or possibly the peritoneum—width being implied by these very features.

Whilst admitting that patients have often recovered after huge and so-called free incisions about the neck of the bladder, I am, however, strongly impressed with the idea that a limited incision is safest. It implies, in my estimation, less injury to important parts. But I scarcely recognise as legitimate any incision in the neck of the bladder which will permit the egress of a stone without at the same time a certain amount of dilatation, or it may be laceration.

Stones of one inch or more in diameter are of very common occurrence; and the circumference, three inches, required for the egress of these, is in pro-

portion. Now, I believe, that if the surgeon aims at making an incision in the neck of the bladder equivalent to the free egress of these without stress or strain, he makes a wound of the most dangerous magnitude, from which the patient is not likely to recover. I dare not, I cannot say that wounds of the membranous portion of the neck of the bladder beyond the prostate are certainly fatal; on the contrary, I believe that they are not so on all occasions; but I am as firmly impressed on this point that there is great comparative safety when a rim of prostate is left at the base of the wound of that gland. I am equally confident, too, that stones of large size may be removed through a wound of this organ which has been, as regards the application of the knife, of comparatively limited extent.

These latter remarks, it will be perceived, bear upon a very interesting and much controverted question as to limited or free incisions in the neck of the bladder. I doubt if this question will ever be settled, but on my own part I have no hesitation in declaring my preference to a limited incision, for I believe as implicitly in dilatation here as I do in the neck and mouth of the uterus in parturition, even though the latter is effected by nature, whilst the former is by force on the part of the surgeon. I am so strongly impressed on this point that I do not object to the term laceration which has often been applied to the somewhat forcible extraction of a stone; for my opinion is that laceration, provided a margin of prostate is left, is safer than the so-called free incision, which fairly divides the left lobe of the prostate and runs into the membranous portion of the bladder. But in advocating a limited incision internally, I am equally convinced that a free external wound is of importance, for it greatly facilitates the deeper and more delicate part of this operation. When the distance between the surface and bladder is great, partly from obesity, partly from enlargement of the prostate, if a free incision be made, the knuckle of the forefinger may be buried in it, so that the point can be more readily placed within the prostate or the bladder, while it permits the more free and safe use of forceps, both in passing them into the bladder and in extracting the stone. Without this free space, there is a risk, in introducing the forceps, of passing them between the bladder and pubes, or between the bladder and the rectum. Moreover, in extracting a large stone, there is a freedom given to the leverage of the forceps, which, whilst much appreciated by the accoucheur, has been too little thought of by the lithotomist.

The difficulties of lithotomy in the adult are, I apprehend, largely of the surgeon's own making. He limits the wound perhaps, making it too narrow throughout, particularly on the surface; or in not penetrating into the bladder, whereby he runs the risk of the blunder of making a space between the pubes and prostate, that which in my lecture last year I endeavoured to impress emphatically as the grand source of failure in the operation in children; or he places it too high in the perineum, thereby setting his work too much in that dangerous strait, the angle of the pubes; or he does not make a proper use of the forceps either in clutching the stone or in drawing it from the bladder.

When the perineum is so deep that the finger cannot reach the bladder, the operator must do the best he can to introduce the forceps, always taking care under such circumstances that force is not used, for if he has cut into the bladder the forceps should glide along with a gentle push. If he has not confidence in simplicity (guided by anatomy) at this stage, he may make use of a blunt gorget, an instrument which seems now to be in a manner out of fashion, although in recent times its use has been revived as an aid in mesial lithotomy.

As to a wound rather high in the perineum, I take but little account of it, provided the operator in using the forceps draws them downwards, and does not persist in working in this space, or even drawing the stone upwards, as has occasionally been done with fatal effect.

The greatest cause of trouble, and even failure. in the actual completion of lithotomy I believe depends upon not grasping the stone. I have often seen all the steps well performed up to this one; but here difficulties, seemingly

unaccountable, have arisen, and the worst evils have followed. I was early impressed with this feature. An hospital surgeon of limited experience operated on a case of stone, and did the work well until he began to extract. Here the easy movements ceased. No sooner did he attempt extraction than the forceps slipped. Effort after effort was made, but all to the same effect. In all surgery I do not think there is so much concentrated energy of mind and body required as in lithotomy, and it is the more exhaustive too, in my opinion, because it has in a manner to be concealed. This was probably the effect in the case I now allude to. The operator, a man of considerable physical powers, became exhausted, not, however, until the lapse of about an hour, and when he and the patient were both well nigh in a similar plight, another surgeon, a master in the art, took the forceps, and extracted the stone with the utmost facility. It was the size of a lemon, and most operators would have had some trouble with it. The fault with the beginner here was, that he did not take the stone fairly within the clutch of the blades; he only nibbled at the end. But the second man put the blades upon its sides; in other words, he inserted them deeper into the bladder, whereby he was enabled to grasp the stone by its middle, so that it could not escape from the instrument, and the small resistance of the neck of the bladder was as nothing compared with the energy of his hand and arm. It has fallen to my lot on two different occasions to extract the stone eight days after failure by other surgeons. In both instances I believe that the cause of failure was that the stone was never properly within the grasp of the blades. It is by no means an uncommon supposition, that the bladder in certain cases encircles the stone so closely that there is no room to get the blades between; but this I fancy is a great error. No bladder that I have ever seen could resist the surgeon's power in expanding the blades, and pushing them in the proper direction.

As to adhesion of a stone to the bladder being an impediment to the fair performance of the operation, I believe it to be a myth. I certainly have seen instances where the bladder and stone have held close approximation by asperities on both, but it never appeared to me that these could for an instant withstand the influence of the surgeon's hand on the forceps.

Cysts containing stones are occasionally met with, and I have seen some remarkable instances; but we hear more of these from bunglers, who have operated only several times, than from those who have had large experience.

The dangers of this operation during performance are indeed few and far between, particularly when the incisions are made with a scalpel of moderate dimensions; but of course this, in unskilful hands, might prove as fatal as the dagger of Jacques, the cutting gorget, or any other ill-proportioned, ill-contrived weapon which has from time to time been used in this classical operation. I give the preference to this instrument over all I know, yet, if rashly used, it may do harm equal to any I ever heard of.

A wound of the rectum I look upon as of little moment as regards the final result of the operation. There is little or no additional danger to life in such a wound; but it is a blot in the operation which should be avoided. The accident has happened to me repeatedly; I have recognised it twice in post-mortem examinations, the patients having died from other causes when I was not aware that it had happened. I have seen it once in the form of a communication between the gut and the membranous portion of the urethra, months after the external wound had healed, and where, being very small, it gave the patient but little inconvenience. Twice I have recognised during the operation that the accident had happened, and in both of these I considered that the rectum had been torn whilst extracting.

The latest writing of a lithotomist may be taken as his last report; and in this sense I may state that Cheselden, having cut in public 213 cases, lost only 20. In my own practice in King's College Hospital, I have cut 100, and of these 15 have died. Mr. Solly has told us within the last few weeks that of 63 cases in all, public and private, he has lost 14. Crichton's statements included both public and private; and as Cheselden did not publish the results of his private practice, we are entitled to infer that the Dundee

practitioner in the nineteenth century was more successful than the reputed hero of British lithotomists in the eighteenth century.

But any humble merit of mine that may be associated with this address and that delivered two days ago is, in my estimation, now to be told. I can give a list of lithotomy, and lithotrity conjoined, which, in as far as I know, has fallen to the lot of few other men in Britain. I have personally by operations treated 271 cases—162 by lithotomy, and 109 by lithotrity. Although having been little more than half the time of Crichton's experience, I have had 70 cases of the disease more than he had. Of the 271 I have lost 47; and that shows a mortality of something more than 1 in 7—not a bad average as operations for stone go; but lithotrity cases being included, I consider it low indeed. And I have now to state that which I look upon as of high interest in the modern history of surgery. Of these 271 cases, 217 were adults; 110 have been treated by lithotomy, and of that number 33 have died; 109 have been treated by lithotrity, and of that number 12 have died!

Here is a contrast between the two operations of lithotomy and lithotrity as practised by the same person. I refer to it as an indication of the favourable progress of surgery during the time in which I affect to glance at its history. In my own hands, whatever clumsiness or skill they may possess, the operation of lithotrity has been considerably more successful in regard to saving of life than that of lithotomy.

If I am not mistaken, this is the first time that such a comparison has ever been made by a British surgeon, who has had large experience in both operations, and I trust that my example may induce others in high places, who have dealt largely with both sides of the question, to give us the benefit of their experience.—*Lancet, June* 24, 1865, *p.* 669.

53.—ON THE TREATMENT OF STRICTURE.

By Dr. William Rutherford, late Resident Surgeon, Royal Infirmary, Edinburgh.

[Unfortunately every radical method of treating stricture hitherto proposed is liable to be followed by disastrous consequences, so that the occasional occurrence of a similar result should not be considered an insuperable objection to a new method of operating, if that operation is not more fatal than the others, and is moreover possessed of several advantages which render it preferable.]

The method of treating stricture proposed by Holt is applicable to all cases where gradual dilatation by bougies has hitherto been recommended, and also to some cases where perineal section has been advised. Over the treatment by bougies it possesses the advantage—important to most patients, but especially so to those met with in hospital practice—of requiring, in ordinary cases, a comparatively short time for its successful completion; and further, although the morbid tendency to the formation of stricture is, in most cases, not removed, the stricture is much less liable to return than where gradual dilatation has been adopted.

With perineal section, Holt's operation cannot be properly compared, for the former will probably never be superseded by the latter,—at any rate, in the treatment of irritable stricture, and most cases of stricture complicated with perineal abscess or fistulæ. That, however, a case of stricture with perineal abscess and subsequent fistulæ may be successfully treated by Holt's method, Case 5 affords sufficient proof; and, further, the same case and Case 8 show that a dense cartilaginous stricture—and Case 12, that a pretty tight traumatic stricture—may be satisfactorily treated in the same manner. Probably, resilient strictures will be found amenable to the same treatment.

With internal urethrotomy, Holt's operation may be favourably compared: here again, however, the comparison is not quite fair, for death has most frequently followed urethrotomy, in cases of irritable stricture, where, had Holt's operation been performed, not improbably, a higher rate of mortality would have resulted. Internal urethrotomy, as may be performed by Maison-

neuve's most ingenious urethrotome, has not been so generally adopted by the surgeons of this country as the merits of the operation would appear to indicate. In France, although its good effects have frequently been less permanent, it has much more seldom been followed by fatal results than perineal section. In the case of ordinary organic stricture, it possesses no advantage over Holt's operation, and, indeed, is not so safe; but for irritable stricture it seems, judging from present experience, to be preferable to perineal section. In internal urethrotomy, the great difficulty has hitherto been, to obtain an instrument which will divide the strictured portion of the urethra, and nothing more; and, moreover, one which can be used in tight strictures. Maisonneuve's instrument, which is far superior to Civiale's, admirably fulfils these indications.

It may, perhaps, be of service to give a short résumé of the treatment generally adopted in the above cases where Holt's operation was performed. If the stricture did not readily admit a No. 3 bougie, it was necessary to dilate it gradually by the passage of bougies until that size could be easily passed. The patient was directed not to pass his water on the morning before the operation, in order that, after the passage of Holt's instrument, satisfactory proof of its being in the bladder might be obtained by the flow of urine through the fine catheter contained in the instrument. This valuable precaution was on no occasion neglected: indeed, so strongly did Mr. Spence insist upon its being attended to, that in one case, where the bladder had unfortunately been emptied just before the time for operation, he, although almost positive that the instrument was in the bladder, postponed the operation until urine accumulated in the bladder. The full-sized dilator was most frequently used, but the calibre of the urethra was always previously measured, in order that it might not be dilated beyond its normal size. The operation was usually performed in bed, as rigors were in that case less liable to supervene.

Immediately after the operation, a catheter of the same size as that to which the urethra had been dilated was passed, and the urine drawn off, thereby proving the completed division of the stricture. After the operation, the safe precaution of not allowing the patient to micturate unless with the aid of a catheter, was adopted in all, excepting Case 12,—(which, although placed for convenience in this report as the 12th Case, was in reality among the first operated upon.) Possibly had this patient been prevented from passing urine over the recently torn surface, the subsequent infiltration of urine might not have occurred. It would not, however, be fair to conclude, that, because infiltration had followed the non-employment of a catheter for two days after the operation in this case of *traumatic* stricture, it would also be liable to supervene after operation in cases of *organic* stricture; for, in the latter, the laceration of tissue is confined to the mucous membrane and the submucous strictural tissue; whereas, in a traumatic stricture, the stricture has usually no such definition, and it is, consequently, impossible to say how far the laceration produced by forcible dilatation may extend; it is, however, even in cases of organic stricture, a safe precaution, and one most acceptable to the patient, for the careful passage of a moderate-sized instrument is much less painful than the contact of urine with the raw surface. In Case 9, the passage of the urine through the torn urethra gave rise to rigors, fainting, and far greater pain than the operation itself did.

After the first two days, a full-sized instrument was occasionally passed to prevent recurrence of the stricture, and the patients were instructed how to pass an instrument, and enjoined to do so, or to have it done, once a month or so.

With regard to the internal treatment, the patient was prepared for the operation by having his bowels moved, if necessary, by a small dose of castor oil,—by rest in bed, diluents, hyoscyamus, and alkalies, if there was any tendency to irritation in the urethra. A grain of quinine thrice daily, with abundance of nourishing diet, was given if the patient's general health was below par; immediately after the operation, a third of a grain of muriate of morphia in a suppository was introduced into the rectum, and five grains of

quinine given thrice daily during the first three days, in order to check the tendency to the occurrence of rigors. Mr. Spence has found this small dose prove quite as efficacious as the much larger dose recommended by Mr. Holt. When rigors occurred, brandy toddy and other hot drinks were given, and hot water bottles were placed round the patient.—*Edinburgh Med. Journal, Sept.*, 1865, *p.* 257.

DISEASES OF THE SKIN.

54.—ON THE TREATMENT OF PRURIGO.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases.

In all obstinate cases, and particularly in the prurigo of old people, I would advise that immediate recourse should be had to a course of strychnia, and after this a course of arsenic. The strychnia may be given in doses of a sixtieth or sixty-fourth of a grain in solution every three or four hours till a decided effect is produced upon the disease, or till nervous symptoms show themselves, when it may be left off: a vigorous course of arsenic should follow it. Nux vomica was a favourite remedy with Neligan, but I believe we are indebted to Dr. Burgess for this mode of exhibiting its active principle. These remedies sometimes act like a specific upon the pruritis, and when this symptom is once thoroughly quelled we shall generally have little trouble in dealing with any change in the skin. I am quite of Mr. Hunt's opinion that prurigo is not such an intractable disease when properly treated, although such an opinion may be looked upon as a modern heresy. Under the old plan of treatment it seems to have resisted every attempt to subdue its malignity, and perhaps there were more suicides from prurigo than from all other diseases of the skin put together. Mr. Hunt says, "of the entire recovery of a patient thus afflicted, or even of considerable alleviation of suffering, not one single gleam of hope can be gathered from any author who has written on the subject. And yet there is no truth in the whole circle of medical science more vividly impressed on my own mind, than that, under proper management, arsenic is an effectual remedy for this disease." Mr. Hunt bleeds to faintness in refractory cases before giving the arsenic; in one case he took about seventy ounces of blood from the arm, and probably fifteen additional ounces by leeches; and certainly his success, in cases which but a few years ago were given up to despair, seems quite to justify his confidence in this powerful means of treatment. He also gives cod-liver oil with the greatest advantage. Lisfranc, in cases where the itching was connected with difficult menstruation, used to bleed the patient a little each time, which always proved an effectual cure in the long run.

Mr. Startin's treatment of prurigo is that of lichen; he relies greatly upon mineral acids, chalybeates, opium, and ammonia, and has never found benefit from mercury or arsenic; on the contrary, they rather do harm. In a very severe case of prurigo formicans recorded by this gentleman, twenty drops of dilute sulphuric acid and ten of Battley's sedative three times a day, followed by tincture of muriate of iron in infusion of quassia and an opiate at bedtime, proved perfectly successful. These means, however, were powerfully seconded by the use of an ointment of white precipitate of mercury and creosote, ten grains of the salt and a few drops of the fluid to an ounce; and after this a weak solution of bichloride of mercury in creosote water, used warm; later on, the ointment was entirely discontinued, and bisulphuret of mercury was added to the lotion. A strict diet, consisting of milk, bread, and boiled meat, was observed. Mr. Startin finds hot-air baths and cinnabar fumigations of great service. Dr. Neligan used to prescribe iron in infusion of hops, with the juice of conium, of which he used to order pretty large doses. He found great benefit from the use of this remedy along with magnesia in senile prurigo.

In some cases, particularly where prurigo attacks the pudendum, scrotum, or anus, the itching is so intolerable that something must be done for it. Mr. Wilson recommends in a general way that the skin should be frequently

rubbed with a damp sponge dipped in fine oatmeal; after this the tincture of croton (made by steeping an ounce of bruised croton seeds for a week in four ounces of spirit) is applied, and after this has been done a few times, a lotion of bichloride of mercury in almond emulsion, fifteen or twenty grains to a pint, will often complete the cure. The Turkish bath is also said to have entirely cured prurigo. Painting the surface with iodine is useful, as is also glycerine applied with a sponge. Bleeding will often act like a charm in allaying the horrible itching, but Mr. Wilson frankly admits that at times all remedies fail to subdue it, so that his experience is rather different from that of Hunt, who treats prurigo as a very trifling matter. For the affection of the pudendum Bateman recommends a lotion made of two grains of bichloride (oxymuriate) of mercury in one ounce of lime-water. Mr. Wilson says the juniper tar ointment is peculiarly valuable in this variety. In pruritis of these parts, injections of very hot water, juniper-tar ointment, blisters to the thighs, and small bleedings seem, from all accounts, to be the most reliable remedies. Mr. Wilson has found an opium injection relieve the irritation after all other means had failed. Strong nitric oxide of mercury ointment, and podophyllin in doses of one-sixth of a grain, are said to have proved of great service in prurigo of the anus. Mr. Startin, in a case related in his lectures, where the scrotum was also affected, directed mucilaginous hip-baths, daily ablutions with yolk of egg and tepid water, the application twice a day of very dilute mercurial ointment, with a few minims of creosote, strict diet, and one-sixth of a grain of bichloride of mercury in cold infusion of hops three times a day. The case was very severe, and occurred in an old man, but a cure was effected in little more than two months. In the prurigo of old people generally Mr. Startin's prescription for external use is a liniment of glycerine and trisnitrate of bismuth, or powdered talc rubbed in with a flesh-brush. Dr. Frazier recommends for trial finely-powdered camphor, mixed with six or eight parts of rice or potato starch, and a small quantity of acetate or carbonate of lead. This is dusted on the skin three or four times a day, and calomel ointment is used at the same time. Latterly Dr. Neligan confined himself almost entirely to chloroform ointment, which seems one of the best, if not the best, ever introduced. It is made by mixing half a drachm with an ounce of cold cream.

Possibly I have had a long series of mild cases; but, as I have already said, I have not generally had much difficulty in treating prurigo, though my prescriptions for external applications are confined to pure soft soap of the pharmacopœia well rubbed in, and bathing directly after with *very* hot water, or getting into a very hot bath. If nothing else be gained, free perspiration is promoted, and though this is no panacea, yet it is attended with relief. Prurigo often arises in persons who have nothing to reproach themselves with in regard to cleanliness. Mr. Startin, indeed, says that this neuralgic itching is perhaps more frequently met with in the respectable walks of life than any other cutaneous affection. Such persons often seem surprised at being told to make a free use of hot baths, but they forget that they do not take sufficient exercise to keep the skin in a healthy state. Elderly people in good circumstances, and people who have retired from business, often seem to think that it is hardly respectable to go beyond a steady walk, and that while their appetite remains good and their bowels are open, nothing more is needed; but prurigo won't yield to such gentle means, and till regular active exercise has become a settled habit the action of the skin must be encouraged. In all cases I think no woollen ought to be worn next the skin.

Hebra views prurigo in the gloomiest light; the picture is filled in with the darkest and most sombre tints that utter despair of success can lend. The patient is doomed—the relentless malady only leaves him in the grave. The complaint is not difficult to cure—*it is incurable!* and all the physician can do is to make his condition a little more tolerable. He seems, however, to have never made trial of Mr. Hunt's plan, and I cannot see how success is to be expected from merely local means. The only remedies which have ever been shown to possess certain control over prurigo are arsenic and cod-

liver oil, and these ought to have a fair trial in every case before it is pronounced incurable. Hebra says placing the patient in a tar or sulphur bath renders his condition more endurable.—*Milton on Diseases of the Skin*, p. 6.

55.—ON THE TREATMENT OF INFANTILE STROPHULUS, PRURIGO, ETC.

[The itching attendant on many skin diseases is so intolerable, and difficult at times to relieve, that any hint is valuable. Dr. Martin, of Bercy, has used the bichloride of mercury as a lotion with great effect. He says:]

"In my young patient I replaced the bath by lotions with a solution of two grains of bichloride of mercury in five ounces of water; but this dose proved too large, and aggravated the symptoms in the same manner as sulphurous baths Disheartened by this failure, after further experiment, I again resorted to lotions with sublimate, but of inferior strength, viz.:

℞. Hydrarg. bichloridi, gr. j.; aquæ destill., ℥v.; alcohol, ℥ij.

The affected parts were bathed three times a day with this preparation, which was allowed to dry on the skin. Prompt improvement followed. I then progressively increased the quantity of the salt, and in the course of a fortnight, on the average, I succeeded in effecting a cure of an affection which in most cases had lasted three, four, or five months, in spite of the exhibition of a variety of remedies which had proved unavailing, or, at best, had given but temporary relief."

We may remark that in the complex disease termed *pruriginos strophulus*, the lichenoid eruption is of very secondary importance, and the prurigo, on the contrary, is the most prominent feature and has a more urgent claim on the practitioner's attention. Now, amongst the local remedies which are especially serviceable in cutaneous neurosis, lotions with corrosive sublimate are unquestionably the most efficacious, and may be resorted to with confidence, of course, in doses proportionate to the peculiar irritability of each patient. It is safer to begin with a weak solution, which can at will be increased gradually, according to its effects.

Mr. Hardy views, it is true, corrosive sublimate baths as an extremely powerful cutaneous sedative; but he also prescribes lotions with the same substance, especially in the localised forms of *Prurigo podicis*, *P. scroti*, *P. pudendi muliebris*. In these affections, and generally in all those, with the exception of scabies, in which pruritus is the most prominent and distressing symptom, Mr. Hardy advises lotions repeated several times daily with four ounces of warm water, to which is added a teaspoonful of the following preparation:—

℞. Hydrargyri. bichloridi, gr. xvi.; aq. destill. ℥iv.; alcohol, q. s.

—*Journal of Practical Medicine, Paris, Sept.*, 1865, *p.* 374.

56.—ON THE TREATMENT OF ALOPECIA.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases.

For the simple falling of the hair in grown persons, which is often due to a mild species of erythema or pityriasis, Mr. Wilson advises that the hair should be cut with great care, and by a person thoroughly skilled in the matter. The tips of all the hairs require to be cut off; this is the grand point. A secondary but still most useful one is the use of a spirit wash and a stimulating pomatum. The same treatment with free brushing is applicable to general falling of the hair as also to area; the head is also to be washed in cold water and dried with a hard towel; it is then to be well brushed, the great point being it seems to brush the skin more than the hair—steel medicines and tonics being also requisite. For this strictly local form (area) Mr. Startin uses, among other remedies, a lotion composed of infusion of white

hellebore, bichloride of mercury, sulphurous acid and glycerine, applied warm with a soft-brush; he has generally found a few applications of this prevent the falling of the hair, and extension of the disease from any patches which may happen to exist. The use of this lotion is followed or accompanied by friction, with an ointment made of two ounces of fresh lard and the same quantity of cold-drawn castor-oil, half a drachm of tincture of kino, the same amount of Goulard's extract, and twenty minims of oil of rosemary. Dr. Frazer employs a lotion consisting of an ounce of aromatic vinegar and six drachms of Spanish flies, united with six ounces of rose-water, and the same of either rum or whiskey.

In all cases of obstinate or confirmed alopecia, my own experience would lead me to rely entirely upon two remedies and on these only; they are the internal use of arsenic and the external use of the Spanish fly. A regular course of arsenic should be given as for an obstinate form of erythema or pityriasis, of which indeed area is only a sub-variety. As to the cantharides, fancy or experience will readily enough dictate some form in which this potent ingredient can be applied. Perhaps Mr. Wilson's unguentum stimulans, mixed with two or three times its weight of scented-leaf-marrow pomatum is as good as could be found; but the cheapest and most certain method tha I am acquainted with is simply to powder the fly and macerate it in glacial acetic acid for a fortnight. The mixture at first should be almost as thick as printer's paste; after a short time a clear fluid gathers at the top, and this, after being strained off and well brushed over the bald surface, or rubbed in with a piece of lint, is by far the most powerful vesicant for a cheap preparation that I know of. A piece of skin the size of a shilling is blistered every second or third day, and in this way the whole of the surface may be operated on without any great soreness being induced. Care should be taken to keep the fluid from the fingers. I have suffered severely from handling it carelessly. Bullin's blistering fluid is also a most valuable preparation, but on account of its cost is not so well suited for hospital or dispensary practice. When patients will not submit to blistering, frictions with a mixture of spirit of horseradish, tincture of cantharides, and Cologne water may be tried instead; but this method is far more expensive and less certain than the other.—*Milton on Diseases of the Skin, p.* 14.

57.—ON THE TREATMENT OF TINEA.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases.

First of all we may eliminate all true pustular diseases of the scalp, that is to say those which really begin with pustules, or of which pustules form the predominating feature, and refer them to impetigo, which is not a contagious disease at all. In some of the scalp diseases, pustules may complicate a true scurvy (or tinea) disease. Next vesicular diseases, if such really exist, for I never saw one, may be thrown out; vesicles, however, may, like pustules, complicate the tinea group. Thirdly, true eczema, or a weeping of the surface, followed by crusts and that form of eczema where there is inflammation of the derma in spots, followed by thick resin-like little crusts, may follow the same path. Thus there remain the true tineæ, dry, scurfy, reddened patches, accompanied by clipping, shedding, twisting, and loosening of the hair, and a secretion of scales, which in favus may even assume the form of crusts; and in this group I would embrace every form, from area to favus, as they are all grades of essentially the same process; the hair-follicles are involved in all, and if there be contagion in a disease of the skin we find it here, and here the vegetable theory must find its city of refuge.

Mr. Hunt considers the diagnosis of these diseases impracticable. "I do not hesitate," he says, "to pronounce it impossible to give a distinctive name to one in fifty, or to determine by its appearance only whether it be contagious or not." I respect Mr. Hunt's opinion in such a matter quite as much as I admire his determination and honesty in speaking out so plainly,

but I think if the rule I have mentioned be adopted the diagnosis will be facilitated.

If I understand Dr. Tilbury Fox's views aright, he contends that the parasitic growths on the skin are as truly vegetable in their nature as the mould upon an orange or melon, because we can distinguish the presence of cellulose externally, and can see internally the primordial utricle coloured by iodine, and because we find in the secretions of these diseases the tubed mycelial thread and a fructification unrepresented in animal structures. Ether, chloroform, and spirits of wine do not affect these parasites, while they make epithelial tissues transparent, and dissolve all fatty matters. The parasites mostly grow in places to which the air has free access, exceptions to this rule admitting of doubt; they grow too alone when removed from the influence of blood and living tissue, a separation which is fatal to animal life. A piece of favous matter, if fittingly nursed and tended, will take root and flourish; the nuclear element will assume the sporular form, the sporules will gain in size, join, bud, and bring forth mycelium, while the very structure on which they are seated is decaying. Dr. Fox showed his friends a beautiful specimen of the tricophyton tonsurans which had blossomed sweetly, while the fibres of the hair on which it was seated had degenerated, and their place had been supplied with beautiful tufts of crystalline fatty matter.

Dr. Fox further argues: "Take away the damaged hairs, &c., of tinea, and nothing diagnostic of the ravages of the parasite remains. *Per contra*, remove all the eruptive and the parasitic remains in its integrity: the necessaries remain, but the accidentals are absent. The presence of the fungus increases the local irritation. . . . Now if the fungi be different there must be different soils, and the treatment be different in kind. If the fungi be identical in nature, then the treatment is a matter of degree only; if fungi be different, then ought they to be so many indices to states of economy, states which in time will be fully interpreted."

Dr. Fox argues, that to cure the disease and to destroy the fungus you must root out the hair. Pull out every hair that is diseased, by gentle means if you can, but pull it out. Dr. McCall Anderson is also unreservedly in favor of depilation, both in tinea tonsurans and sycosis. Dr. Hillier says that tinea tonsurans and tinea favosa cannot exist without their respective fungi. These we must destroy, and nothing destroys more promptly than compound sulphur ointment. M. Bazin saw no safety but in pulling out the hair, and there can be no doubt that these views have found a pretty wide acceptance.

Mr. Wilson gives us his treatment of these diseases. Chloasma and sycosis being elsewhere touched upon, I propose to omit all mention of them, and confine myself to what has reference solely to tinea and favus. Mr. Wilson's general directions then are: Remove the crusts and furfuræ by thorough washing with juniper tar or petroleum soap, or by the use of a liniment consisting of equal parts of soft soap, juniper tar, and alcohol; wash daily with tepid or cold water to cleanse the diseased parts and gently stimulate the skin of the head; comb the hair and brush it when it is not too sensitive; rub in night and morning a very dilute ointment of nitric oxide of mercury; give tonics, cod liver oil, and iodide of iron, arsenic, &c.

Mr. Wilson then goes on to say: "And this brings us to the question of the use of depilation: that it disposes to the cure of the local disease there can be no doubt. We cannot disbelieve the statements of Bazin that he succeeds in curing these diseases through the agency of depilation; but we do and must disbelieve that the removal of the fungus is the proper explanation of the cure. We see in depilation a stimulant, and a valuable stimulant—a stimulant that we ourselves commonly use when we seek to make a black hair grow up in place of one that is white—a treatment that for this purpose we have been in the habit of using for years, and with remarkable success; so that we have been led to regard avulsion as one of our best, indeed our only reliable remedy for producing *deep stimulation of the cutaneous tissues*, for setting up a new action in the papillæ of the hair; as the only remedy, in fact, by which we can reach successfully the fundus of the hair-follicle.

And this, in our belief, is the 'methodus medendi' of depilation and avulsion in the phytodermic diseases. It sets up a new action, a healthful inflammatory process in place of a morbid inflammatory process—a process whose natural course is to end in resolution, instead of one whose nature is perpetuity. For morbid cell-genesis it establishes healthy and plastic inflammation and sometimes healthy suppuration, when, as Bazin declares, the fungus is starved or drowned. Even Bazin has recourse to avulsion as a stimulant in old cases of mentagra, wherein the hair acts the part of a foreign body, of a thorn, in the skin, and so keeps up irritation."

Mr. Wilson's particular treatment of tinea is sulphur and cream of tartar; cod liver oil, especially if there be struma, citrate, acetate, and hydrochlorate of iron, iodide of iron, iron with quinine, nitro muriatic acid, a good diet, and plenty of porter.

The hair is to be cut, and the crusts to be detached with water-dressing, the head is to be washed with old-fashioned yellow soap. The patches are then well painted with tincture of iodine or strong acetic acid or vinegar of cantharides. After this an ointment of one of the nitrates of mercury is well rubbed in. When ointments are objected to, a lotion of sulphuret of potassium, a drachm to a pint, or of bichloride of mercury, a grain to an ounce, may be used instead. The dry scurfy state of the skin which may remain is generally removed by dipping the head in cold water, and rubbing in some simple pomatum.

Dr. Frazer recommends vesicating collodion and strong iodine paint for the patches of tinea. Dr. Fox speaks highly of carbolic acid for this complaint, as does also Dr. Frazer; it dissolves freely in alcohol, ether, or glycerine, but very sparingly in water. Mr. Startin uses a compound sulphur ointment of his own, and Dr. Jenner recommends one in all essentials the same as Mr. Startin's, composed as follows: "half a pound of sulphur, half an ounce of ammonio-chloride of mercury, and the same amount of sulphuret of mercury, four ounces of olive oil, sixteen ounces of lard, and twenty minims of creasote."—*Milton on Skin Diseases, p.* 16.

58.—ON ECZEMA, AND ITS TREATMENT.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases.

[Almost up to the present time eczema has been classed with scarcely a dissenting voice among the vesicular diseases. These vesicles, however, are by no means an essential feature of eczema. In a paper read now nearly four years ago before the Medico-Chirurgical Society, this view was advocated by Mr. Milton, who considers that more accurate observation of the disease will convince the profession that true eczema is *not vesicular* at any period of its existence.]

Three circumstances, however, were noticed which may have contributed to support the view generally taught.

1. The first is, that when the discharge comes from a part furnished with hair, as the leg for instance, minute accumulations of fluid form at the junction of the hair with the skin. These, I assume, are the so-called vesicles, and M. Devergie seems to be of this persuasion. But on more close examination it will be found that neither their course, situation, nor form, correspond with those of a true vesicle, and on touching one of them *gently* with a glass tube, a minute drop of serum is found to adhere. I have always failed to detect any trace of cuticle on these little drops of fluid; certainly if there were, the pressure was much too slight to break it, whereas we know that the covering of the vesicles will resist some force. When M. Devergie speaks of the fleeting nature of the vesicles of eczema, he overlooks the fact that genuine vesicles are not very fleeting even in such transient disorders as miliaria, and that those of herpes, with which he classes eczema, not only bear such distension as to become globular without bursting, but endure the friction of the clothes several days without rupture. Besides, however often herpes may reappear, it always does reappear in the form of vesicles, where as I have never seen an outbreak of vesicles on a patch of eczema.

2. The second circumstance is, that in the vicinity of eczematous patches, vesicles, and even bullæ, are occasionally found; the former not unfrequently appear about the knuckles or back of the hand. These vesicles, however, last but a very short time, and in all the cases I have seen their return or reappearance was very uncertain, and not at all dependent upon the state of the eczema. Whether they arise from the irritation of the disorder itself, or from the use of ointments or lotions, could not be determined, as in general more than one of these exciting causes was in operation at the same time. These vesicles were always rare in proportion to the number of spots occupied by the eczema; that is to say, a patient with them might have thirty or forty eczematous patches, and only one near which there were vesicles. They healed quickly, and did not become eczematous, and the disease in their immediate neighbourhood remained uninfluenced both by their outbreak and departure; sometimes appearing afterwards, in its usual form, on the very spot where they had shown themselves.

3. The third reason is that a disorder which I consider a form of herpes, has been confounded with acute eczema. It begins with heat, severe itching and pungent stinging pains. Vivid red patches form, serum is effused under the cuticle, which is raised up in firm irregular vesicles, then the serum becomes of a whitish hue and grows turbid, the vesicles break, the fluid oozes out, the cuticle is next thrown off, leaving the skin red and tender, and the disease passes away mostly, I believe, to return no more. In still more severe cases numbers of little pustules form round about, and the irregular vesicles speedily grow turbid and break, forming really an impetiginous herpes. But except that the vesicles run into a mass instead of being separate, I see nothing that distinguishes this complaint from herpes zoster; it disappears almost, if not quite, as fast when nothing is done as when it is actively treated; the vesicles are firm and only break when the fluid has become turbid and flaky; at the end of two months' duration of the disorder in a chronic form, I have seen some of the vesicles still full of fluid.

The acute form of eczema is not very common, but its course and symptoms separate it readily from vesicular diseases, and ally it closely to an acute erythema or erysipelas, or that form of inflammation described by Mr. Wilson as the scalled head. In this affection a large portion of the skin, as the forehead for instance, becomes red, swelled, glazed, and stiff. This is followed by desquamation of the cuticle, and the formation of moist scales or even crusts; there even may be a slight though decided weeping, which is more frequently the case if it attack the leg. I once attended a gentleman who in the course of eighteen months had several attacks on the face, the whole surface of which generally suffered; but on no one occasion could either he or I detect vesicle. This form of eczema appears to me to form a pretty exact link between erysipelas and erythema.

Now, the following appears to be the process which ensues in all cases of a truly eczematous nature. A portion of the skin becomes red, inflamed, and uneasy, stiff, and itching, but rarely swollen except when the complaint attacks the ear. The cuticle rapidly dies, and is cast off, or torn off by scratching. To this succeeds a discharge of serum which, judging from the appearance of the orifices whence it exudes, is poured out by the sudoriporous ducts. When the process is more slow, the falling of the cuticle is succeeded by a cuticle thicker and coarser in its texture, and gradually assuming the look of a soft scale. For the most part the redness starts from several points or papulæ, which are neither so acuminated as those of lichen, nor so dark-coloured as those of prurigo, and which may rapidly, often within twelve hours, spread and coalesce into a patch. In some cases of old standing the eruption at a very early period, particularly when seated on the backs of the hands and wrists, presents the appearance of numerous minute, low, separate elevations of thickened red skin, partly denuded of all covering, and partly covered by discoloured but firmly attached cuticle, and sometimes indented so as to assume a cup-like form. For the most part, such spots, when torn or rubbed, do not discharge serum by any means so abundantly as those beneath the thicker and moister flakes or crusts, which often pour it out in a surprising quantity.

I would venture to define eczema as a non-contagious inflammation of the

papillæ of the skin, attended with increased nervous sensation in these parts, augmented and abnormal action of the perspiratory ducts, the contents of which often exhibit pus globules and plastic lymph, and increased secretion of very imperfectly formed epidermis, standing nearer to erythema and pityriasis than to herpes or impetigo, though capable of being complicated by both; divisible in its natural state into two great forms, the acute and chronic, between which it is not always easy to draw a clear and positive line of separation, instances, out of both divisions, of cure under the same form of treatment being numerous enough.

On the Treatment of Eczema.—[In the treatment of eczema it is not uncommon to find an author speaking of twenty or thirty powerful medicines—the use of many of which is quite superfluous. Tar, antimony, guiacum, and sulphur may be given up. Antiphlogistic treatment is injurious, as severe eczema never occurs without a certain feebleness of health.]

Cod liver oil is one of the most valuable remedies in eczema as in so many other diseases of the skin, and, perhaps, it would hardly be going too far to say that every chronic case is benefited by it, if taken long enough and in a proper way. Eczematous patients are generally very sensitive to cold, and means calculated to evoke heat seem to agree best with them. The dose should not be too large, never more than the stomach can bear comfortably; grown persons seldom require more than three or four tablespoonfuls daily; children bear relatively a much larger quantity, and may take, even when quite young, two to six teaspoonfuls daily. The great obstacle to its use, the nauseous taste, and the idea of swallowing a coarse animal oil, are soon overcome, and the result compensates the patient for any disgust felt at the outset. Should the difficulty of taking it inwardly be insurmountable, as when it always brings on sickness, or when the patient is an infant at the breast, a broad fold of linen steeped daily in the oil, may be worn round the waist. The worst of this plan is that it makes such a filthy mess, and creates such an unpleasant smell, but it certainly does good, and children will get well and fat on oil used in this way.

On arsenic alone, it may be safely said, no reliance can be placed at any stage except the last, and not always then. Even Mr. Hunt, enthusiast as he is about the value of this mineral in lepra or lupus, has much less to say in favour of its power over eczema. It often proves of great service when the skin remains red and tender, with a quick reproduction of unhealthy cuticle or scabs, great itching, and a strong tendency in the disorder to stand still. This and this only is the stage for which I would recommend it, and here I think arsenic is invaluable.

It may perhaps excite some scepticism when I state, that some of the patients who could not at first support the action of arsenic took it comparatively well after a course of tincture of steel. This was peculiarly the case with adults. I never saw it in children, who indeed bear arsenic very well. In my own case, five minim doses of the liquor arsenicalis soon brought on such irritation of the stomach, such nausea and purging, that I had to give it up at the risk of seeing the worst symptoms return. But after a good course of quinine and steel these doses induced no discomfort, except in the conjunctiva, an not much there. I believe, however, that this toleration of arsenic is quite artificial; after a respite of a few months from all medicines I have seen the same quantity of it produce the same effects.

Steel. I now come to the remedy which, in my hands, has proved of more advantage against the eczema of middle or advanced age than any other. I have already stated the circumstances which first of all led me to make trial of steel, and as they most probably would not interest the reader I omit them here. I had long felt, that as eczema has little tendency to bring on structural change, that as it is a disorder rather than a disease, the secret of its inveteracy lay rather in our not being able to find a suitable medicine than in the nature of the complaint, and the speedy removal of a most obstinate eczema by very simple means confirmed this view.

The patient was a shoemaker, suffering from eczema of the ankle, the skin of which was so tender and cracked that for long, even when it did not discharge, he had never been able to walk in any comfort. The itching tor-

mented him incessantly, and having endured for years, bade fair to last as long as the patient himself. All the usual remedies were tried in this case, among others tincture of cantharides and sulphur, and antimony in various forms. Tar also was given, first in pills and then in boluses, of which he swallowed a tolerably large stock, though they always occasioned a scalding sensation when at stool. Nitrate of silver, lead ointment, and nearly all the mercurial ointments were applied outwardly, but with so little effect that at the end of several months the patient disappeared, not only uncured but unrelieved. About half a year after I met him looking quite well, and learned that he had been quite cured by the use of Streatham mineral waters. He promised to send me a bottle of this water that I might have it analyzed, but he failed to keep his word, and to this day I do not know what Streatham water is.

The observations and trials made upon the different preparations of iron have now been continued for several years, and after the usual amount of disappointment from losing sight of a patient just as some interesting case was drawing to a close, from conflicting evidence and conclusions disproved by later evidence, the following results were, I think I may say, pretty decisively established :—

1. It was found that the steel wine, carefully prepared and given in doses of one or two drachms two or three times daily, sufficed to remove most cases of eczema during the first year or two of life. It was, however, necessary to use the utmost care to ensure fresh air and proper food, as it is not always so easy to make people understand that invalids require these just as much as healthy persons do. The wine should be very carefully prepared, as I have tasted specimens of it so harsh that I do not wonder at its creating distaste in children. A very agreeable preparation is made by Messrs. Bell, of Oxford Street.

2. When children have passed this age, steel wine requires to be given in large doses to take any effect, and therefore becomes rather an expensive medicine, especially in hospital practice. Here the saccharine carbonate may be substituted, or the tincture of the muriate in doses of five to fifteen minims, well sweetened to suit the stomachs of these rather refractory patients, or the ethereal tincture also well sweetened may be used. The great point is to give the iron in a soluble form of tolerably pleasant taste and small bulk. If the sweetened carbonate be chosen, two or five grains may be ordered two or three times a day. These preparations of iron generaly suffice with patients suffering from eczema up to the age of puberty; if, however, the bowels be confined a small quantity of mercury and chalk may be taken occasionally. This preparation is well suited for scrofulous children, it may be continued for a great length of time, it has the advantage of being tasteless, and does not interfere with the use of steel.

3. From this period (puberty) upwards more powerful remedies are called for, eczema being a more formidable affair at puberty than at childhood, and at manhood than at puberty. To balance this, however, the patients have now reached an age when, among girls particularly, the question of personal attraction is becoming one of the most important in life, so that there is seldom much difficulty in persuading them to swallow medicine somewhat less agreeable. I have generally found that these patients recover under the use of steel mixture in doses of two to four drachms two or three times a day. Perhaps owing to this being a very vigorous time of life, and to these patients being very favourably situated for getting fresh air and active exercise, eczema, so far as I have been able to judge, is neither so severe nor so refractory at this time of life.

4. In persons who had reached middle age or the decline of life, the tincture of the muriate of iron was often a great service, and the benefit was frequently so marked as to excite the notice of those who witnessed the employment of it; indeed, when properly aided by food, exercise, baths, and aperients, it may be safely said that few, if any, cases of eczema will, in the long run, resist its influence. It is, however, essential that it should be prepared not only according to the London pharmacopœia, but with such

care as to ensure that no amount of free acid is present, which frequently preponderates to such an extent that the tincture cannot be given in the proper doses; indeed a good deal of the trash sold under the name of tincture is almost wholly composed of muriatic acid. When properly made, a drachm or more may be given at a time, whereas half a drachm of the coarse acid tincture will set the teeth on edge and make the patient feel sick. I have often known a patient, who was taking the pure tincture with benefit, turn quite ill after a dose of this stuff; several times severe vomiting has followed, and one gentleman was so sick and was purged so violently from taking two drachms of the common tincture that his family thought he had got the English cholera. This kind of thing has now happened so often that I always beg of patients, if they run short of the genuine tincture, rather to do without it for a day or two than get it from any source they are not certain about. The tincture of the perchloride of the British Pharmacopœia is said to be a superior preparation, and I should think such was the case, but I have not had sufficient experience of its action to be able to decide.

The tincture must not only be given in pretty large doses, but it must be given for some time. It is of no use prescribing twenty or thirty drops for two or three weeks; as well not give it at all. It should be prescribed by the measured drop, and used for two or three months at least. Now and then the patient may rest, if he have grown sick of his medicine, or suffer from dyspepsia, till the difficulty is set right again, but so soon as this is done the tincture should be resumed. If it cannot then be borne, as will sometimes happen with persons of feeble digestion, it should be abandoned in favour of some other preparation of steel, such as the ethereal tincture or Griffith's mixture.

When it is doing good the health often visibly improves under its use, the skin grows clearer and the muscles become firmer; while in some cases the face loses the careworn look it had acquired from long-continued ill health. At times, however, not only does this improvement cease without any evident cause, but a decided relapse will ensue even when the patient is taking the steel with every precaution. In this case there is no choice but to abandon it for a time and give some other tonic, such as quinine or cod liver oil. But it should be resumed as soon as possible, and continued till the moist state of the affected parts has quite passed away, and the dry stage been reached, when it may be given up for arsenic.

The objections to the tincture of steel are that it induces constipation and dyspepsia, and that patients get tired of the bulk of the dose and of constantly swallowing medicine.

The constipation may be easily remedied by the occasional use of the mild aperient pill previously spoken of: frequently it passes off spontaneously when the steel has been taken for a short time. The dyspepsia, the most frequent form of which is marked chiefly by coldness of the stomach and nausea, followed by some degree of griping and flatulence, is generally soon vanquished by leaving off the steel and giving some mild aromatic and antacid. There are so many of these, and their properties are so familiar to the reader, that I presume it will be quite unnecessary to specify them more minutely. To get over the aversion felt for the steel, the only plan is to give it up for a while; during this, if the patient feel very low, a short course of quinine or ammonia in infusion of cascarilla may be given; or, if the liver be sluggish, the nitro-muriatic acid in infusion of gentian.

I wish the reader, however, to understand that all these precautions are rarely required except in severe chronic or recurrent eczema; in the majority of cases, provided a soluble form of steel be given, and provided it sit lightly on the stomach, and do not interfere with the action of the bowels, I believe it does not so much matter what preparation of steel is used. At St. John's Hospital I have for a long employed a simple solution in acid; a few pieces of iron perfectly clean and weighing two ounces, or, what is still better, the same weight of iron wire drawn by the magnet, are dropped into a pint of strong muriatic acid of 1160 specific gravity; at the end of a few days a very saturated solution is formed, which is made up for use, and of this ten to fifteen minims are given twice or thrice a day to an adult. This

preparation answers very well, but, if the reader know a better, by all means let him keep to it, in the full belief that from his favourite prescription he will obtain all the good he might expect from the one I have spoken of; what is more, if he give it properly, he may feel confident that he will always do a certain amount of good in eczema, and if the patient's system should not answer to the helm, then, I think, he may rely upon it that a suitable time for giving steel has not been chosen, and that no preparation of it is likely for the moment to be of service. Such, at any rate, are the conclusions which, I think, my notes warrant me in drawing.

Supposing, however, that steel has been given, and that the patient has not improved as the surgeon could wish, what is the proper step to take? Decidedly, I should say, give cod liver oil and arsenic. But if even after a course of these, eczema, though much better, be not quite cured and wear an aspect that unmistakeably threatens a relapse, what should be done? Shall we send the patient for change of air, or give tar, cantharides, and mineral waters? I reply, that forty-nine patients out of fifty cannot get change of air; and as to the Spanish fly, tar, &c., I believe that their virtues, if they possess any, are powerless against these inveterate cases, and they are by no means imaginary ones.

I believe that no good whatever is likely to come in such a dilemma of again using medicines already tried in larger doses and fresh combinations. The antidote for this relentless nuisance is, not a remedy, but a course of treatment which will give it no rest; success is only to be secured by watching each step of this course, and whenever the power of any part of our treatment begins to flag, then in going further, but further only in a prescribed and foreseen direction.

At this stage the mercury comes to our help, and a short course of it now will often be productive of very good effects. I have, however, so far only seen this action manifested by two salts of mercury, namely the biniodide and the bichloride. I scarcely know which is the more powerful of the two, but, on the whole, I think the bichloride is, and Mr. Startin seems to have had great success with it. About an eighth to a sixth of a grain of either will generally prove sufficient for a dose, which may be given once, twice, or thrice a day, according to the severity of the complaint or the age of the patient. I have given young boys and girls a sixth of a grain daily with the best consequences. The mercury may be ordered with a sedative and aromatic to prevent griping, perhaps two or three grains of extract of lettuce, with a strong essential oil, as of cassia or peppermint, will serve this end as well as anything. When much pain of any kind complicates eczema, or when there is a tendency to diarrhœa, a quarter of a grain of pure opium may be added. If the form of a pill be objected to, the bichloride may be given in tincture of cinchona, a grain in each ounce; the biniodide can be prescribed in solution by mixing bichloride of mercury and iodide of potassium. After a short course of either of these salts, the arsenic may be resumed with the best prospect of success.

Mr. Wilson's treatment of eczema rubrum is first to put in force antiphlogistic treatment, and follow this up by the use of nitro-muriatic acid in bitter infusion. The external means which he employs at first are lead lotion, with the addition of a little spirit, and when the acute stage has passed away, he prescribes the oxide of zinc ointment mixed with liquor plumbi; this he follows up by the nitrate of mercury ointment, diluted with an equal quantity of simple cerate. For chronic eczema his remedies are bitters and alkalies, accompanied by gentle aperients and alteratives, to which succeed five minum doses of Fowler's solution given three times a day. Locally he uses here the benzoated zinc ointment with rectified spirit, a drachm to the ounce, and after this directs the patient to rub in the nitric oxide of mercury ointment diluted with spermaceti ointment, in the proportion of two-thirds of the latter to one of the former. It is well known how successful Mr. Wilson is in his treatment.

Mr. Startin's treatment of eczema is much the same as that for impetigo, and which the reader will find in the section devoted to that complaint; and that he relies very much on baths, using vapour baths, daily tepid ablutions

with yolk of egg, mucilage or bran water; he places great confidence in the bisulphuret of mercury in ointment, and for children when there is great irritation a mixture of Goulard's liquor and cream.

External Applications in Eczema.—In acute or slight eczema almost any mild astringent, especially if it possess some amount of soothing power, will suffice. Oxide of bismuth in elder-flower water or camphor mixture, a drachm to eight ounces, or the liquor of the diacetate of lead in the same fluids and the same proportions, form a useful lotion. When expense is not an object, half an ounce of glycerine should be added, as from its faculty of retaining moisture it powerfully aids the purpose lotions are intended to serve. When there is a large weeping surface, especially if of long standing, the reader may try Dr. Hughes Bennett's plan. It consists in applying a solution of carbonate of soda, half a drachm to a drachm in eight or ten ounces of water, by means of a piece of lint soaked in the solution and laid on the part; it must be covered with thin gutta percha to prevent its getting dry, or else be entrusted to the care of some person who will see that it is kept moist, for which purpose the lint may be simply sprinkled continuously with cold water, the lotion being only occasionally applied. If this plan be properly carried out, if the lint be kept wet all day long (and all night too, if possible), the effects are often marvellous; but if it gets dry the soda soon irritates the surface; the addition of glycerine, half an ounce to the eight ounces, will mitigate this to a certaint extent, but nothing compensates for want of attention. Dr. Wallace has reported some cases in which the excellent results of this plan were very marked indeed, but the reader will not fail to observe that one of the very instances in which its action was at first so manifest is reported at the close as "almost cured." My experience of it is that, though it gives great relief, yet that it has very little curative power; that it is only suitable for a large wet surface, and that it is apt in eczema of the head to give a bad cold.

In chronic eczema, if the surface be much inflamed, a lead lotion may be applied at first, or the solution of soda as above stated. So soon as the discharge and inflammation are checked, Mr. Wilson's zinc ointment forms an admirable dressing; it is rubbed down with spirit and smeared like cream on the part. When once ointments are begun with there should be no more water used, except when eczema is discharging freely from the head, as happens especially in children. At such times the ointment, if applied too freely, causes the discharge to be retained under the crusts; a white bread poultice and then a daily washing with yolk of egg will soon remove this, and the ointment can be resumed. It is a good plan to use some means for preventing the ointment from being rubbed off, and protecting the tender surface which it covers from friction against worsted flannel or cloth. To this end the surface can be covered with linen or tissue paper. When eczema has reached the dry stage, the ammonio-chloride of mercury is a very useful application, and for an efficient preparation is perhaps the cleanest and freest from smell of any we have; but in point of efficacy, I would place it decidedly below the dilute nitrate of mercury ointment, which is the most valuable preparation at this stage of any that I am acquainted with, and, to my thinking, superior even to the red precipitate ointment, in which, however, Mr. Wilson places great faith. It is absolutely necessary that it should be fresh made, and a small quantity of olive oil should be added to render it rather fluid. If the eruption be seated on the head, the hair should be cut off in children and the ointment applied night and morning. But grown persons cannot, or will not, always submit to this; and here I would suggest the use of chloride of zinc for a few days. The head is washed daily with hot water and yolk of egg, then with clean hot water, and after this the affected parts are bathed with solution of chloride of zinc, a grain or two to the ounce, a little creosote or gum acacia being added. After this the ointment may be begun with; the hair should be parted, any crusts gently detached, and the ointment lightly rubbed in. Some persons with a strong tendency to eczema, or after the cure of the worst part of their complaint, suffer a good deal from a chapped, red, and tender state of the skin. Cold cream is often very effectual, or the patient when going out may dust the surface over with rice

powder, to which a small quantity of camphor and oxide of zinc has been added. When the hair is falling from eczema, I know of no remedy so effectual as cutting it quite short and blistering the skin as for alopecia.

The preparations of tar, creosote, &c., seem to merit special consideration; nearly every writer recommends them, and I believe few use them. In creosote I confess at once I have very little faith; I never saw any particular benefit from it in any skin disease. Tar makes such a filthy mess that I doubt if it will ever come into general use, and how Hebra can get his private patients to rub in a mixture of tar and cod liver oil, quite passes my comprehension.

To relieve the itching of eczema, Mr. Wilson recommends juniper tar ointment, as also emulsion of bitter almonds with prussic acid; but I believe the only effectual remedy for the itching is the cure of eczema, and this holds good of another disagreeable symptom spoken of by Mr. Wilson, a kind of neuralgia or rheumatism of the skin, which I have certainly seen, though very rarely. Mr. Wilson says he has found no remedy for it equal to a solution of nitrate of silver, a grain in an ounce of spirit of nitric ether.

People say ointments are barbarous and apt to become rancid. But a remedy which promotes the patient's comfort and a cure of the disease cannot, in fairness, be called by so hard a name; and when an ointment is rancid, either there has been great neglect or bad materials have been used. Mr. Wilson relates, that having been consulted by the surgeon of a London hospital, he ordered him zinc ointment, which the surgeon procured from his own hospital; Mr. Wilson, curious to know what a hospital's idea of zinc ointment might be, asked to be favoured with a view of a specimen, which he found to be composed of a material much more fitted for greasing wheels than for applying to the human skin.

This neglect is all the more unpardonable, because very simple means will secure efficiency. Thoroughly purified lard only should be used, and this is only to be had by paying a higher price than usual; the addition to each ounce of lard of ten grains of gum benjamin will prevent any rancidity. The pure gum or tears should be selected, these are broken up and digested in the lard for forty-eight hours. This mixture is afterwards strained, and then the active ingredient is added. For zinc ointment the oxide is used, and Mr. Wilson prefers the buff-coloured oxide of the pharmacopœia to the white oxide made by combustion. I think, however, anything to be used for either the hands, head, or face can scarcely be too free from colour, and as the white oxide can now be had very pure, I decidedly prefer it.

I appeal to the experience of the best authorities in these diseases, whether ointments do not agree better than lotions or any other applications. It is well known that for many years both Mr. Wilson and Mr. Startin have devoted the greatest care to this specialty, and they both use ointments freely. But the reader has only to apply them properly in order to satisfy himself that in eczema and some other affections they not only exert a decided action over the disease, but that they form the most perfect defence against the irritation which the contact of air with the diseased surface is apt to set up; a point of no slight importance if the weather be raw, or very dry and cold. Mr. Wilson thinks they prevent the great oxygenation of the blood, which is apt to occur in inflammation of the skin. They cannot be too sedulously employed in eczema; even when in the house the face, if affected, should be covered with oxide of zinc ointment, white precipitate ointment, or cold cream; if the hands be affected the patient should avert the rubbing off of the ointment by wearing gloves with the tips of the fingers cut off.—*Milton on Skin Diseases, p. 27–58.*

59.—ON THE TREATMENT OF PSORIASIS AND LEPRA.

By Dr. T. M'Call Anderson, Physician to the Dispensary for Skin Diseases, &c., Glasgow.

[Although as a rule the constitutions of patients suffering from psoriasis are sound, yet their health is generally, for the time being, below par. In the treatment attention must always be directed to this point. Many so-called alteratives have been used.]

In prescribing a course of *constitutional* treatment, the fact must be borne in mind that, as far as we yet know, there is no medicine which is capable of eradicating the predisposition to the disease, and that our efforts must be directed towards the removal of the exciting causes. Before subjecting our patients, therefore, to a course of alterative treatment, it is necessary to find out, if possible, the exciting causes, and to endeavour to remove them. Thus, one man, predisposed to psoriasis, may lead a very sedentary life, be much harassed by business, and his mind be constantly on the rack. He takes his meals at irregular intervals and sleeps badly. His digestive and nervous systems are deranged, and out comes his enemy. It would be injudicious in most cases to subject this man to a course of alterative treatment, in the first instance; but we make him give up business for a time; we send him away for change of air—we shall say to Harrogate, if it is the season and there is plenty of cheerful society, for this has a very beneficial effect upon such cases. The torpidity of his digestive organs is corrected by a judicious course of Harrogate waters, or what is nearly as effectual, by the administration of some aperient medicine in the shape of cream of tartar and sulphur, or a pill containing a little calomel; he gets plenty of fresh air, he takes his meals at regular hours, goes early to bed, rises early in the morning, and has nothing to harass or worry him. It is wonderful, after a few weeks, in what a different state of health he returns. In some cases the eruption has disappeared or is fading; but if not, it is then time to prescribe an alterative medicine, such as arsenic.

Wilson cites a similar case, that of a surgeon who was affected with lepra complicated with debility and malassimilation, and which was removed by the mere administration of a course of nitro-muriatic acid and gentian.

In fact, we shall generally find, if we examine our patients carefully, that though, as a rule, their constitutions are sound, the eruption has been called forth in consequence of their health being below par; and this we must always, in the first place, endeavour to correct. In doing this we must be guided by general principles. If we have succeeded, and the eruption persist notwithstanding, we must then make use of the so-called alteratives.

Those which are much used, and which are in my opinion useless, are tincture of cantharides, decoction of dulcamara, balsam of copaiva, oil of turpentine, tar water, and tar pills. Those which I have found of service are arsenic, cod liver oil, and alkalies.

The use of arsenic in the treatment of psoriasis is so well known, and so thoroughly appreciated by those who have had much experience of it, that it is unnecessary for me to dwell upon it. Fowler's solution is the preparation which I am most in the habit of using, and I do not think that any other form of arsenic has a more beneficial effect. It may be given to a child of seven years of age in doses of two or three drops, to an adult of five or six thrice daily after food. It agrees better with the stomach if it be given in a bitter infusion—as infusion of cascarilla or gentian, in doses of a drachm to half an ounce. The patient should be warned to be careful of catching cold while using it, to take a nourishing but unstimulating diet, and to attend to the state of his bowels. Every three or four days the dose should be increased by a drop, till the disease begins to yield or till the medicine disagrees. I am convinced that arsenic would be found much more serviceable if physicians would continue it steadily for months, and not to be induced to stop it without some very pressing reason. In some cases asiatic pills may be used instead of Fowler's solution; namely, when the patient prefers

pills, and when one wishes to conceal from him what he is taking, for Fowler's solution is well known to the public to be a preparation of arsenic.

There can be no doubt that arsenic is the most valuable internal medicine which we possess in the treatment of psoriasis, and that few cases, in which it is patiently and perseveringly employed, altogether resist it, while in many instances the eruption completely vanishes. The more recent the eruption, the more readily it is acted upon by arsenic, while chronic cases to which there have been many relapses are sometimes very rebellious.

Next in efficacy to arsenic in the internal treatment of psoriasis, though applicable to a different class of cases, comes cod liver oil. By the majority of dermatologists this medicine is not even hinted at as serviceable in this complaint, while Hebra of Vienna only mentions it in order to state that it is useless. There are many cases, however, which are greatly benefited by it. In those rare cases in which the eruption appears in strumous persons, there can be no doubt theoretically that cod liver oil should be used, while I have myself had practical demonstration of its value. If my readers will refer to my remarks on the cause of psoriasis, they will there find the account of a remarkable case of very extensive and old-standing disease occurring in a hunch-back, where cod liver oil dissipated the eruption almost entirely in six weeks.

But there is a far more extensive class of cases, those, namely, in which the patient, though constitutionally sound, is debilitated either by previous disease, insufficient diet, over-lactation, or the like, in which cod liver oil has in my hands proved of great service. And I would go further even than this: for I am of opinion—one which is not yet, however, sufficiently strengthened by examples to be stated as a fact—that many cases of old standing, in which other remedies have been tried unsuccessfully, and in which the patients to be neither debilitated nor strumous, are benefited by cod liver oil.

The finest pale oil is the preparation which I prefer, of which a teaspoonful may be given to an adult thrice daily at first, and the dose gradually increased ill four tablespoonfuls are taken per diem. It is most likely to agree if taken after food; but many who are accustomed to take it, and with whom it agrees well, should take it in the middle of the interval between each meal, when it may be looked upon in the light of a supplemental meal.

On no account should the patient be allowed to stop it for a slight reason; and often on inquiry it will be found, that when it is said to disagree with the stomach, it in reality agrees quite well, but is disagreeable to the taste. Even if the patient loses his appetite or is sickened with it, it should not be omitted entirely, but only for a few days, and then given at first in a smaller dose and immediately after food. If the fine pale oil disagree, one of the other varieties may be tried, or "Furley's cod liver oil cream," or cod liver oil chocolate, made by M. Lebaigue, 9, Langham Place, introduced by Erasmus Wilson. Each pound of the chocolate contains four ounces of oil. Sometimes the addition of a little syrup of the iodide of iron (say 3ss. to each dose) to the pale oil renders it more palatable, and is besides of value in the case of debilitated or strumous patients.

Very successful results may often be obtained, in cases where one is in doubt whether cod liver oil or arsenic is indicated, by combining the two.

Alkalies are not nearly so generally applicable to the treatment of psoriasis as arsenic or even cod liver oil. They are chiefly used when there is a gouty or rheumatic taint, or when the patient is much addicted to the use of stimulants, and when there is a tendency to acidity of the stomach, and to the deposit of lithates in the urine. The indications, indeed, for the use of alkalies in eczema and psoriasis are nearly the same.

The alkali most used and greatly praised by Dr. A. T. Thomson, the late Dr. Joseph Bell, who published a series of cases of psoriasis which were benefited by it, and others, is liquor potassæ, which is given in doses of 20 to 30 drops thrice daily in a large quantity of water, or, as the late Dr. Neligan recommended, in table beer or veal broth. Dr. Easton, of Glasgow, says he

has found much benefit from the use of the acetate of potash—which is, however, not an alkaline but a neutral salt—in doses of 3ss. thrice daily.

The carbonate of ammonia is the alkali from which I have myself derived most benefit. It must be made up in small quantities and kept in a well-stoppered bottle, as it loses strength rapidly when exposed to the air; and the apothecary must be warned to give an article of full strength, and one which has been carefully prepared. It should be given in doses of ten grains gradually increased up to 30 or 40, and largely diluted with water. Sometimes it will be found of service to combine it with a preparation of arsenic, or with colchicum if there be a gouty tendency. The latter may be added in doses of about 10 drops.

In persons addicted to the use of ardent spirits, the eruption is apt to appear in a very aggravated form, and I have seen it very rapidly improved by a course of carbonate of ammonia. In nervous persons, whose nervous system has been acted on by some depressing cause, a course of carbonate of ammonia often benefits the eruption, especially when combined with change of air and scene, and cheerful society.

But we shall suppose that the eruption is chronic at the commencement, or that, by means of one or other of the above methods of treatment, the acutely inflammatory symptoms have subsided and the eruption remains stationary, what are we to do now?

In the first place, we must remove the scales; and this is done by means of warm baths, or by the hydropathic treatment, and thorough rubbing with rough towels after each; or by taking two or three Turkish baths, with thorough shampooing. This having been done, and the morbid surfaces being fully exposed, we proceed to the application of local stimulants and alteratives.

If the eruption be extensive, there is no class of remedies so generally useful as preparations of tar. Of these some prefer the common tar (Pix liquida), others the oil of cade (Oleum cadini, manufactured at Aix-la-Chapelle), or Oleum rusci (obtained from the bark of the white birch, and a supply of which has recently been obtained from Germany for me), while the mixture which I have alluded to in my book on Eczema under the name of "Tinctura saponis viridis cum pice" is frequently of service. Whichever of these is used, it should be rubbed firmly into the eruption, and the application repeated night and morning, after the previous one has been washed off with soap and tepid water. During the treatment by means of tarry or unctuous applications to a large extent of surface, the patient should be made to wear a woollen dress next the skin; and he should be warned not to change it oftener than once a fortnight, as a dress impregnated with the application keeps the skin in an atmosphere of it, and is beneficial.

Sometimes the application of tar is not well borne, and I have known a single application greatly aggravate the inflammation of the skin, producing a severe dermatitis; and the unfortunate circumstance is, that one is often unable to tell beforehand in what case it is likely to occur. For this reason it is generally better to begin the tar treatment cautiously, and to use at first a diluted preparation in preference to the pure tar. The incorporation of tar with an ointment is a very good way of diluting it, rendering it more agreeable to the patient, and less likely to excite inflammation. It may be incorporated in varying proportions with zinc ointment, or with a mixture of powdered oxide of zinc and almond oil; * or the common tar ointment may be used with benefit, although I am surprised to find that it has been omitted from the British Pharmacopœia. Those who are familar with the use of tarry preparations in the treatment of eczema, and of the great benefit which is derived from them in chronic cases, will be much disappointed at first to observe that eruptions of psoriasis are very much less under their influence, and that they act very much more slowly upon the latter. I was particularly struck by this in a case of psoriasis of the elbows, combined with eczema of

* ℞. Olei cadini (Aix-la-Chapelle) ʒ vi.; ung. simplicis, pulv. oxidi zinci, olei amygdalarum, aa ʒ i.; cochinillini gr. iv.; olei rosæ m. xviii. M.

other parts. I ordered the "tinctura saponis viridis cum pice" to be rubbed upon both eruptions twice daily, and in a few days the eczema had all but disappeared, while very little impression had been made upon the psoriasis. My object in mentioning this is, not to undervalue preparations of tar in the treatment of the latter, but only to warn those who are making use of them that, as a rule, they require to be persevered with for a much longer time than in cases of eczema.

It is well to know that when tar is rubbed upon the skin it is to a certain extent absorbed, and circulates through the body; so that when a large extent of surface is under treatment, and particularly if the tar be rubbed very firmly into the skin, disagreeable symptoms may be induced, such as nausea, vomiting, diarrhœa, and feverish symptoms. The matters vomited, the stools, and the urine have a green or black apparance, owing to the presence of the colouring matter of the tar. These symptoms, however, rapidly subside if the treatment be omitted for a few days, and a diuretic given to cause increased excretion of tar through the kidneys.

Tarry preparations are not so convenient to apply to the head, owing to their gluing the hairs together; or to the face, owing to the discolouration and consequent disfigurement which they produce. I am therefore in the habit of applying to these parts one or other of the lotions or ointments which I am about to mention, and of only using preparations of tar when the others fail or when the inconveniences just alluded to are of no consequence.

One of the best applications—one which I have used with great advantage, and which is much employed and justly praised by Hebra—is soft soap (potash soap, black soap, sapo mollis), or what is better, a solution of it in water* or spirit.† These require to be rubbed into the roots of the hair and on the spots on the face very firmly every night, or, if convenient, every night and morning, and washed off as seldom as possible. I have noticed, an observation which was first made by Hebra, that the eruption of psoriasis on the head and face is much more readily removed by local measures than when it is situated on other parts, and that the above treatment invariably removes the eruption from the head in a comparatively short time.

Hebra recommends the following modification of Pfeuffer's treatment in cases of extensive eruption; but I have not used it often myself, owing to the pain which it produces, and the necessity of confining the patient to bed. This consists in rubbing soft soap into the whole of the eruption twice daily for six or eight days, each patch being once rubbed, during that period, so hard as to remove the scales entirely and cause slight bleeding. The patient lies in bed all this time enveloped in blankets, and for three or four days after it. He then takes a warm bath and dresses himself. Hebra remarks with regard to this treatment that it is only successful in the minority of cases, that it often requires to be repeated, and that it may be combined with one of the other methods of local treatment with advantage.

Ointments containing mercury are often of great value; but they are more applicable to the treatment of limited eruptions, as it would be unsafe to apply them for any length of time to an extensive surface, owing to the danger of their being to any great extent absorbed, thereby producing salivation —an occurrence which is not only to be avoided for its own sake, but which has the additional disadvantage of tending to produce a more copious eruption, owing to the depression of the general health which is thereby induced. There is no reason, however, why, in extensive eruptions, one of these ointments should not be applied to certain parts, such as the head, face, or hands, while other methods of local treatment are employed for the other parts which are affected. Those which are most useful are, citrine ointment (Unguentum hydrargyri nitratis), white precipitate ointment (Unguentum hydrargyri ammoniati), ointment of the red oxide of mercury (Unguentum hydrargyri oxidi rubri), and ointment of the red iodide of mercury (Unguentum hydrargyri iodidi rubri). These must be rubbed into the eruption, after the scales

* ℞. Saponis mollis ʒi.; aquæ bullientis ʒii.; olei citronellæ ʒss. Solve et cola per chartam.
† ℞. Saponis mollis ʒii.; spiritus rectificati ʒi.; sp. la andulæ ʒi. Solve et cola per chartam.

have been removed, night and morning. It is of great importance to use a very small quantity of ointment, to melt it completely before using it, and to rub it in very firmly. Great care must be taken to obtain an ointment which has been recently and properly made, and to get it in small quantities, so that it has no chance of becoming rancid by too long keeping. And one must be constantly on the watch for symptoms of salivation, especially when it is remembered that the amount which, when absorbed, is capable of producing them, varies much in different persons.

For limited eruptions, Hebra recommends the use of Vleminckz' solution of sulphuret of calcium, a preparation which I have recommended in my work on the "Parasitic Affections of the Skin" for the cure of scabies. It should be rubbed into each patch firmly with a piece of flannel till slight bleeding is induced, when a little more of the lotion is laid on, and it is allowed to dry. The patient then sits in a warm bath for about an hour. The parts are then washed and dried, and a little oil is applied. One good rubbing generally destroys a patch of psoriasis, but the treatment is very painful.

When the eruption is very limited it may be removed by blistering. For this purpose collodium vesicans (Neligan) or glacial acetum cantharidis—that is acetum cantharidis prepared with glacial acetic acid, the ordinary solution of the Pharmacopœia being too weak—may be used. It should be made in small quantities at a time, and kept in a good stoppered bottle, the stopper being removed for as short a time as possible; and, when not in use, covered with leather, otherwise its strength soon diminishes, and much annoyance is thereby occasioned. A little of this solution should be taken up by means of a paint-brush, and painted firmly over the part till it becomes perfectly white. If the fluid is full of strength, and the skin thin, as on the face, it usually blisters it at once; but if the opposite holds, and especially if the head or palms of the hands are to be blistered, it may require to be painted over them for several minutes. When the skin is *thoroughly whitened*, enough has been applied; but it must be remembered that the skin rarely "rises" after it, as after the application of a common blister. One application is often sufficient to remove the patch.

The eruption on the hands or other parts may often be benefited by what may be termed "localised cold packing." This is done by wringing a handkerchief out of cold water, and rolling it firmly round the parts, and then covering it completely with oilskin. This is done every night, and the application is allowed to remain on till morning. In a short time the eruption is modified, and in some cases it disappears altogether; but if not, it is then of advantage to make use of one or other of the ointments above referred to.

If the eruption does not yield to medicine at home, a course of mineral baths is to be recommended, especially if the disease is of old standing, and the patient in need of change of air, and relaxation from business. Those of which I have had most experience are Leuk, Aix-la-Chapelle, and Kreuznach. The last two are the most agreeable residences for visitors, while the baths of Leuk are probably the most effectual. It is generally of advantage to go for two or three summers in succession to one or other of these places, remaining three or four weeks each time; and frequently the benefit derived is not experienced till some time after the course has been taken. It is a common opinion that the advantage derived is due, not so much to the ingredients in the baths, as to the maceration which the skin undergoes, owing to the prolonged immersion in hot water, an opinion which is probably correct.—*Glasgow Medical Journal, July*, 1865, *p.* 144.

60.—ON THE TREATMENT OF ACNE.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Skin Diseases.

My own experience is, that in acne there is only one medicine really necessary, and that is arsenic; all the others only clear the way for it. When

there is any great feeling of heat and irritation, white tongue, irritability of the stomach, tendency to vomiting and constipation, it is generally necessary to give a mild saline aperient for a few days before beginning with the arsenic. Similarly, when arsenic brings on headache, sickness, coating of the tongue, and nausea, it is as well, if these symptoms do not soon begin to yield again, to leave off the medicine and give the saline. Again, if there be loss of appetite, a mineral acid may be prescribed in bitter infusion; and perhaps few preparations of this kind are superior to the nitric or nitro-muriatic acid in infusion of quassia or calumba. Suppose, however, that loss of appetite is conjoined with acidity of the stomach, foulness of the tongue, and constipation, then liquor potassæ may be substituted for the nitric acid, and mild aperients may be added; if there be no particular loss of appetite, the liquor potassæ may be given in milk, particularly if there be much thickening of the skin. I believe my observations quite warrant me in saying that, even when given alone, it will, after a time, produce considerable absorption of the matter around the follicles, making the skin much thinner and softer, sometimes also paler. In this respect it paves the way exceedingly well for arsenic; but I have never been able to cure one severe case of acne with it, however carefully it might be used, and with whatever amount of perseverance.

If the bowels become constipated at any time while these medicines are being taken, the necessity for attending to this state should, I think, take precedence over all other indications for treatment, especially if the patient have any tendency to piles, as is sometimes the case in bad acne rosacea. The simpler, however, the aperients which are used the better: they need not in any way interfere with the employment of other remedies, and may be continued even when the arsenic has been begun with; though generally they are not requisite then, because the arsenic itself rather tends to bring on looseness. When aperients are not necessary, small doses of mercury may be given, especially if there be any objection to the use of arsenic, to the employment of which some practitioners are very averse; but I do not think mercury should be given either to overcome constipation or along with aperients, as it is apt to distress patients. Perhaps no salt of mercury fulfils the requisite indications here better than the biniodide, in doses of a sixth of a grain or so daily, in combination with some mild sedative or aromatic; but I think that its administration had better not be continued after the arsenic has been begun with. Should there be much exhaustion, steel may be given; and it has more than once happened to me to protract the cure rather than hasten it, by hurrying on to the use of arsenic before putting the patient through a course of steel, the necessity for giving it being shown by the immediate benefit which ensued as soon as it was had recourse to. When there is no dyspepsia present, the tincture of the sesquichloride of the British Pharmacopœia, or some similar preparation, may be exhibited; and when these disagree with the patient, the steel mixture of the Pharmacopœia sulphate of iron, either with or without sulphate of quinine, may be substituted.

So soon as these remedies have even partially fulfilled the indications expected from them, not a day need be lost in giving the arsenic; it is not in any way necessary to get over every little disturbance of the system before using it, partly because the beneficial effect of the tonics, &c., to some extent continues after they have been given up, and partly because arsenic is a powerful tonic, keeps the bowels open, and removes many obstinate nervous symptoms. When it is beginning to produce a decided effect upon the disease, it may be advisable, and indeed it is sometimes necessary, to reduce the dose; but this should only be done when absolutely requisite, as it cannot be raised again so easily. Should a relapse occur, I think it is decidedly preferable, instead of increasing the dose, to give the saline, and acid, or steel, for a short time, after which the arsenic can be given in the same quantity as at first.

As to external applications, I confine myself to saying that I have tried several, and that, with a few rare exceptions, I think they may be dispensed

with, and that they exert no material influence over the duration of severe acne. Any mild mercurial preparation, such as calomel ointment, seems to agree very well with punctiform, or simple inflamed acne, and the ointment of the hypochloride of sulphur is a valuable preparation in the more severe forms, and in acne rosacea. I have removed a considerable degree of rosacea by simply employing this remedy freely every night. Blistering with nitric acid or cantharides has been recommended on excellent authority, but I have never found it requisite to employ so sharp a remedy. Lotions and washes of all kinds are, I think, superfluous. They may be admitted as luxuries, but such things divert attention from the more important subject of medical treatment; and when the precaution is taken of always washing with very hot water, without soap, of using ointment freely every night, and defending the face from the action of cold dry winds, they are very rarely required.

I will now give a few cases recently treated in this way. If they establish the conviction in the reader's mind that they are adequate to the purpose, the object aimed at in this paper will be gained; for my wish is simply to call attention to the benefits of a plan which may enable us to get rid of unnecessary elements, seeing that the nature and uses of one drug can be much more easily learned and retained in the memory than those of several, and offer a more easy standard of comparison with any new remedy we may be tempted to try.

Case 1.—*Acne Rosacea of long standing.*—A. G., a strong, otherwise healthy-looking man, applied as out-patient at St. John's Hospital, October 13, 1863, with this affection, under which he had been suffering several years. The skin over almost every portion of the face was raised up in large papules of intense redness; there was no lividity in them, the colour being a deep rose, or damask. They also extended over a great part of the front of the chest, particularly along the line of the clavicles. The skin in the few parts of the face not assailed by the acne was, I think, without exception, the coarsest I ever saw; it was almost like a grater, with the colour of a nutmeg or old lemon. The patient suffered from acidity of the stomach and heartburn, accompanied, and as he thought, aggravated, by constipation. He was directed to take twenty minims of liquor potassæ three times a day, and an occasional aperient, to wash only with very hot water, avoiding the use of soap, and not to take any hashes, stews, pork, &c., the use of which was generally followed by an exacerbation of the heartburn and acid eructations.

His employment only allowed him to come very seldom, and it was not till March that I could secure his regular attendance. The arsenical solution was now added, in doses of five minims three times a day; but it almost immediately produced such disorder of the stomach, that it had to be given up, and steel was prescribed instead, a saline being first of all given, in order to allay the irritability of the stomach, and remove the coated state of the tongue induced by the arsenic. Some slight improvement took place under this treatment; he remained free from indigestion and costiveness; but as he did not attend very regularly, he made no great progress till July, when, having arranged to come once a week, the arsenic was resumed in the same doses as when first used, the ointment of the hypochloride of sulphur being added. He now attended very punctually, and, so far as I could gather from his own statement, suffered no inconvenience from taking the arsenic, beyond now and then a slight feeling of sickness, which, however, was not severe enough to induce him to leave off the medicine; and the beneficial action of this was now so steady and rapid, that by the end of September the disease had almost entirely disappeared, and on the 24th of November, when he was last seen, it was gone, the cure being to the best of my judgment complete.

Case 2.—*Acne Rosacea of nearly Nine Years' Duration.*—G. R., a strong-made man, aged 35, a waiter by trade, was admitted December 5th, 1865, as out-patient at St. John's Hospital, with this complaint. His account was that, nearly nine years ago, he noticed it after long and wearying debility and lassitude. It first appeared on the cheeks, and gradually spread to the forehead. The face is now almost entirely covered with hard, fiery, red papules, several of which are in a state of suppuration, and the intervening

skin, except in one or two small spots, is of the same fiery hue. The face is a good deal pitted, from the suppurating of the follicles. He denies having been irregular in his habits, though he may have, he says, on a few rare occasions, given way to intemperance. He has been under treatment, and has also attempted to cure himself. I need scarcely add that, while he was his own physician, he took a quantity of cooling medicine and sarsaparilla, both with the same amount of benefit.

He was ordered a dose of the saline mixture of the hospital every morning, and to wash only with very hot water. Having continued this treatment for a week, the dilute nitric acid was prescribed in infusion of quassia, three drachms of the acid to a pint of the infusion; an ounce three times a day. He was directed to take plain good food, and a little pale ale or dry sherry as a stimulant. As generally happens under this treatment, the tongue rapidly became clean and the appetite improved. The ground being thus cleared, he was, on the 19th, ordered the liquor arsenicalis, without spirit of lavender, five minims three times a day with his food; he was also directed to apply the ointment of the hypochloride of sulphur to the face freely on going to bed, and to allow it to remain on all night.

By the 13th of February the disease, though somewhat retarded by a slight outbreak of indulgence shortly after Christmas, which required a return, for a short time, to the nitric acid and quassia, was quelled; the suppuration had long ceased, the tubercules had flattened down almost to a level with the skin, and the fiery hue had given way to a slight amount of deepish redness. He had only taken the arsenical solution twice a day during a greater part of the time. He continued to progress in a very satisfactory manner till the beginning of March, when he was suddenly assailed by a severe looseness of the bowels; believing this to be the effect of the medicine (the arsenic), he left it off, and as he seemed afraid to resume it I directed him for a few days to use only the ointment. About the middle of the month there seemed to be a strong tendency to relapse, owing, perhaps, in great part to the extremely bleak east winds which prevailed at that time; but this outbreak subsided again under the continued use of the ointment, and when seen on the 27th he was, for all practical purposes, cured, the spots being quite shrunk, and differing only a very little in colour from the surrounding skin.

Case 3.—Acne Rosacea of Twenty Years' Standing.—Annie S., an unmarried woman, came under my care, as a private patient, on January 3rd, 1863, with this affection, which she attributed to anxiety; it was very severe, the skin thick and deep red. She was ordered the dilute nitric acid in infusion of quassia, the eighth of a grain of biniodide of mercury daily, and a lotion of the carbonate of soda. It was, however, very soon evident that this did not agree with her skin, and it was left off. Her appetite having improved under the use of the nitric acid, the liquor potassæ was begun with on the 7th of March, and by the 28th there was also a very marked improvement in the acne; the liquor arsenicalis was, therefore, ordered, but I was obliged almost immediately to relinquish it, even in the smallest doses. She was now attacked with severe rheumatism, for which colchicum was given with iodide of potassium; the treatment of the acne being for a time suspended, a relapse took place. I attempted to keep it in check by the use of sulphuret of potassium lotion and other local means, recommended by different authors, but they never agreed with her skin, making it decidedly more red and tender. The rheumatism being subdued, Donovan's solution was tried, but her stomach would not bear it. Tincture of steel was then ordered, but it almost immediately brought on dyspepsia, and, as a last resource, she was directed to take the steel mixture of the Pharmacopœia and an occasional aperient pill, using no application to the face beyond very hot water and glycerine and calomel ointment at night. This agreed thoroughly well with her, and she took nothing else during the remainder of her attendance, which, with a few exceptions, lasted up to the middle of October, 1864, when she left, cured, so far as such cases are cured at her age and after lasting so long.

I have greatly condensed this account, as I took very copious notes of it originally, and therefore the inferences I draw may not appear so clear

to others as to myself; but it appeared to me that all the benefit obtained was really due to the nitric acid, the liquor potassæ, the steel mixture, and the calomel ointment.

Case 4.—*Simple Inflamed Acne.*—Being anxious not to occupy too much space, the following case is given only in a very condensed form; nor would I have brought it forward but that it was such a severe form of the disease, and filled up the gap between rosacea and punctiform acne:—

The patient, E. S., admitted March 18, 1864, as out-patient at St. John's Hospital, was a young man whose face was entirely covered with inflamed acne; the health was thoroughly out of order, the tongue being extremely foul, and the patient himself very weak; the muscular system greatly relaxed. He entirely recovered in about three months, under the use of the acid and deutoioduret of mercury, followed only by three minim doses of liquor arsenicalis three times a day.

Case 5.—*Acne Punctata and Comedones.*—Sarah S., a young unmarried woman, applied Dec. 9, 1864, with this disease. The face was almost entirely covered with spots of one kind or other, and on the cheeks the black dots were very numerous. She reported herself in perfect health, except that her appetite was not very good; her tongue was clean, bowels regular, menstruation good. She was not aware of any cause for the eruption, but had always washed with cold water, and fancied that of late it was worse, and that the skin smarted more after washing. The saline was ordered, and immediately followed up by the use of the dilute nitric acid in infusion of quassia; she was directed to wash only with hot-water, and to apply the ointment of hypochloride of sulphur on going to bed. As she fancied that beer always made her heavy and sleepy, she was directed not to take any, but otherwise to live well.

By the 7th of January she reported her appetite as ravenous, and some improvement had taken place in the acne. She was now ordered the solution of arsenic, five minims three times a day, and to leave off the acid, but continue the ointment. The history of the case from this time is that of a moderate, rapid, and uniform decline of the affection. On the 11th of February she was seen, and then presented a remarkable improvement; she was very much pleased with the progress she had made. The black dots had very much diminished in number and there were not above one or two acne spots left, but the skin was almost covered with brown stains, apparently from the action of the hypochloride of sulphur, and was still somewhat rough to the touch. The medicine had made her at times feel low and languid, but she always managed to take it three times a day, and never took anything else all the time she was under my care. When last seen (March 18) she considered herself cured; there had been no fresh spots, the old ones were fading away, and the dots were lessening in number. I wished her to remain under treatment, but she discontinued her attendance.—*Medical Circular, May* 3, 1865, *p.* 279.

61.—RECENT RESEARCHES ON SCABIES.

There is no better way of tracing the development of scabies and no more conclusive proof of the real nature of the affection, than to place an impregnated female acarus upon the skin, and then to leave it to its own devices. The fact of impregnation, which is of course all-important, is readily determined in the way pointed out by Gudden. We have only to extract an acarus, without injuring it, from a recent cuniculus, and then to cut off this portion of the skin containing the cuniculus, and to examine it under the microscope. If it should present ova, we may be quite sure that the animal will afterwards deposit others. Hebra says, however, that a female acarus, even though it may lay eggs, does not always with certainty generate scabies; and he recommends, for the artificial production of the disease, the application to the skin of a whole cuniculus removed from another person suffering from that affection.

For the experimental investigation of the habits of the acarus, however, it is best to take a single female, and place it upon the skin. It moves briskly over the surface and soon begins to attack the epidermis. It may either proceed at once to bury itself, or it may leave off after a time, and commence again at another spot. In penetrating the superficial part of the epidermis, the acarus seems to experience some difficulty. It places itself in a position almost vertical with respect to the skin, supporting itself by means of the long bristles which project from the hinder part of its body. Having once pierced the surface of the cuticle, it makes more rapid progress, and in from ten to thirty minutes it is completely buried. It naturally attacks in preference those parts of the skin which have the thinnest epidermis, and for this reason it appears often to select the entrances of the hair-sacs.

Having thus ensconced itself within the epidermis, it continues to traverse the rete mucosum in a horizontal direction, and proceeds to lay its eggs. The number of ova which are deposited by one acarus is variously estimated. Hebra has never seen more than twenty-four to twenty-six ova in one cuniculus. Gudden, on the other hand, counted fifty-one in one passage, at the bottom of which was the parent, which still contained ova; and he assumes fifty to be the ordinary number. The rapidity with which they are laid varies with the age of the acarus. An experiment of Gudden's, made in the winter of 1859-1860, gives very precise information upon this point. He placed an acarus, which was ascertained to have laid previously only three ova, and which contained another nearly ready to be deposited, upon the skin of a healthy person, who was kept all day in a moderately warm room. At the end of five days, the cuniculus was cut out, and was found to contain eleven eggs. In other observations in which the acari were more nearly exhausted, the eggs were deposited much more slowly, one in two or three days. According to Bourguignon and Gudden, apertures are always found, at more or less regular distances, in the roof of the passages formed by the acari, and it is supposed that they may serve for air-holes. Their existence is positively denied by Hebra; but however this may be, it appears certain that they are not used by the female acarus as means of egress from the cuniculus. The animal, in fact, never emerges from the passage which it has formed for the reception of its ova. It continues to eat its way through the skin, and when its power of laying eggs is exhausted it dies. The whole duration of the life of an acarus is estimated by Gudden at three or four months. Impregnated females, placed on a healthy person's skin, were found by him alive, but exhausted, in the seventh or eighth week afterwards.

Eichstedt stated that the ova are not deposited in the egg-passage in a continuous series, but that there are always intervals after two or three, or at most six, ova. Gudden admits that this is the case in the cuniculi which are seated on the hands, where the acarus is exposed to frequent disturbance; but he says that on the trunk as many as twenty-one eggs are often found in an unbroken row.

Embryos are rapidly formed in these ova. In examining under the microscope any cuniculus in which several eggs have been deposited, these are usually found to present different stages of development. There are, however, rarely more than from eight to fourteen which contain embryos; the others are merely egg-shells, from which the young acari have already escaped. Assuming that the parent animal lays one or two eggs daily, this gives some idea as to the rate at which they are hatched. There is, however, more direct evidence on this point, though it is not very satisfactory. If the ova are placed in watch-glasses and kept at a proper temperature, which is effected by fixing them in the axilla, they may be hatched artificially. Gudden says that four days was the shortest time within which the young acari have left the shell in his experiments. The statements of earlier observers are rendered valueless by the doubt whether the ova had been recently deposited when the process of incubation was commenced. It is, however, tolerably certain that the time is not always the same; for Gudden

says that, after placing upon the skin acari which no doubt at once proceeded to deposit eggs, no empty shell was found within the cuniculus, in one case on the fifth, in another on the sixth day. It would be of great importance to ascertain how long the ova retain their vitality, when they are placed under unfavourable circumstances; but as to this point there is at present no evidence.

Having left the shell, the young acari almost immediately make their exit from the cuniculus also; and, according to Gudden, they avail themselves of the air-holes which he describes as existing in its roof. They then move over the surface of the skin with considerable rapidity, and, after a time imbed themselves again within the cuticle. The passages which they form are usually very short; Gudden says that he has seen some a line in length, but that these are rare exceptions. He states, however, that the young penetrate into the skin more deeply than the older acari, and that they therefore produce more irritation. Their bite is often followed by the appearance of a small papule or vesicle, in which exudation shows itself on the second day. Before this time the animal has usually moved off to another part of the skin. Hence the young acari are very difficult to find, but, according to Gudden, a diagnosis may sometimes be made, in slight cases of scabies, in which none of the larger cuniculi are to be detected, by cutting off the heads of some of the vesicles or papules, and examining them, without any varnish to render them transparent. The entrance of the passage formed by the young acarus may then be made out. When the light comes from below, the dried edge of the opening looks dark.

It is well known that the acarus is exceedingly sensitive to changes of temperature. Itch-insects, when first extracted from the cuniculus, often remain motionless; but if the glass slide on which they are laid is warmed, or if the warm breath is allowed to pass over them, they at once begin to move. Gudden states that, when they are placed upon the skin, the rapidity of their movements is always observed to depend upon the temperature of the surface at the time. Admitting the influence of warmth on the activity of the acari, it seems natural to ascribe to the movements of these animals the irritation which those who have scabies experience when the body first becomes warm in bed. Everybody knows that the itching is then worse than at other times; and Gudden says that a similar increase of irritation is felt whenever the skin is warmed, whether by the sun's heat, by exposure to a fire, or even by exercise; and, according to the same observer, cooling the surface quickly relieves the itching.

Some of the other peculiarities of scabies appear to receive their explanation on the same principle. If the acarus moves only on those parts of the skin which are warm, and therefore chiefly at night, it would seem to follow that parts which are uncovered during sleep will remain free from their presence. In this way Gudden accounts for the well-known fact that the face rarely presents any traces of the eruption in cases of scabies, and he states that this immunity is enjoyed by the face only when it is exposed at night; and, as we have already seen, several observers have found acari on the face or forehead of infants, who lie with those parts deeply imbedded in pillows. An experiment made by Gudden points to the same conclusion. He placed a female acarus upon the left hand of a man who always slept on the left side, and with the clothes carefully pulled up round his chin. The young brood were traced up the arm on to the trunk; and one was caught on the left cheek, which lay in the warm. The other side of the face remained free from their presence. The same writer says that cuniculi are rarely found on the feet of people whose feet are constantly cold. He also relates a very interesting case in which acari occurred in great numbers on the trunk, while none could be detected on the hands. This patient was confined in a strait-jacket, and her hands and feet were always cold, and did not become thoroughly warm even when she was in bed, as she was so unquiet that the bed-clothes could not be kept over her. The inner side of the arm, which was fixed to the side, presented a few cuniculi. This case is the more con-

clusive, as Gudden states that it was left undisturbed for a considerable time.

We are inclined to explain in a similar way a fact which seems to have been observed first by Hebra, and to which we have more than once heard him draw the attention of his class—that the disease is always present in large quantity on parts of the skin which are exposed to pressure. Thus in cobblers, tailors, weavers, and other artisans who sit on hard benches or stools, the buttocks, and especially those parts of them which come into contact with the seat, are always covered with the eruption. This consists in part of papules, in part of pustules and crusts; and some of these elevations of the skin present cuniculi on their long axis; others contain no parasite. On the other hand, in joiners, carpenters, and bricklayers, this part of the body is unaffected by the disease. Again, in women the eruption always shows itself on those parts of the skin which are pressed on by straps or girdles; and the pressure of garters, trusses, or of any tight article of apparel, or even of a crutch, is said by Hebra to be sufficient to produce the same effect. He accounts for these facts by ascribing the papules and pustules to direct irritation of the cutaneous surface; but it seems to us more reasonable to suppose that the pressure keeps up the warmth of the skin at that part, and that this favours the development of the acari, and at the same time prevents their wandering to other parts of the epidermis.

Hebra, however, does not admit that the distribution of the itch-insect over the skin is influenced to any great extent by temperature; but his reasons for this opinion do not appear to us to have any great weight. He says that if it were so, the eruption of scabies ought to increase rather than diminish during the progress of acute diseases when the patient is kept constantly in bed; but there is every reason to believe that the morbid condition of the system has in this case a direct influence in checking the development of the parasite. Another of his arguments is, that if the acarus loved warmth, it ought to select the axilla, the bend of the elbow, and the softer parts of the skin, rather than the hands and feet, the penis, the buttock, and the extensor surfaces of the joints, on which parts cuniculi are found most abundantly. But we have already seen that at the moment when the acari are moving over the surface of the skin, these parts are for the most part warm, and that the distribution of the acari is rather favourable to the view to which Hebra is opposed. Again, he gives as a reason for thinking that the acari are not much affected by external warmth, the fact that they reside in the deeper layers of the cuticle, which must be kept of a nearly constant temperature by the blood circulating through the derma; but this appears to us to remove a difficulty in the way of Gudden's view, by accounting for the fact that the ova can be developed in the cuniculi which are seated on the hand, and which must otherwise be exposed to considerable variations of temperature.

The truth is that, in Hebra's opinion, the acarus forms a very unimportant element in the causation of scabies, except so far as contagion is concerned. He criticises the views of Gerlach and Aubé and others, who looked on the acarus as a kind of nocturnal beast of prey, which hunts for spoil at night on the surface of the skin, and retires during the day into the dark gallery which serves as its retreat. He says that it is not likely that the acari leave their stations of their own accord and without necessity, and take long journeys, in order to seek at another spot and with considerable trouble what they can get abundantly by staying where they are. Even in the propagation of the disease, Hebra regards them as altogether passive. He thinks that the cuniculi are torn open by the patient's finger-nails, and that in this way the ova and young acari are carried not only to the skin of other persons, but even to different parts of the same patient's body. Now, it is universally admitted that the female acarus never emerges from the cuniculus in which she lays her eggs; but it appears to us to be quite established, by the experiments of Gudden and others, that the young acari have the power of distributing themselves widely over the skin by their own movements; in fact,

according to Gudden, they probably seldom remain more than three days in the same place.

Hebra makes the remark (and other dermatologists have said the same thing) that, viewed as a disease of the skin, scabies can scarcely claim an independent place in the scheme of classification, but that it should be regarded rather as an artificial eczema, caused by the presence of the acari and by the *scratching of the patient*. Now it is obvious that, if the acarus be the sluggish animal which Hebra supposes it to be, its presence in the skin is not likely to give rise to very much inflammation; and accordingly we find that of the two factors to which he ascribes the development of the eruption, the scratching on the part of the patient is the one to which he attaches most importance. It has long been known that the severity of the eruption is by no means proportionate to the number of acari which can be discovered, and that in the pustular scabies of children scarcely any cuniculi are sometimes to be found. Hebra also states that in no case does the seat of the acari and that of the eruption correspond; for while the former are found chiefly on the hands and feet, the penis, &c., the eruption is always most abundant on the anterior surface of the body, between the mammæ above, and the knees below. This he accounts for by supposing that the acari only produce a vague itching, not referred to any particular point, and that therefore the patient scratches himself at that part of the body which is most accessible to himself; that is to say, on the chest, abdomen, and thighs. To us this explanation does not appear very satisfactory. With the exception of those papules or pustules on which cuniculi are seated, Hebra does not admit any part of the eruption of scabies to be caused directly by the irritation of the acari, but regards it all as the effect of scratching, with the exception of those appearances which, as we have seen, he ascribes to pressure.

There is, however, good evidence to show that papules and pustules may be produced in scabies, independently of scratching. Gudden says that his patient, who was confined in a strait-jacket, presented tolerably large pustules not only on other parts, but also on the shoulders, which were but little exposed to external irritation. Again, he placed a female acarus on the skin of a healthy man: she formed a cuniculus and deposited ova. Even during the first few days a kind of itching was felt over the whole body, which he rightly ascribes to excited attention; but on the ninth and tenth days a new sensation was felt, which was referred to the hand, and resembled a pricking or gnawing; and before long papules and vesicles made their appearance, and gradually increased in numbers. *Scratching was altogether refrained from*, and the eruption was caused solely by the bites of young acari. The point is not, perhaps, of much importance, for every one must admit that scratching does to a great extent aggravate the eruption in most cases of scabies; but it is, as we think, quite clear that the great German authority on cutaneous diseases is not altogether justified by the facts in his views upon this subject.

There can be no question that, since the appearance of the cuniculi has been thoroughly understood, the diagnosis of scabies has been immensely facilitated; it has in fact, in a large number of cases, become a matter of demonstration. But we doubt whether these passages are generally recognised by medical men in this country. Only three or four years since an eminent physician recommended searching for ova, rather than for the parent acarus, in doubtful cases. We have several times tried to find them, as he advised, by scraping the skin or examining the crusts; and, as might be expected, we have always been unsuccessful. But when one has once learned thoroughly the appearance of the cuniculi, the finding of the ova is often an affair of a few seconds only. We would strongly recommend to those who are learning medicine, the practice of slicing off with a sharp knife or pair of scissors any doubtful cuniculi, and examining them, *en masse*, under a low power of the microscope. In order to remove the parent acarus, it is necessary to cut rather deeply; but the detection of the ova or of the fæces may usually be effected without causing the slightest pain. The recognition of these cuniculi is of the more importance, because it appears

that in some cases they are the only manifestations of scabies. Baum and Eichstedt have especially drawn attention to this form of the disease, in which there is no trace of papules or of pustules; and Lanquetin describes a similar case, that of a young hysterical girl, who did not suffer from itching, and whose skin was quite healthy. This case might no doubt be cited in favour of Hebra's opinion, that the eruption is caused by scratching; but it is more probable that some morbid state of the cutaneous sensibility caused the absence of both the itching and the eruption.

We have already quoted the directions of Gudden for discovering the young acari or the short cuniculi in which they dwell. Gudden thinks that in almost every case of scabies, even though none of the longer passages can be found, it will be possible to detect those of the young animals; but it does not appear that he has under observation more than a small number of patients. In his earlier papers Hebra asserted that without the discovery of the cuniculi we are not justified in concluding that scabies is present. But the experience of 40,000 cases had caused him to change his opinion, and he now says that it is possible to make a diagnosis of the disease with great probability, if not certainty, in cases in which no cuniculi, nor even any of the papules caused by young acari, can be recognised. There can be no doubt that such cases do sometimes occur. Moreover, it must not be forgotten that the presence of cuniculi on the hands may be interfered with by many circumstances. In those who wash their hands very frequently, or whose use pumice-stone to cleanse them, these passages are quickly destroyed; and, according to Hebra, they are also absent on the hands of washerwomen, hatmakers, and bricklayers, because these people employ in their business substances which are prejudicial to the acarus. In Paris an exaggerated importance is attached to the presence of a cuniculus on the skin of the penis, or on the glans, in the diagnosis of this disease; and according to Piogey, scabies is sometimes limited to that part only. We have heard Hardy insist on the fact, which we think he has also published, that eczema, confined to the female breast, is almost always caused by the presence of acari.

In No. LXVI. of the British and Foreign Medico-Chirurgical Review (April, 1864, p. 392), an account was given of the 'rapid cure" introduced by Hardy at St. Louis. This method appears to be attended with very considerable success, and it undoubtedly presents the great advantage that it is carried out under the eye of the physician. Lanquetin states that it is doubtful whether all the cases are cured, as they are not kept under observation within the hospital. Great irritation of the skin is sometimes caused by this treatment, and therefore it is not adapted for private practice. Moreover, as Hebra remarks, the saving of time is more apparent than real, for the disappearence of the eruption must always take some days; and, till that has occurred, the lay observer would not believe that a cure had really been effected. Altogether, Hardy's plan of treatment seems best adapted for soldiers and the poorer class of labourers. We have often wondered why one of the large metropolitan hospitals does not establish a "skin department" on a large scale, in which these affections could be treated locally with baths, &c., on the premises, as is done in St. Louis and at Vienna.

In cases in which the eruption is generally diffused and severe, while cuniculi can be found only on certain parts of the skin, it might perhaps be advantageous to recommend inunction only at those points. Hebra states that he did this at one time with very fair success, but he then believed that the acari were rarely to be found except on the hands and feet; and when he gave up this opinion, he seems to have modified also his practice, so that his statement upon this point ought perhaps to be received with some caution, and as indicative of the sanguine nature of the man.

It seems to be generally thought in this country, that in order to prevent relapses it is necessary to disinfect the patient's clothing, in addition to inunction of the skin. Dr. Frazer of Dublin states that he has seen pustular scabies return more than once in a young child, till a pair of boots were destroyed to which the virus (according to his view) was persistently adher-

ing. But though it cannot be denied that portions of loose epidermis, or crusts containing ova, may remain in the clothes, and so that a second contagion may arise, it does not appear that this is frequently the case. Gudden says that persons often sleep in their dirty beds, after treatment, without any relapse occurring. In the General Hospital at Vienna, where 1500 patients come under treatment every year, no attempt is made to disinfect the clothes, although there is every appliance requisite for doing so. The relapses are said never to exceed one per cent. It appears to be certain that living acari never wander of their own accord into articles of clothing, as pediculi often do.—*British and Foreign Medico-Chirurgical Review, July*, 1865, *p.* 24.

62.—LARGE AND PAINFUL CARBUNCLE TREATED SUCCESSFULLY BY SUBCUTANEOUS SECTION.

By Christopher Heath, Esq., at the Westminster Hospital.

In June, 1862, Mr. French, surgeon to the St. James's Infirmary, brought before the Medical and Chirurgical Society a plan of treating carbuncle which he had pursued with great success for many years. It consists in the subcutaneous section of the indurated tissue by a tenotomy knife, which is introduced a little beyond the edge of the induration; and, the forefinger of the left hand serving as a guide upon the surface, the tumour is divided just as a tendon would be cut through. The skin itself is left uninjured. If the carbuncle is large, this process is repeated in different positions, so that the mass is crucially divided subcutaneously. We have seen a large number of cases treated by this method. The immediate relief obtained and the rapid recovery which follows are very striking. Every surgeon who has practised the old operation of crucial incision knows that it is necessary to cut deeply enough to divide the hardened base of the carbuncle. Mere division of the skin is useless; more than this, it is mischievous, for a widely gaping wound is formed, which is often most troublesome to heal, and leaves eventually a very ugly cicatrix. This is entirely avoided by Mr. French's process.

The following is a case of carbuncle in which Mr. Heath pursued this plan with the highly satisfactory result recorded:—

Henry W., aged 46 years, was admitted July 3rd, 1865, with a large carbuncle on the back, measuring six inches in diameter each way. This had been coming for ten days, and two days before admission the skin in the centre had given way, exposing a slough, but the surrounding tissue was greatly swollen and inflamed, and he was suffering great agony.

July 4th. The patient having been placed under chloroform, Mr. Heath made a crucial subcutaneous section of the carbuncle with a narrow bistoury. The knife was entered at the margin of the inflamed skin, carried well beneath the carbuncle, and the point brought up to the central opening, when the whole thickness of the subcutaneous tissues was divided crucially, the knife being entered four times on account of the large diameter of the carbuncle. The hemorrhage was very free, and was checked by a pad and broad bandage. Low diet; beef-tea, two pints; wine, ten ounces. Aromatic spirit of ammonia, half a drachm; liquor of cinchona, fifteen minims; water, one ounce: three times a day.

5th. Passed a good night, the pain being greatly relieved by the incisions. Pad removed. The inflammation has considerably subsided, and a small quantity of pus exudes from each puncture. Poultice.

12th. Patient's recovery has been uninterrupted. The inflammation has entirely subsided; the hole in the centre has not increased, and a slough of cellular tissue is now separating.

14th. Water dressing to wound, which is quite clean. Full diet. Dilute nitro-hydrochloric acid, twenty minims; liquor of cinchona, fifteen minims; water, one ounce: three times a day.

21st. Convalescent, and on the 25th he was discharged cured.—*Lancet, Oct.* 21, 1865, *p.* 455.

63.—ON A CURE OF ULCERATION OF THE TONGUE WHICH HAD EXISTED FOR TWENTY YEARS.

By Dr. Arthur Leared, Senior Physician to the Great Northern Hospital.

[The patient was 62 years of age, and had a sloughy oval-shaped ulcer on the middle of the dorsum of the tongue, the long diameter of which was that of a shilling. The ulcer had existed for twenty years, during which time its shape and appearance had undergone little alteration. The patient had had primary syphilis thirty years before.]

Partly influenced by the apparent non-syphilitic view of the case, I prescribed chlorate of potash in full doses, with a strong solution of the same for local application. This treatment was persisted in for more than a month without benefit. I now resolved to try the effect of more active agents. Four minims of Fowler's solution of arsenic were ordered to be taken three times a day, and a mercurial bath twice a week; the ulcer of the tongue to be touched daily with nitrate of copper. This treatment was continued for twenty-five days, the dose of arsenical solution having been increased to five minims. It was noted that after the first week the tongue had slightly improved, but further improvement, if any, was very trifling. The baths seemed to have caused a troublesome eruption between the shoulders. Donovan's solution of arsenic, iodine, and mercury was now given in doses of twenty minims twice a day.

On the fourth day of this treatment I had the satisfaction to find that the ulcer on the tongue had broken up into two patches, leaving a sound intervening portion between them. As the medicine purged, a pill containing half a grain of opium was directed to be taken after each dose.

On the tenth day the largest of the ulcers on the tongue was about the size of a split pea. The patient's feet had now become so sore that he could walk only with difficulty; there was a decided inflammatory blush round the heels and balls of the great toes, which were also very tender.

On the twenty-sixth day the tongue had healed, except in a mere point; but the feet were very sore, red, and desquamating; the palms of the hands were also becoming affected. He complained also of an aching in the back of the gums. The dose of the medicine was therefore reduced to one-half. Two days later the tongue and mouth were quite sound, with the exception of some aching in the gums. The medicine was persevered in for some time longer in diminished doses, during which the affection of the extremities gradually disappeared.

About five months after the healing of the ulcer, copper-coloured scaly patches appeared on the lower extremities. These yielded slowly to iodide of potassium with mercurial baths. He is now quite well, and the tongue and mouth have continued sound.

Remarks.—The persistence of an ulcer on so vascular an organ as the tongue for twenty years is of itself remarkable; but its cure illustrates in a signal manner the power of medicine. The most inveterate sceptic could hardly maintain that the cure had been spontaneous, and that the medicines coincidentally taken had been simply inert. Such a case is an answer to those who deny the efficacy of drugs—a thing, let me add, only denied by those who have never given their minds to the study of therapeutics. The employment of arsenic and mercury together was suggested by a natural anxiety to benefit the patient as speedily as possible; but, in reference to the determination of the separate action of the remedies, would have been better avoided. The skin eruption proved the syphilitic nature of the case, and it is an interesting circumstance that the ulcer seems to have acted as a derivative, since there was no eruption while it existed. Mercury was probably the most effective agent; but there is no reason for supposing that in certain cases of syphilis arsenic is not also highly useful. Experience convinces me of the greater utility of these powerful drugs in some chronic cases than either of them separately. Inflammation of the feet and hands is a singular effect of arsenic, only glanced at in works on therapeutics. Another well-marked instance of it occurred to me some time since in the

case of a gentleman, for whom I successfully prescribed Fowler's solution in increasing doses for a refractory form of eczema. His feet were as red and swollen, and walking as much interfered with, as if he suffered from a severe attack of gout.—*Lancet, July* 8, 1865, *p.* 34.

64.—SOLUTION OF VENICE TURPENTINE AS A DRESSING FOR WOUNDS.

Alluding to M. Nélaton's approval of camphorated spirit as a dressing for wounds in his wards, M. Kerner states that this has the disadvantage of being expensive and easily evaporable. He states that, being surgeon to the Messrs. Dulfuss' factories at Mulhouse, where from 3000 to 4000 workpeople are employed, he has ample experience in treating all kinds of wounds, and finds the following a most valuable as well as cheap application:—Venice turpentine, 2 lb 8 oz. troy; bicarbonate of soda, 6½ drachms; water, 18 pints Imperial. This is to digest in a bath for six days, at a temperature not higher than 75° C., and then filter. It constitutes a highly soluble turpentine soap, which is very cheap, and evaporates slowly. The cicatrisation of even very large wounds under its use is wonderfully rapid, the secretion of pus being very slight, while there is an entire absence of fœtidity. A compress, consisting of eight folds of linen well soaked in the solution, is applied over the whole breadth of surface, and left on (covered with oiled silk) for twelve hours, being moistened by means of a sponge with some of the solution every four or five hours. Granulations speedily shoot forth, requiring to be touched with nitrate of silver; but the discharge is quite insignificant, so that none of the encumbering apparatus required when cerates are used need be resorted to. When the wound is accompanied with crushing or contusion, leading to the fear of much inflammation, it should be exposed to a jet of cold water for some days before employing the turpentine.—*Medical Press, Sept.* 20, 1865, *p.* 274.

65.—ON ILL-SMELLING FEET.

By Professor Hebra.

[There is no effectual remedy known for this disagreeable affection. Professor Hebra has endeavoured to investigate the nature of this anomaly in secretion, and to discover the means of relieving it.]

The first question he put to himself was, whether the smell was inherent to or derived from some condition external to the economy, and he soon came to the conclusion in favour of the latter view. In repeated instances the shoes and stockings of persons suffering from stinking feet were taken away from them, and the feet carefully cleaned with soap and water by means of a nail brush. They were then put to bed, warmly covered up, and freely supplied with warm and diaphoretic drinks until a free transpiration was secured. The feet were carefully enveloped in gutta-percha paper, or other waterproof material, so as to prevent the passage of the sweat. The sweat of the entire body, though having its acid odour, did not manifest anything of the stinking character; but when the waterproof coverings of the feet were left unchanged the penetrating stink was gradually produced until it predominated. The shoes, which had been kept away from these persons, retained for weeks the filthy odour. The expression "stinking foot-sweat" is, in fact, an incorrect one, the proper one being "stinking shoes produced by an excessive production of sweat." The influence of stout, thick shoes, as a proximate cause of the odour is seen in the facts that copious sweating of the hands is not attended by ill-smell, evaporation not being impeded, and that persons going barefoot and women who wear thin shoes are seldom liable to it.

The indications for treatment are leaving off the shoes which have induced the stink, and the application of means which have been found by experience

capable of diminishing the excessive secretion. In slight cases it suffices to dust the insides of the stockings with some simple powder—such as lycopodium, alum, or even common flour. If this simple means fail, the following may be employed with certainty of success:—Some diachylon is to be gently melted over a fire, and then an equal weight of linseed oil is to be added, so as to form a homogeneous ointment. This is to be spread on linen, in which the foot, having been thoroughly washed and dried, is to be completely and exactly enveloped. Where the toes come in contact, shreds of lint covered with the ointment are to be interposed. So wrapped up, the foot is to be covered with a stocking and a light shoe, well open at the instep. At the end of twelve hours the application is to be removed, and the foot is to be well rubbed by means of a dry towel or one of the powders mentioned before, but neither washed nor bathed. It is then to be covered up with the ointment again. This procedure will be required to be repeated, according to the intensity of the evil, during eight or ten days, but the patient meanwhile is enabled to go about his ordinary occupations. After this time the ointment is to be left off, but friction by means of pulverulent substances is to be continued a while longer, and the ordinary shoes worn. After some days brownish-yellow portions of epidermis, about half a line in thickness, separate from the affected parts, leaving a white, clean, healthy epidermis behind. It is only after this separation has taken place that washing the feet or the use of a foot-bath is to be allowed; and for some time afterwards the pulverulent substances should still be rubbed into the foot. In this way, at the end of from fourteen to twenty-one days, the foot-sweating either disappears for ever, or, at least, for one or more years. In quite exceptional cases, to secure this end, a repetition of the above procedure for a second time is required; but then it is invariably successful. Professor Hebra has, during fifteen years, employed in several hundred cases this with success, and without the least attendant disadvantage.—*Allgem. Wiener Med. Zeitung.*

[M. Stanislas Martin (*Bull. de Therap.*, t. lxv., p. 143) observes that some of the applications employed for removing this disgusting infirmity are not always harmless, the arrest of transpiration having in some cases been followed by neuralgia, disturbance of the digestive organs, &c. The diffusion of the abominable stink may be effectually prevented by placing a sole containing a layer of the powdered charcoal either between the foot and the stocking, or between the latter and the shoe. A paste, composed of forty parts of powdered charcoal, forty of water, and fifteen of gum, should be thickly spread over a piece of filtering-paper, flannel felt, &c., stretched over a board or pasteboard. The paste is then covered over with another piece of paper, which is to be smoothed with the hand so as to remove all asperities. The whole is submitted to compression during an hour, after which the water is to be allowed to evaporate. When quite dry, the sole may be cut out of the required size. Being so cheaply made, these soles can be changed once or twice a-day, if required.]—*Medical Times and Gazette, Sept.* 2, 1865, *p.* 265.

DISEASES OF THE EYE AND EAR.

66.—ON CATARRHAL OPHTHALMIA.

By Henry Hancock, Esq., Senior Surgeon to the Charing Cross and Royal Westminster Ophthalmic Hospitals.

[Catarrhal ophthalmia is predisposed to by whatever tends to depress the nervous system and to vitiate the blood. It is a perfect pest to the children confined in our large pauper establishments. The disease should be treated more by general than by local means.]

From many years' experience in the treatment of catarrhal ophthalmia by local astringents, such as nitrate of silver, sulphate of zinc, &c., I am convinced, more frequently than otherwise, that their employment prolongs the complaint, and causes granulation of the lids. Freely admitting that bleeding, local or general, frequent doses of calomel, or any violent depleting reme-

dies, do harm and should be avoided, as in erysipelas, I have for some years past entirely restricted my treatment of catarrhal ophthalmia to constitutional remedies, for the most part of an attractive, a stimulating, and tonic character, merely directing the eyes to be fomented frequently with warm water, as much for the purpose of cleanliness as of comfort. I now very seldom see granular lids follow catarrhal inflammation; whilst the cure has certainly not been more protracted than when local applications were most successfully employed. The cases in which this treatment is most beneficial are those described by Mackenzie as "pure mucous or blenorrhœal inflammation of the conjunctiva;" in other words, in simple catarrhal inflammation, ophthalmia *unaccompanied by supra- or circum-orbital pain*, and for the most part confined, at all events in its early stages, to the conjunctiva and the Meibomian follicles. In these, the disease mostly comes on suddenly, commencing with a sensation of itching and stiffness, speedily converted into a feeling as though grit or sand were in the eye. Upon examination, the eyelashes are found loaded with thickened glutinous mucus. The eyelashes are swollen, their lining conjunctiva being of a darkish red color and flabby; whilst that of the eyeball, in the milder forms of the disease, is traversed by a network of vessels of the same dark red colour, which can readily be moved by pressing the lower lid against the eyeball, which in most instances is marked, especially at its lower half, by patches of extravasated blood, distinguishing this disease from the more acute and phlegmonous inflammation of the conjunctiva.

In its most simple form, the disease will be restricted to the angles of the lids and adjacent conjunctiva. When more severe, it will extend over the whole surface of the lids and eyeball; whilst, in still more severe and serious cases, the subconjunctival cellular tissue is infiltrated and raised, forming a distinct elevated fleshy ridge around the cornea, which appears sunken in, and at times partly obscured by irregular masses of swelling, investing its surface, and constituting that condition termed chemosis. At the same time, the conjunctiva of the eyelids becoming in like manner infiltrated, the latter are puffed and swollen, creating difficulty of opening the lids, and in this way impeding vision.

Even when the amount of chemosis is considerable, the cornea for a time remains clear; but, if this condition exist for two or three days, it will become hazy, opaque, yellowish, and ultimately slough and give way, allowing the iris to protrude, resulting in more or less destruction of vision.

Lachrymation is always increased, the secretion being sometimes quite clear, at other times thick, yellowish, or mucopurulent. The symptoms towards night increase in severity; the intolerance of light is greater; the patient is feverish, and complains of pain and weight in the head, principally in the region of the frontal sinuses, and in the course of the lachrymal sac and ducts. These symptoms usually subside after the patient is quiet in bed; but they are sometimes so severe as entirely to prevent rest or sleep. In such cases, the Dover's powder or opium is of the greatest service, and will frequently cut short the attack. In ordinary cases, I give bark and ammonia; but, when the patient resides in a low damp locality, it may be necessary to give the sulphate of quinine in full doses, with or without opium, according to the intensity of the pain and the irritability of the patient; whilst the local applications should be restricted to warm water, or at most a poppy-head fomentation, to the exclusion of all local remedies of a stimulating or astringent character.

So long as the tongue is coated, and the breath foul and offensive, mild alterative aperients, such as pilula hydrargyri, with hyoscyamus and extract of colocynth, are of great service, especially where there are chemosis of the conjunctiva and tendency to opacity or sloughing of the cornea. In such cases, bleeding, whether local or general, violent purging, and local applications of a stimulating and astringent character, do considerable mischief, whilst, on the other hand, I have commonly seen the happiest results from mild alterative aperients—bark and ammonia or quinine, with or without opium, according to the amount of pain; with warm and soothing local applications.—*British Medical Journal, June* 24, 1865, *p.* 633.

67.—ON THE YELLOW AMORPHOUS OXIDE OF MERCURY AND ITS APPLICATION IN CONJUNCTIVITIS AND CORNEITIS PHLYCTENULOSA.

By Dr. PAGENSTECHER, Wiesbaden.

Red precipitate, the red oxide of mercury, has hitherto played an important part in the treatment of the superficial diseases of the eye, and in many instances constitutes the basis of several secret popular remedies. It is especially used in various kinds of blepharitis, in the form of an ointment, and valued by every oculist as a very favourite remedy. In diseases of the conjunctiva, and cornea too, when the object of the surgeon was to produce a stimulant or alterative effect, we find in the "Pharmacopœia" several compound ointments of this description recommended. But these ointments mostly possess the disadvantage of being inapplicable to all these forms of disease, and especially so to conjunctivitis and corneitis phlyctenulosa, being often found to irritate too much. Numerous and prolonged trials that I instituted years ago, proved to me that the red precipitate in ordinary use was, from its crystalline form, not sufficiently finely divided, and consequently did not act uniformly on the surface of the diseased membranes; very often, too, got retained in the folds of the mucous membrane, and there set up a caustic action, which was rather prejudicial than otherwise to the subsidence of the disease. I became positive of this fact from some experiments I instituted with this substance, which was first reduced by many hours' trituration to as fine a powder as possible, using as a vehicle fat, which melted quickly from the natural warmth of the conjunctiva. Thus the remedy was, by the movements of the lids, equally distributed over the diseased surfaces; and at the same time, the probability, or rather the possibility, was avoided of any irritation being produced by its remaining too long in the conjunctival sac. Even these experiments yielded by far more favourable results. But the ointment was rendered still more perfect when I, acting under the advice of apothecary Dr. Hoffmann, substituted the yellow amorphous oxide of mercury. This preparation, which up to that time (1856) had found no place in the pharmacopœia, but was well known to chemists, possesses the great advantage of being in a state of the finest possible division, prepared, as it is, by precipitation; and being altogether destitute of any crystalline form, it does not, like the red precipitate, adhere to the conjunctival tissue by any fine points and angles. But this preparation differs materially not only in form, from those which have hitherto been employed, but also in its chemical properties, which explain and render intelligible the results obtained in practice. I may be permitted to give the preparation of the ointment in Dr. Hoffmann's own words:—

"Two forms of oxide of mercury are recognized:—

1st. The crystalline or red oxide, prepared by the dry method, and commonly known as red precipitate, constituting the very common remedy; and

2nd. The amorphous, or yellow oxide, prepared by the wet method by precipitation; up to within a few years unknown to the pharmacopœia, although it is indubitably preferable to the first.

The commond red precipitate is rendered applicable to practice by being triturated in a porcelain mortar till no more brillant crystalline points can be perceived: a powder is thus obtained, which is quite soft, and when rubbed between the fingers no longer imparts any gritty feel. If this, after being prepared in the most careful possible way, is submitted to the microscope, it may, under a magnifying power of even 120 diameters, be recognized as a mass of broken crystals. The point up to which the trituration should be continued, which forms the measure of the fineness of the division, is in this method uncertain and inconstant. Thus this preparation occurs in different degrees of fineness in different shops; and as its efficacy is intimately connected with its fineness, the surgeon gets preparations which act with unequal strength.

For obtaining, therefore, a preparation uniform in its effects, and in the finest possible state of division, the yellow precipitate, which is thrown down, is highly to be recommended. Thinking this would also prove a far more energetic preparation. I, in 1856 for the first time, prepared some, and recommended its use to Dr. Pagenstecher in his eye-practice, instead of the common precipitate, and found my anticipation most gratifyingly confirmed. The mode of preparing the yellow precipitate, although well known, may be still worth mentioning. Care must be taken in the precipitation to obtain a pure oxide, and not any of its compounds, to which precipitates of mercury have a great tendency—a fact which might detract from the efficacy of the preparation. The precipitation is effected by adding a solution of the chloride of mercury to a solution of potash, in such a way that there is always an excess of the latter. After the precipitate has deposited itself, the supernatant fluid it at once poured off, the precipitate thoroughly washed with distilled water, and dried by a gentle heat, with exclusion of daylight. Thus prepared, the yellow precipitate has a light-yellow (that of the yolk of egg) colour, and is an exceedingly fine powder, which, even under the microscope, appears completely amorphous. In addition to both the above-signalized properties, it differs from the ordinary precipitate in its chemical behaviour, being much more quickly acted on by reagents. A solution of oxalic acid, which acts on the red oxide only after boiling, very quickly changes the yellow oxide, even at the ordinary temperature, into the white oxalate. The preparation of hypochloric acid gas depends on the property the yellow oxide of mercury possesses of decomposing in contact with chlorine gas; the results being hypochloric acid and chloride of mercury; whereas the red oxide undergoes, with chlorine gas at the ordinary temperature, hardly any change. This difference of chemical behaviour of the two oxides constitutes a different degree of resistance to the various agents they are submitted to, and is explained by their different states of cohesion. In respect to the use of the yellow precipitate for eye-ointments, I may be allowed to say a few words on the vehicle of the ointment. The most perfect vehicle for an eye-ointment must be very soft, without, however, being too fluid, lest the heavy oxide sink to the bottom; but when in contact with a moderate heat of the body, it must completely melt, so that the preparation it contains may become quickly and uniformly diffused over the eye. Besides this peculiarity of consistence, the vehicle must be, as far as possible, indifferent in its behaviour to the oxide, and exhibit the least possible tendency to rancidity, which might exert a deoxidizing, reducing action on the oxide. Numerous experiments with hog's lard, butter, glycerine, glycerine ointment, and mixed fats, have led me to give the preference to the last; and I recommend either the mixture of spermaceti, wax, almond-oil, and rose-water, known as 'cold cream,' only omitting the water, as this favours rancidity, and substituting for it quantities of almond-oil, varying according to the heat of the weather; or a mixture of butter, of cocoa, and almond-oil, likewise proportionate to the temperature. In both compounds the almond-oil must be as fresh as possible, and had best be prepared by the apothecary himself.*"

The ointment is indicated in conjunctivitis and corneitis phlyctenulosa, and all the allied and resulting forms of corneal diseases. To avoid any misunderstanding, I will enter a little further into the symptoms and nature of this disease. At the risk of repeating what has been long known and universally accepted, I must give a short sketch, in order to indicate in

* As regards the strength of the ointment, I generally use one drachm of oxide to one ounce of fat. This may appear very strong to some, but experience amply shows that, applied in proper cases, it does not in any way irritate too much. Idiosyncrasies may, of course, be observed, as in every remedy; and if the ointment in a given case irritates too much, its strength may be reduced to 30 grs. of oxide to the ounce. I may further remark, the two constituents of the ointment must be rubbed up to a most intimate admixture, if it is to act well. The following are, then, the two formulæ:—

℞. Hydrarg. oxydat. flavi, gr. xxx. (via humida parati). Ung. cetacei, ʒss. Misce exactissime et fiat unguent.; or,

℞. Hydrarg. oxydat. flavi, gr. xxx. (via humida parati). Ung. cetacei, ʒj. M. exactiss. et fiat unguent.

what forms and under what conditions I principally recommend the use of the ointment. I consider conjunctivitis and corneitis phlyctenulosa, which have been called by different authors by the most different names, according to the views they took of their causes, as a purely local disease of the conjunctiva and cornea, as one and the same condition, which becomes modified secondarily by the constitution of the patient. The anatomical, typically characteristic, form consists in vesicles, or pustules, on a basis of infiltrated tissue of the ocular conjunctiva; they are developed mostly near the cornea at the limbus conjunctivæ, or, from the very commencement, on the cornea itself. These vesicles soon pass, as the epithelium gets destroyed, into raw surfaces, the surrounding infiltrations becoming removed, a loss of substance in the form of an ulcer thus ensuing. The morbid action varies extremely in its intensity; so does the extent of surface denuded of its epithelium; but a greater or less development of vessels is always observed as a concomitant phenomenon. The mode of arrangement of the vessels around the vesicle is somewhat peculiar; the vessels are all directed centripetally towards the vesicle. If the vesicle is on the ocular conjunctiva, the vessels run uniformly from all sides towards it as a centre; but if the vesicle is at the edge of, or on the cornea itself, the vessels also run to it from the adjacent parts of the conjunctiva; and thus aggregations of highly hyperæmic vessels are seen, which often rise above the level of the conjunctiva, whilst the rest of the conjunctiva exhibits comparatively few signs of irritation. If, on the other hand, the vesicles are developed more or less over the whole ocular conjunctiva, this latter may assume a more general redness. This may also occur if the vesicles form over the entire edge or surface of the cornea. As a rule, the formation of vesicles is limited to the cornea only, when the conjunctiva had been previously affected. The dependence on and relation to the conjunctival disease of any given purely corneal vesicle is perceived in the higher degree of redness and vascularity of the corresponding part of the conjunctiva; a pointed triangular leash of vessels running beneath the corneal epithelium very often forms the connection between the vessels of the conjunctiva. As the vascularity gradually invades the greater part of the corneal surface, we get the form of disease described by authors as corneitis vasculosa, scrophulous pannus, pannus phlyctenulosus. We very often see the disease represented by one single vesicle on the cornea, which runs the usual course, *i. e.*, passes into an open ulcer, as the epithelium, separated by the fluid in the vesicle, gets detached, after the vesicle has, as above remarked, become connected by a narrow leash of vessels with those of the conjunctiva. At the border of the ulcer next the edge of the cornea a fresh turbid swelling of the cornea ensues, and this, as it advances towards the centre, is continuously followed by the bundle of vessels, constituting one of the most obstinate corneal diseases, known as corneitis fasciculosa. These anatomical phases may, under certain circumstances, undergo further changes, either from local irritation, want of cleanliness, wrong treatment, or from constitutional predispositions, such as the lymphatic or scrofulous crasis.

Or from a continuance of such unfavourable circumstances, ulcers at first of very limited extent appear to spread in area, or even invade the deeper layers of the cornea, when we have to contend with corneitis ulcerosa, or corneitis profunda—forms to which the ointment is often inapplicable.

In the above briefly indicated forms of corneitis, every practitioner will, I presume, agree with me in recognizing one and the same type of disease that acquires its special form from the different kinds of irritation or structures in which it has originated. In all these forms I assert the yellow oxide of mercury ointment is a certain and almost specific remedy.

The good effect of the ointment is most displayed in the more chronic cases, after the originally greatly increased irritation of the cornea has somewhat abated, and the vascularization appears under the form of what is generally known as passive congestion. If the disease is in a more acute stage, the ointment may be applied from the very commencement, if care is taken to completely remove it from the conjunctival sac, as some may, from the spasm of

the lids which is associated with the photophobia, get retained in the folds of the mucous membrane. The disease may possess the peculiarity of the phlyctenulæ recurring. Fresh phlyctenulæ then form near the ulcers, always giving rise to increased irritation. Our attention is attracted by increased intolerance of light, lacrymation, and ciliary neurosis; an accurate examination then generally reveals small abscesses or phlyctenulæ. As soon as the abscess has, by breaking through the epithelial layer, discharged its contents, the previous state of quiescence at once returns. These intercurrent attacks must not deter us from continuing the application of the ointment; for it rather accelerates, than otherwise, the course of the disease. Only when the base of the ulcer is extending deeper than Bowman's layer, into the cornea-substance proper, must we exercise caution in using the ointment, and only try it in the first instance as a matter of experiment. If we, from the too long continuance of the stage of reaction, gather that the ointment irritates too much, we must wait for a day or two before trying it again. In such cases it is as well to employ also remedies of known sedative effect, such as atropine, a compress-bandage at night, warm fomentations of 35° R. for a few hours. It may also happen that superficial ulcerations of the cornea under unfavourable circumstances pass into purulent corneitis; purulent infiltrations in the layers of the cornea or hypopyon form, and, of course, here to apply the ointment is out of the question. Only after the purulent corneitis has passed into the reparative stage, may we again try the ointment, and may be surprised at the rapid regeneration of the corneal substance that ensues. The ointment is also an excellent means of clearing the cornea in all those exudations which persist after inflammations, especially in those cases in which the insufflation of calomel has hitherto been employed. I can further recommend it in mild cases of scleritis, limited to the superficial layers of the sclera (episcleritis) unaccompanied by any marks of irritation in the iris. In all the above forms of disease we may convince ourselves that the ointment acts favourably, more or less rapidly, and in but few cases is it necessary to employ any constitutional treatment in addition to the local. Purges are, as a rule, of no use, and cannot be recommended, for the reason that all weakening of the constitution must be avoided. Derivations through the skin, counter-irritants, blisters, act, according to my experience, injuriously rather than favourably on the eye-disease.

The frequent occurrence of conjunctivitis phlyctenulosa along with diseases of the skin, eczema, crusta lactea, lupus, and generally in persons with a very irritable skin, explains the above fact sufficiently, as the effect of this remedy is not counter-irritant or derivant, but the very reverse—to increase the already existing irritation. The patients require a more strengthening and tonic treatment, as they have often been already too long kept for weeks in dark rooms, and prevented enjoying the fresh air. We order, therefore, above all, a good nutritious diet, going out into the fresh air, careful attention to the skin by cleanliness, cold water, and friction. These must be especially attended to. They materially assist the local treatment. If children suffer from great intolerance of light, giving rise to a nearly constant spasm of the orbicularis, I have often seen quite surprising results from suddenly dipping their faces in cold water, as recommended by von Gräfe.

The contra-indications of the ointment may be briefly enumerated, as they are easily self-comprehensible. As regards the other inflammations of the cornea, no good effect can be observed from the yellow oxide of mercury. Corneitis purulenta, blenorrhoica, or corneitis vasculosa, originating in granular lids and trachomatous pannus generally get worse under it. In syphilitic corneitis parenchymatosa it has no effect one way or the other; but in the consequent obscurations of the cornea the ointment may be used, after all acute symptoms have completely vanished, to clear the cornea. If any iritis coexists, the ointment must be studiously avoided, as well as in all deep infiltrations and ulcerations of the cornea. I have often asked myself on what physiological fact the good effect of the yellow oxide of mercury depends; but must confess my inability to satisfactorily answer this question. Its immediate effect is undoubtedly irritant; for any previous vascularity is at once

heightened on its application. The immediately increased flow of tears and disagreeable feeling of pain in the conjunctival sac, which are not perfectly explicable by its action as a foreign body, prove there is no necessity for the application remaining any length of time on the diseased parts to produce its effects. When the ointment is applied for the first time, the irritation may persist for several hours, till it gradually sinks to its previous degree, some, easily coagulating, mucus being at the same time secreted from the conjunctiva; a certain feeling of comfort even often ensues, which is attributable to the diminution of the extreme photophobia. On the second or third application, always observing an interval of four-and-twenty hours between each, the immediate irritant effect is already much less in degree and continuance than on the first. The eye gradually so accustoms itself to it that generally after it has been used for a week the re-action only lasts from a quarter to half an hour. It is principally the intolerance of light that markedly decreases, and *pari passu* the flow of tears. The mucous membranes lose their turgescence, partly by the vessels becoming diminished in calibre, partly by their disappearing altogether, and partly, also, by absorption going on in the swollen tissue itself, the mucous membrane acquiring a more dry appearance. The raw exudation-surfaces, whether on the conjunctiva or cornea, lose their rough appearance; their yellowish colour gets more grayish-white; at last they become quite smooth; if they were deep in the substance of the cornea, they get more raised at their edges, and clear up. On the site of the previous ulcers a fascetted surface may remain for some time; that, however, as the corneal substance becomes regenerated, becomes smooth and covered with epithelium. This process agrees perfectly with the ordinary course of the healing of these ulcers; it is the rapidity with which we observe the regeneration and repair to take place under the useof the ointment that surprises us. As regards the photophobia, we possess in the yellow oxide of mercury an excellent sedative, due, it is true, to the previous brief irritation, just as we explain the similar action of nitrate of silver in catarrhal ophthalmia. Red precipitate has been long known as a particularly excellent application in indolent ulcers. The red precipitate in many cases irritates too much; but the yellow oxide of mercury, probably from its fine state of division, agrees better with the more delicate structures of the eye, and only excites that slight amount of irritation which is necessary to exert an alterative action on the diseased tissue, and on the, perhaps exposed, nerves.

I must, finally, call attention to a very important matter, namely, the method of applying the ointment, as its results depend in great measure on this. The ointment is to be applied only once a day; best with a small brush, which is dipped in the ointment and applied between the eyelids; these, by their closure, rub off the ointment from the brush. If the ointment is properly made, *i. e.*, has the right consistence, it gets, by the movements of the lids, diffused over the whole conjunctiva and cornea, and then soon, by the same agency, extruded from the eye. It is best then to wipe it off with a piece of linen, lest it, by remaining too long in contact, create any undue irritation. If the movements of the lids are insufficient to completely remove it from the conjunctiva, we may try to effect this by gently rubbing the closed lids, and afterwards raising both the upper and lower lids from the surface of the globe.

Any ointment that still remains becomes then very quickly washed out by the tears. If the ointment is not well made, *i. e.*, if it does not melt at once on its application to the conjunctiva, some may remain in the reflection from the lids, and give rise to serious signs of irritation, from an extensive slough forming, and, as a consequence, a partial separation of the mucous membrane. It is, therefore, as well to inspect the conjunctiva again a few minutes after applying the ointment. On this account the ointment is better suited for patients on whom the surgeon is in a position of watching its immediate action. However, I do not hesitate to give it to intelligent patients for them to use at home, after having instructed them in its mode of application, and the precautions to be observed in its employment. Cold applications after its use generally very quickly subdue the first somewhat violent signs of irri-

tation. If the eye still exhibits any irritation after the expiration of one or two hours, it disappears after a walk in the open air.

I am convinced that every medical man will in proper cases recognise the efficacy of the yellow oxide of mercury.—*Ophthalmic Review, July*, 1865, *p.* 116.

68.—VASCULAR CORNEÆ AND GRANULAR LIDS OF NINE YEARS' STANDING—CURE BY PURULENT INOCULATION AFTER PERITOMY.

Case under the care of J. Hutchinson, Esq., Metropolitan Free Hospital.

[The patient was a poor Irish girl of rather feeble health, who for nine years had had persistent ophthalmia, with granular lids, and vascular corneæ. The corneæ were completely covered by vessels.]

Mr. Hutchinson drew attention to the fact that it was in these cases that the measure was comparatively without risk—precisely the cases most needing it being those in which it could be done with impunity. The more vascular the cornea the greater the probability that no sloughing would follow, and *vice versâ*. In well-selected cases he had scarcely ever seen any harm result. Recently it had been the practice not unfrequently to perform circumcision of the conjunctiva first, and then to inoculate a few weeks afterwards; and this was the plan he proposed to adopt. The following are the notes of the case:—

June 8, 1865.—B. S., aged 15, came to-day to report her condition, she having been discharged about nine months ago, and not seen since. It is just a year since her admission. She had then suffered from granular lids and vascular corneæ for nine years, and was only able to see the largest objects. She had been treated at various hospitals and with great variety of treatment. A week after her admission Mr. Hutchinson performed circumcision, removing a broad belt of conjunctiva around each cornea, and being very careful to clear away every trace of tissue down to the sclerotic. About six weeks later but little benefit was apparent. Both eyes were now inoculated from those of an infant suffering from purulent ophthalmia in a moderately severe form. A fairly acute attack of purulent inflammation resulted, which was allowed to run its course, free ablutions only being resorted to. When the ophthalmia was subsiding a weak alum lotion was used. She left the hospital three months after her admission. At present date, a year after operation, both corneæ are clear, with the exception of a degree of haze so slight that it requires very careful examination to detect it. There is no vascularity of the conjunctivæ, and the granulations under the lids have entirely disappeared. She can see perfectly. The eyes are at times a little irritable. The belt from which the conjunctiva was removed has been so perfectly restored that it is almost beyond belief that such an operation was ever performed.—*Medical Times and Gazette, June* 17, 1865, *p.* 625.

69.—AN IMPROVED SPECULUM FOR THE EYE.

By J. Z. Laurence, Esq., Surgeon to the Ophthalmic Hospital, Southwark.

I shall briefly enumerate the disadvantages of the wire speculum in ordinary use for exposing the eyeball for operations, and the remedies I have introduced in my form of instrument.

1. The parts of the instrument which are directly employed in separating the eyelids are too short—they do not separate them in their entire lengths; in my instrument these parts of the instrument are increased to about seven-eighths of an inch.

2. These same parts are straight; in my instrument they are gently curved, in conformity with the natural curves of the palpebral sinuses; thus any separate and unnatural dragging on individual parts of the eyelids is avoided.

3. Whenever the patient resists, more or less violently, by powerful spasmodic action of his orbicular muscle, the separation of the eyelids by the wire speculum, the eyelids, especially the upper one, are forced down over or under the wires, so that the part of the eyeball exposed to view is materially diminished; indeed, in certain operations (*e. g.*, those on the cornea, iris, and lens), more especially when the eyeballs are much sunken or small, the action of the wire speculum is practically annihilated.

This disadvantage I have met by substituting for the wires, which should separate the eyelids, grooves of metal—troughs, if I may so say—which receive the eyelids and absolutely prevent their overlapping, and thus contracting the surgeon's field of operation.

1. With the head-rest fix the head; 2. With this speculum expose the globe fully; 3. With bulldog forceps fix the eyeball itself, and you may operate on the most unruly subjects and children without assistants, without chloroform (where the nature of the operation does not specially indicate it), with the greatest ease and certainty. My own experience in all kinds of operations on the eye of the preceding method of securing immobility for the purposes of operation is most favourable. I now submit it to others less prejudiced perhaps than myself.

The "groove speculum" is made by Messrs. Weiss, 62, Strand, London. —*Med. Times and Gazette, June* 24, 1865, *p.* 647.

70.—A SIMPLE MODE OF EXAMINING THE FUNDUS OCULI.

By Dr. A. M. Rosebrugh, Toronto.

I find that the fundus of the eye can be examined without the mirror of the ophthalmoscope as follows: The lamp is placed on a table in a darkened room. The patient is seated on a chair two or three feet from the table, and facing the light. The chair of the observer is placed with the back against the table and between the patient and the lamp. When the surgeon is seated, the lamp should be immediately behind the right shoulder and very nearly on a level with the eye. By now bringing the right eye in a line with the lamp and the eye of the patient, but so as not to shade it, if the pupil had been previously dilated with atropine—the fundus is seen to be illuminated. In order to see the details of the posterior internal surface of the eye, a lens of about two inches focus is placed within an inch or two inches of the eye under examination. An inverted image of the fundus can now be seen forming an aerial image about two inches in front of the lens.

The optic nerve entrance and the radiations of the retinal vessels can be seen by directing the patient to look at the light, or two or three inches farther from the surgeon's eye, but in the same horizontal line.—*Canada Med. Journal, Sept.*, 1865, *p.* 106.

71.—ON CATARACT.

By Dr. A. von Graefe.

[Any preparatory treatment of the patient by the administration of purgatives is generally injurious. The atropine should be applied the evening before the operation. For six years previous to the use of the compress bandage, the upward section was always employed by Dr. Gräfe, but since then he has preferred the downward section. He says:]

My experience is, that there is no notable difference between the two, if equally well performed. The average duration of recovery after the upward section is three days less (from twenty-one to twenty-four days in hospital); but as regards the percentage of prolapses of the iris, of iritis, or of suppuration, there is no appreciable difference. On the other hand, partial suppurations terminate more favourably after the downward than after the upward section, in which latter the gravitation downwards of the morbid products

exerts a deleterious influence. In favour of the downward section we must further remark, that the operation itself is carried out more perfectly, in consequence of the greater rapidity and facility with which it may be performed, and of the surgeon being less dependent on the tractability of his patient, and on the assistance during it. The operator can avoid any rubbing or eversion of the flap by the eyelid, even in patients who have but slight control over the movements of their eyes. He can completely remove any remaining cortical débris without the necessity of introducing any instrument, and so forth. One may, it is true, with sufficient practice, bring the upward section to a successful issue, even in the case of sunken eyeballs, fractious patients, &c.; but the operation may be thus prolonged, or the apposition of the flap interfered with by the upper eyelid, even with the most skilful—all circumstances which add to the injury inflicted on the eye, interfere with the perfection of the operation, and are thus calculated, in doubtful cases, to turn the scale in an unfavourable direction.

As generally occurs in estimating the relative advantages of particular methods of operating, a variety of reasons have been advanced, pro and con, which, when tested by experience, are found to be for the greater part fallacious. Thus, for example, the downward section has been said to favour the escape of vitreous, in consequence of gravitation downwards. The reverse would be nearer the truth. Vitreous escapes, if the zonula Zinii or the fossa hyaloidea is burst. Such an occurrence is less likely to happen the easier the exit of the lens and of any cortical fragments is effected, as it undoubtedly is in the downward section. It has been further stated, that in case of any corneal cicatrices remaining, these impede vision more in the downward than in the upward section. No cicatrices of any moment, however, occur, excepting the case has gone wrong; either that the iris has prolapsed, or partial suppuration has occurred. In the former case the pupil becomes displaced, which is on the whole less desirable upwards than downwards; in the latter case the pupil is either closed or narrowed, and must, whether or no, be remedied by an iridectomy. It has also been urged against the downward section, that if the flap is not in accurate apposition, the edge of the lower eyelid easily gets into the wound. Such a malposition is, however, certainly exceptional, and must be remedied, whichever method has been employed. Either a portion of iris must be excised, or even some vitreous allowed to escape—operations which are far better performed after a downward than after an upward section. After a successfully executed downward section, the flap is so accurately adjusted that, even if the patient looks suddenly downwards, the lower eyelid does not cause the wound to open. Even in the case of abnormally protruding eyeballs, I am not at all afraid of extracting by the downward section, since I have methodically employed the compress-bandage. I do not mean to say I am a decided opponent of the upward section. It would be an injustice against the excellent results that have been obtained by many operators by that method, as it would be against my own previous experience.

I have myself before admitted that the medium duration of the healing of the wound is shorter after the upward section. This is caused by the winking of the eyelids keeping up rather longer some irritation in the wound after the lower section, it being covered by the lid in the upper section. I only wanted to direct attention to the fact that many of the so extravagantly vaunted advantages of the upward section are fallacious; that the real advantage of the method consists in the wound being equally covered by the upper eyelid during the first stage of the reparative process; but that this is in great measure replaced by the compress-bandage, and is hence counterbalanced by the greater quickness of performance and the less dependence on the patient that ensues in the other method. Assuming, what I will not contest, that, on the whole, both methods yield equally good results, still the easier performance of the downward section must always preponderate. At all events, I recommend every one—the practised as well as the unpractised surgeon—to operate on intractable patients by the downward section.

We operate on the patient usually in *bed*. Although the operator expe-

riences a little more inconvenience in this position, still the patient has the advantage of being in the most complete muscular repose, and of being able to remain in the same position after the operation. Surgeons who operate upon their patients in the sitting posture are obliged, if anything unusual occurs during the operation, to make them lie down,—as in certain accidents, such as loss of vitreous, *e. g.*, the horizontal posture is indispensable. The operation being on the right eye, the surgeon must sit behind the head of the bed, unless, indeed, he has the natural gift of being ambidexter, or, during the time of his professional education, in accordance with the now exploded whim of oculists, taught himself to become semi-ambidexter. Every one may follow his own taste. All we would do is to protest against such surgical display, and warn those who still harp on being ambidexter, that to create mechanical difficulties, which may be very simply avoided, does not constitute a gain, but only shows a want of prudence. Surely no surgeon would be regarded as ingenious were he to amputate one leg with the right, the other with the left hand, instead of placing himself in the suitable position that would enable him to amputate either with the right hand. And in operations on the eye there is no difference, as long as the principle is adopted of operating on the patient in the recumbent posture.

Immediately after the operation we apply the *compress-bandage*, which we are in the habit of employing in our practice. This consists in equably padding the orbit with picked lint in the form of little disks, over which a simple ascending turn of a flannel bandage is moderately tightly drawn across the eye. This is made fast by a turn round the forehead, half of which passes in front, half behind the ascending turn over the eye. The middle part of the bandage which passes over the eye is made (instead of flannel) of knitted cotton. The proper application of this bandage is especially important in collapse of the cornea. For the first few hours pretty firm pressure may be exerted, which must subsequently be gradually relaxed, so as not to check too much the escape of any discharge from the eye.

Is *opening the eyelids* attended with any danger? We are for deferring opening the lids in a case which runs a perfectly favourable course, till the fourth or fifth day, because it is, to say the least, unnecessary, or, under certain circumstances, even injurious. But if the case does not go on well, we should not hesitate to cautiously inspect the eye, so as to be certain in what condition it is. Supposing we found general suppuration, we should give up all hopes of saving the eye and confine our treatment to what best suited the comfort of the patient and his general constitution. But if suppuration of the wound was but partial, and prevented it uniting, our hopes would not yet be gone, and we should have to be guided in our treatment by narrowly watching its effects.—*Ophthalmic Review, Jan.*, 1865, *p.* 313.

72.—ON THE REMOVAL OF CATARACT BY THE SCOOP. METHOD, OR THE METHOD BY TRACTION.

By George Critchett, Esq.

[It is admitted by all that cataract occurring in persons who have passed the middle period of life should be extracted. This is generally done by the flap operation. In a considerable percentage of cases, however, imperfect sight and loss of sight occur. It is an interesting subject of inquiry, therefore, whether there is any safer method of removing a cataract.]

The indications for the performance of a safe operation, or rather an operation in which the elements of safety are most completely fulfilled, may be stated as follows: to avoid as much as possible all the rocks upon which we may be said to split in the flap operation; thus the opening in the cornea should partake rather of the character of a slightly curvilinear slit than a semicircular flap, but it should be of sufficient size to admit the ready exit of the lens when gently drawn out by means of a suitable spoon. In the next place a portion of the iris should be removed so as to facilitate the exit of the cat-

aract, prevent the possibility of prolapse, and diminish the liability to iritis and closed pupil. In the last place we must endeavour to remove the entire cataract, leaving no fragments behind, without, at the same time, bruising the iris or contusing the edges of the wound, and without rupturing the hyaloid membrane. If these indications have been carefully and efficiently carried out, every condition for obtaining a successful issue has been fulfilled, and the happiest result may be legitimately expected. Previous to describing the operation that is now adopted in a large majority of cases by some of my colleagues and myself at the Moorfields Hospital, and which I may state as one of the main objects of the present paper, I will endeavour to trace the various steps by which the operation has been brought to what I will venture to call its present advanced stage towards perfection. In the writings of Professor Graefe we find frequent allusion to the removal of a portion of iris as a desirable precaution in certain cataract operations. Thus, in linear extraction, which was initiated almost simultaneously by Mr. Bowman in this country, and by the Berlin professor, the latter advises a removal of a portion of the iris in some cases as giving additional chances of success. In cataract complicated with old pupillary adhesions, he advises iridectomy previous to flap extraction. More recently he advised, in certain ordinary cases of flap extraction, that an iridectomy be performed some weeks previously, and still more recently he recommended that linear extraction should be applied to cataracts of medium density by introducing certain modifications into the operation, and he removed in this way, with a scoop, cataracts through a large linear opening, an iridectomy having been also performed. Having thus given an impetus to this important modification of the old method of flap extraction, the idea has been worked out in different ways by some of the more distinguished of his pupils on the continent, and also at the Moorfields Hospital by Mr. Bowman and myself. Thus Dr. Mooren took up the plan of performing a previous iridectomy, and then, at the end of five or six weeks, performed the ordinary flap extraction, he published a large number of successful cases, and urged the plan as meriting universal adoption, and it is now associated with his name. Professor Jacobson combined the ordinary flap extraction with an iridectomy performed at the same time; in a similar way he supported his claims by a large array of successful cases, and his name remains attached to the method; and Professor Schuft (Waldau) worked out more fully the modified linear extraction, suggested a peculiar form of spoon, and published a paper in which the various stages of the operation are fully described, and the method supported by the citation of numerous successful cases. Thus suddenly appeared three new methods of operating for cataract, bearing the names of their several champions—the method of Mooren, that of Jacobson, and that of Schuft (Waldau); but justice compels me to state that these gentlemen lighted their tapers at the torch of their great master, Professor Graefe. Each of these methods had been previously suggested and practised by him, but only in exceptional cases instead of as a general rule. It does not come within the scope of this paper to enter analytically into the respective merits of those three methods. The title of the paper by implication shows my bias in favour of scoop extraction; it seems to me desirable to avoid the formation of a large flap wound of the cornea, the disadvantages are obvious, and have been already alluded to in this paper; the only advantage to be placed on the other side of the question is the ready escape of the cataract, and if this can be accomplished as well through a curvilinear opening, as I have already said, there seems to me that a considerable advantage is gained. Soon after the publication of Schuft's (Waldau's) pamphlet, the operation was put upon its trial at Moorfields, both by Mr. Bowman, myself, and others, and from that time until the present, with certain modifications, we have continued to perform it in an increasing proportion of cases, and during the last year I have exclusively performed scoop extractions. The operation performed at the present time by myself and that by Mr. Bowman differs somewhat, both in the mode of performance and in some of the instruments employed; I shall, however, confine my remarks to my own method of performing, and to a description of the scoop that I employ, and I have reason

to believe that Mr. Bowman will add some remarks upon his method of proceeding, and his views as to the comparative value of the operations; remarks. I need hardly say, that must be most interesting and welcome to every ophthalmic surgeon. I find, on reference to my note-book, that I performed my first extraction with Schuft's (Waldau's) spoon on the 17th July, 1860, at the Moorfields Hospital, and I continued to employ it until February, 1864, when I began to use the scoop that I now employ, and that is figured in this paper. My first few cases happened to be very successful, but as my experience increased I was less fortunate; I became more and more convinced that success depended upon great attention to the small details. I also became convinced that the spoon I was employing was seriously defective in form; owing to its broad, thick frontage and to the sharply defined edge of its dish-like cavity, it was difficult to introduce and to insinuate behind the lens. The sharp edge had also tendency to break up the lens into fragments, and to force some of these into the space behind the iris, where they remain fixed for a time and then become washed into the pupil, or on the anterior surface of the iris, or into the wound, and set up serious reaction. Again, the

FIG. 1. FIG. 2. FIG. 3.

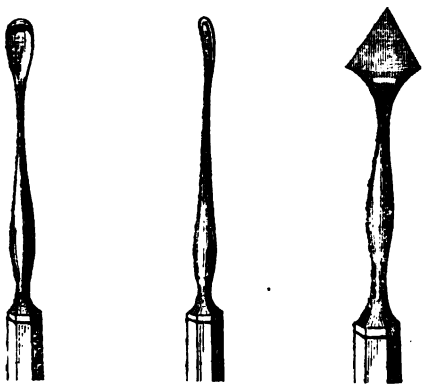


FIG. 1. and 2. Form of spoon employed by Mr. Critchett.
FIG. 3. Spear-shaped knife (Keratame) used for making the corneal incision

edges of the spoon are very liable to bruise the iris and the edges of the wound, and in cases of hard senile cataract it was all but impossible to insinuate it behind the lens without displacing it, and risking a rupture of the hyaloid membrane, and even if this were accomplished, the bulk of the spoon, and the space that it occupied, added seriously to the difficulty of bringing the lens out through the corneal wound. The result was that some unfavourable cases occurred, and by degrees the operation fell into discredit, particularly on the continent, and was quite given up by several who were disposed at first to adopt it. This was partly to be attributed to defects in the instrument employed, and partly to errors in the mode of conducting the operation, —errors that must necessarily arise in every new proceeding in which the details are not fully understood, and for which a special manual dexterity has not been as yet acquired: some fresh education being necessary even for those who were previously dexterous operators. Some of my cases, however, had made such a rapid recovery, had obtained such excellent sight, and had given me so little trouble and anxiety, that I became strongly impressed in favour of the operation. I was convinced that by carefully studying all the details of the operation, by making some alteration in the instruments used, and by avoiding the elements of failure, such as bruising and contusing either the iris or the edges of the wound, leaving fragments of the cataract still within the eye or in the wound, or rupturing the hyaloid membrane, better averages might be obtained and a more rapid recovery be brought

about than in flap extraction. I was also convinced that it might be made applicable to every form of cataract for which flap extraction can be employed, including the hard amber senile cataract. Previous to describing the various steps of the operation, it may be well to point out in what essential particulars flap extraction and the scoop method differ, and what are the principles and ideas that guide us in the various steps we take, and in the form of the instruments we employ. In flap extraction the idea is to make such an opening in the cornea as will allow the cataract readily to escape through it, either spontaneously or when gentle pressure is made on the globe. In the scoop extraction the idea is to make a wound of such a size and shape as will permit the transit of the cataract, but will not open so as to allow of its spontaneous escape, requiring for this purpose the use of an instrument to draw the cataract through the opening. This necessitates the employment of a suitable cutting instrument in order to make a corneal incision of the requisite curvilinear form; and, furthermore, a scoop of such a form as will glide readily behind the lens between its posterior surface and the hyaloid membrane, will occupy little space, will adapt its concave surface to the convex posterior surface of the lens, and on its withdrawal will so possess itself of the lens as to bring it out through the corneal opening, the edges of which are thus mechanically opened to allow of its exit. Hence it will be seen that although the escape of the cataract may be attended with more difficulty than in the flap extraction, when it is once accomplished the parts are left in a much more secure state, and the prospects of a speedy and complete healing of the wound are greater. I will now proceed to describe the various steps of the operation.

It is one of the advantages of the method of proceeding I am about to describe, that chloroform can be given with safety. I usually employ it, because a perfectly quiescent state of the eye and the absence of all spasm and involuntary squeezing together of the lids materially promotes a satisfactory performance of the different steps of the operation. Nevertheless, I have frequently operated without it, and with excellent success. In deciding this point, I am partly influenced by the wishes of the patient and partly by the estimate I form of their probable behaviour. In forming an opinion on this latter point, I am guided by various considerations, such as the strength of the orbicularis muscle, and the degree of irritability in the conjunctival membrane. This I usually test by gently manipulating the eye, touching the surface, straining open the lids, &c. It will thus be found that the sensitiveness of patients differs to a remarkable degree.

A very correct estimate may in this way be formed of the probable behaviour of the patient during the operation, and I am very much guided by it in determining whether chloroform should be given or not. As a rule, however, I think that the various steps of the operation can be more completely and safely carried out, and that the chances of accidents and unfavourable complications can be best avoided by the administration of chloroform, and, therefore, I usually advise it. The first step of the operation is to open the lids fully with a wire speculum so constructed as to fix at any definite point and regulate exactly the degree to which the eyelids are separated. The eye must be fixed, and its position determined accurately by means of a pair of tolerably strong forceps. In doing this it is not sufficient to seize the conjunctiva; this easily tears and gives way. It is better to include the subconjunctival fascia, and even the insertion of one or other of the recti muscles; this enables you to take a firm hold and to fix the eye in any position that may be most favourable for the performance of the subsequent steps of the operation. The eye being well fixed, the next stage consists in making the opening in the cornea—the size, form, and position of this are important—the incision must be slightly curved, forming a small segment of a large circle, and corresponding both in size and form to the curvature of the anterior surface of the lens or cataract. When there is reason to believe, from the appearance of the cataract, that the nucleus only is hard and the circumference is comparatively soft, the extent of the incision may be more limited, but in the senile hard amber cataract the incision must be of sufficient size

to allow it to be drawn through entire. The incision must be made in the cornea about half a line from its junction with the sclerotic, and its extreme limits must trench slightly upon the junction of the sclerotic with the cornea in those cases requiring a sufficient opening for the removal of the entire lens. If the nucleus only is to be drawn out, a smaller opening will suffice, and this may be kept within the limits of the true corneal tissue. The direction of the incision is also important. I usually make it at the upper part of the cornea because the upper lid conceals the wound and the altered form of the pupil, but where this consideration has no force it is rather easier to operate at the outer part of the eye, and the result, as regards vision, is equally good. In making the incision I employ the spear-shaped knife, curved at an angle on the flat, using rather a wider blade in those cases in which it is desirable to obtain a full-sized opening. The knife must be entered in the substance of the cornea, about half a line from its junction with the sclerotic, and must be carried across the anterior chamber up to the hilt, parallel to and just in front of the iris. This of course ensures an opening in the cornea equal to the width of the knife at its base or widest part, it also gives to the incision a slightly curvilinear form. If the incision is not of sufficient extent to allow of the ready exit of the cataract, I sometimes enlarge it with a pair of fine, blunt-pointed scissors, preserving the direction of the curved line of incision. One of the most important points connected with the operation is the apportioning the size of the corneal opening to that of the cataract that has to be drawn through it. This is more especially the case in the hard amber cataract; serious embarrassment may occur if the corneal opening is not of sufficient size to allow the cataract readily to pass through it. Of course this apportionment can only be approximative, and no decided rule can be given, but with a little care and practice a very correct estimate may be formed. If, however, an error be committed, it is better to err on the side of having the opening too large than too small, as it is very undesirable to exert any force in drawing the cataract through the corneal opening. The next step in the operation is the removal of a portion of iris; for this purpose I employ a small pair of forceps bent at an angle, and with fine Asselini points. I usually remove only a small portion of iris; it seems to me to answer every purpose, and leaves less subsequent deformity of the pupil. A slight inconvenience that sometimes attends the cutting away a portion of the iris is the effusion of blood into the anterior chambers, obscuring the parts and impeding the subsequent stages of the operation. If, however, it is gently pressed out with the spoon of the curette before it has time to coagulate, it soon ceases. The next step in the operation consists in opening the capsule; this is done in the same way and with the same instrument as in an ordinary flap extraction, it is however desirable that it should be freely torn through, with the twofold object of preventing, if possible, the formation of a secondary or membraneous cataract, and also to favour the advance of the cataract out of its vitreous bed. In doing this care must be taken not to press upon the anterior surface of the cataract so as to risk the rupture of the hyaloid membrane. The next and perhaps the most important stage is the removal of the cataract; for this purpose I employ a spoon or scoop, which I have already described and figured; it is so constructed as to glide readily behind the posterior surface of the cataract; it is thin, flat, and concave, so as to adapt itself accurately to the posterior convex surface of the lens, and having passed completely behind it there is a small receding edge which holds the cataract, and as the scoop is gradually withdrawn, brings the cataract out with it, or such nuclear portion of it as is of sufficient density to escape entire. This part of the proceeding requires considerable care and some dexterity. In the introduction of the spoon through the wound, the back must be pressed gently against the posterior part of the corneal slit, so as to depress it and bring it on a level with the edge of the cataract, the anterior part of the spoon must then be inclined backwards so as to glide along its posterior surface, making its way between that and the hyaloid membrane of the vitreous humour. This must be done slowly and carefully, watching the anterior surface to see if there is any dis-

placement, or if the instrument is passing safely and easily along. When the scoop has reached the further edge of the cataract it may be gradually withdrawn, the receding border will fix itself in the substance of the lens and bring it out through the corneal opening, or such portion of it as possesses sufficient consistency to pass through entire. Just as it is escaping peculiar care is required, as this is the moment when the hyaloid membrane is most liable to give way and some of the vitreous humour to escape. We must also avoid using any undue force or pressure, so that the surface of the iris and the margins of the wound may not be bruised or injured in any way. If (as may occur in those cases where the cataract is of the senile hard amber character) we find that it has escaped entire, the operation may be said to be completed; but if, as is more frequently the case, the nucleus only is drawn out, there is a residue of lenticular matter more or less considerable, varying in its consistence in different cases, and occupying not merely the pupil but the space between the iris and the hyaloid membrane, where portions are very apt to remain concealed. It is very desirable to remove these as far as possible from the eye, and for this purpose I employ, usually, a small spoon. When the pupil is cleared in this way, gentle pressure upon the front of the eye with the upper eyelid will often succeed in bringing fresh particles forward which may be removed in a similar way. It is important not to allow the spoon to touch the iris itself. I have not unfrequently observed that after every care has been taken to spoon away every particle of opaque lenticular matter, and the pupil looks perfectly clear and black, if a few hours are allowed to elapse and the aqueous humour re-forms, the pupil will again be found filled with particles of the cataract. As this may become a source of irritation, it may well admit of question whether it may not be desirable again to open the wound and remove these fragments. There are certain practical inconveniences attending this proceeding, but I can well conceive cases in which I should be disposed to adopt it. Great care is required not to leave any particle between the lips of the wound or so near the margin as to allow of its being easily washed into it, also to remove any little clot of blood or fold of conjunction that may separate the edges, and in this way interfere with primary union. I am inclined to think that many of the cases of suppuration of the wound are due to this cause.

The after-treatment I employ is usually of a very simple character. For a few hours, and as long as it feels agreeable to the patient, I apply cotton-wool soaked in cold water to the surface of the eyelid, and renewed as often as it becomes warm and dry. I also drop in a strong solution of atropine three times a day, so as to keep the pupil well dilated. If pain comes on severely, and if the lid begins to swell and redden, I apply leeches (six or eight) to the temple. It is just in the early stage of inflammation that leeches are usually of service. I also at this stage, if there be pain, examine the condition of the eye. If I find that fragments have come forward into the pupil and anterior chamber, or are pressing upon the iris, I sometimes endeavour to remove them, but unless this is done within 48 hours it is not attended with a satisfactory result. If purulent infiltration has commenced in the edges of the wound, extending between the layers of the cornea, and even into the anterior chamber, which is usually indicated by the state of the lids, and may be verified by an examination of the eye itself, I generally find warm applications more suitable than cold. It is in such cases that carefully applied support to the eyelids, by means of cotton-wool (a method strongly recommended by Professor von Graefe), is especially indicated. A liberal diet is also required in such cases. It sometimes happens that the suppurative process can be arrested at this stage, and that some useful sight may be ultimately obtained, but much more frequently the purulent infiltration extends, and the eye is lost. With the exception of acute suppurative inflammation of the entire globe, it is the most serious and fatal sequel of this operation, and fortunately it is very exceptional and rare. It is, of course, difficult to trace accurately, or with any degree of certainty, the cause or causes of this form of traumatic inflammation. They are probably both local and constitutional, sometimes the former, sometimes the

latter preponderating. If the edges of the wound are bruised, contused, or stretched during the drawing out of the cataract, or if any foreign body, such as blood or lenticular matter, remain in the corneal aperture, purulent infiltration is favoured. On the other hand, it sometimes occurs where we have good reason to believe that none of these causes have been in operation, and we are constrained to fall back upon some constitutional proneness to this form of inflammation. In some cases, doubtless, both local and constitutional conditions have combined to bring about this untoward result. We must at the same time bear in mind that it not unfrequently happens where both eyes have been operated on at the same time, one has progressed most favourably, and the other has suffered from this form of purulent inflammation. In such a case we seem to be logically compelled to seek for the cause in some local conditions. A much more frequent result of this operation is the occurrence of iritis. This generally begins to show itself about the third day. It may be attended with severe pain, or it may be very insidious and painless. The pain is usually of a neuralgic character, radiating to the contiguous branches of the fifth pair. It may be attended with increased aqueous secretion, closure of the pupil, and abnormal hardness of the globe. Where this is the case, puncturing the anterior chamber often gives considerable relief, and this little operation may sometimes be repeated with advantage at intervals. Or again, the iris may be swollen and vascular, and the anterior chamber small. A third condition that not infrequently occurs is a peculiar bulging forward of the iris, with adhesion of its pupillary margin to the capsule, together with considerable recession of the adherent portion. This condition is also capable of relief by puncturing the cornea and iris, and restoring the communication between the anterior and posterior chambers of the eye. If this do not prove permanent, an irideotomy must be had recourse to. It sometimes happens that traumatic iritis is attended with severe pain of a neuralgic character, extending to the brow and the face, and taking the course of the branches of the fifth pair of nerves, sometimes occurring at irregular intervals, but not unfrequently assuming the periodic type. When this occurs it is a very painful and intractable complication. Locally soothing and warm applications, such as opiate fomentations, solutions of atropine, or belladonna, usually give the most relief. Blisters are sometimes useful, but leeches seldom benefit.

Constitutionally, the indication would seem to be to sustain the powers of the system, and at the same time to quiet the nerves. A generous diet and a certain amount of stimulus must be given; tonics, and particularly quinine, would seem to be indicated on account of the periodicity of the attacks. In many of these cases, however, I have been rather disappointed, and have failed to obtain any specific or immediate effect, even from the exhibition of large doses of quinine, the attacks being complicated, and kept up by the presence of local inflammation, the specific influence of quinine is not felt. I place my chief dependence in these cases upon the use of opiates, and by far the best method is the endermic. I usually am in the habit of injecting, under the skin of the brow or temple, a solution containing from a sixth to a third of a grain of the acetate of morphia. The effect of this is usually very speedy and very satisfactory. The pain subsides, a calm condition is induced, and a quiet sleep in most cases follows.

The influence usually lasts from eight to ten hours, and when it passes off the symptoms return, but the intensity is lessened. It may be repeated with advantage every twenty-four hours, until by degrees the pain and inflammation pass off. When traumatic iritis is very persistent and severe, it inflicts more or less permanent damage upon the eye. In some cases it extends to the ciliary region and the choroid, produces effusion between the choroid and retina, softening of the globe, and loss of sight. In less severe cases the pupil is closed, the iris thickened and altered in its structure, and its colour, and the anterior chamber diminished in size. Even in the slighter cases there is always partial closure of the pupil, and formation of a false membrane, filling up the pupillary space, and obscuring to a considerable degree the sight. In the course of this paper I have endeavoured to show the advantages of the

scoop extraction over the former plan, by means of a corneal flap. On the other hand, I feel that I ought in all fairness and candour to state, as the result of my experience, that iritis is more frequent after scoop extraction than when the old method is used; also that the occurrence of false membrane with adhesions is by no means uncommon, which vary exceedingly in the degree both of density and opacity, and also in other qualities. These secondary membranes often cause considerable disappointment to the patient and embarrassment to the surgeon, and many cases have come under my observation in which, either through neglect or mismanagement, permanent injury to the sight has been the result. As may be readily conceived, we have every shade of variety in these membranes, occupying the pupillary space. It not unfrequently happens, even in cases that have in all respects made rapid and satisfactory progress, and in which the pupil looks black and clear, that when the sight is tested with suitable convex glasses, it does not reach the normal standard. Under these circumstances we may suspect the presence of a delicate false membrane. It is often quite impossible to detect this with the naked eye, or with ordinary light. Sometimes it can be seen with the ophthalmoscope, using only a moderate degree of light and a weak lens; but the best, indeed the only sure way of detecting these thin semi-transparent membranes is by oblique illumination. By means of a three-inch lens the light of a lamp placed about three feet from the eye may be brought into a focus upon the pupil, and if this illuminated spot is examined through another lens the membrane can be distinctly seen like a fine piece of cobweb, with fine spots upon it. The tendency of this membrane is to become tougher and thicker. It can only be got rid of by means of a needle operation. It is important to determine at what period after the first the second should be undertaken. If, on the one hand, it is performed before all signs of irritation have passed off, it is very apt to light up fresh inflammation; if, on the other, it is delayed too long it is apt to become floating and elastic, and very difficult to disperse or tear through. I think it is better to wait until the eye becomes perfectly quiet, usually from about four to six weeks after the first operation. The pupil must be widely dilated with atropine, the lids being fixed open by means of the wire speculum. A fine needle may be passed through the cornea into the centre of the opaque membrane, and depressed so as to tear it through. If it gives way readily and freely, as is usual, then all that is needful is accomplished; if, on the other hand, it is elastic and moves with the needle, then a second needle must be introduced from the other side, as recommended long ago by Mr. Bowman, and by means of the two acting in two opposite directions, the membrane, however tough, can generally be torn.—*Ophthalmic Hospital Reports, Vol.* 4, *Part* 4, *p.* 315.

73.—ON THE EXTRACTION OF CATARACT BY A TRACTION INSTRUMENT, WITH IRIDECTOMY; WITH REMARKS ON CAPSULAR OBSTRUCTIONS, AND THEIR TREATMENT.

By William Bowman, Esq., F. R. S.

[Nothing can rival the excellence of the ordinary extraction operation for cataract, when all the dangers attending the healing process have been happily surmounted. The highest results cannot however be reckoned on with anything like certainty in any case, and a considerable proportion of cases turn out unfortunately, chiefly owing to the largeness of the flap. About five years ago, Von Graefe proposed to extract certain cataracts through a smaller opening, by exeising the iris opposite the wound, and making use of a spoon for the withdrawal of the lens.]

It is evident that in all these earlier examples an *iridectomy* had formed no essential part of the proceeding. So far as I am aware, we owe to Von Graefe the first systematic combination of iridectomy with the use of a traction instrument, in extracting cataracts of a certain solidity through an opening, small in comparison with the old flap. In flap-extraction the large

wound opens so readily to give exit to the lens, that this often bursts forth at the moment when the incision is being completed; and usually, unless the pupil proves rigid, very slight external pressure on the eye suffices to expel it. But with the smaller incision no amount of external compression can force out the lens; the iris and vitreous humour may escape, but the entire lens cannot do so. Hence it is a necessity, if the incision is to be of limited extent, to help out the lens on another principle, that, viz., of introducing a *traction-instrument* of some description within the eye, to take hold of the lens and draw it out, or at least to add this principle of traction tc that of pressure applied to the exterior of the globe.

It would appear, that unless the incision is to be made into the posterior chamber, which on many accounts is inadmissible, such an instrument, however contrived, cannot be used with advantage, without a previous removal of so much of the iris as lies across the track of the instrument. For otherwise either only the central pupillary area is available for the traction-instrument to work in, and this cannot, with safety to the iris, get thence into its requisite position behind the nucleus, so as to take hold of it, or else the instrument must pierce the marginal part of the iris, corresponding to the external incision, entering the lens out of sight of the operator, and almost unavoidably bruising the overlying iris, or dragging it out with the lens, so as to oblige its excision, if iritis is to be averted.

It might seem at first sight a bold thing to make the removal of a portion of the iris a condition of cataract extraction, thereby permanently altering and enlarging the pupil, when the flap-extraction admits of so easy a removal of the lens through the natural pupil, leaving the iris and pupil in their integrity; and it is not surprising that this should have seemed to some minds a fatal objection from the outset to the new method.

But it had already become abundantly obvious to those who had practised iridectomy on a large scale, especially for glaucoma, that this enlargement of the pupil, especially if made upwards, need detract in no important degree from subsequent acuteness of vision; and, as a matter of fact, it is certain that such altered pupils admit of the smallest test-types being read.

[Mr. Bowman does not wish it to be supposed that he is for altogether abandoning the old operation, for there are occasions when a number of circumstances will combine to give it the preference over every other. The author then proceeds to give the details of the traction method, the object of which operation is to get away the whole lens, through a small opening, with the necessary sacrifice of only that portion of the iris which lies in the track of the instrument.]

Chloroform should generally be given, and if given should usually be given fully, so that the eye remains quiescent throughout the operation. When it seems desirable to administer as small a dose as possible, I often make the preliminary incision and the iridectomy under its partial influence, reserving its full effect for the most critical period—the introduction of the instruments which have to deal with the lens—a period lasting perhaps only a few seconds.

Operation.—If the pupil is small and rigid, atropine may as well be applied previously, otherwise not. The patient lies on his back with the face upwards, and his feet towards a window. The operator stands or sits beyond his head, which is slightly inclined towards the light, uses the wire stop-speculum for the lids, and fixes the eye with forceps, which take hold of the tendon of the lower rectus precisely in the middle, close to its insertion. He then rolls the eye downwards with care, not at first either to compress or to pull upon (I do not mean to rotate) the globe, for to pull upon it is really to compress it. The holding forceps should be slightly toothed, so as to seize the tendon readily. To seize the tendon, or at least the subconjunctival tissue, and not the conjunctiva only, is to obtain a perfect hold on the globe; the conjunctiva alone may tear away, or by sliding on the globe may render the forceps no longer the guide they should be to the point of incision, by being precisely opposite to it; for the eye, in being held downwards, is usually somewhat rotated on its anterior-posterior axis. During the act

of iridectomy, unless the eye is quiet and lies well, I generally give the fixing forceps to an assistant, taking them again into my own left hand while the traction instrument is being used with the right hand.

As to the incision. *Position.*—The iridectomy should be usually made directly upwards, both because it will not be visible afterwards, and because, being covered by the lid, it will the less detract from the optical perfection of the organ. The lower or central part of the resulting pupil will be that used in vision. It is perhaps a little less easy to execute the steps of the operation when the upward direction is chosen, but if the eye is rendered passive by chloroform, and the globe rotated sufficiently downwards without being compressed, no real difficulty need occur.

Shape.—The lancet-shaped knife (which should be slightly bent on the flat, and of which the cutting edges should meet at an angle of about 50° or 55°) should enter the extreme margin of the anterior chamber, and be pushed on with its flat parallel to the iris, until its point reaches the opposite rim. It is then gradually withdrawn, and as the aqueous flows slowly away it may be made to enlarge the wound at one or both angles, if requisite. In withdrawing it the point may incline to one or other side of the chamber, so that the lens should not incur risk from the point traversing the pupillary space, in case of any sudden, unexpected movement of the eye. This is a good precaution in all cases of iridectomy, but, of course, all the more important when the lens is transparent, and not to be involved in the operation.

The wound thus made is curvilinear, and follows the rim of the chamber, passing through the corneo-sclerotic junction. It cuts this tissue not vertically, but in a plane about parallel to the iris, and its middle part and ends are in the same plane. As it need hardly in any case occupy more than a fourth of the circumference of the cornea, the effect is that if the globe is compressed by the lids, or if the vessels of the eye become turgid under the chloroform or afterwards, so as to occasion internal distension, the wound hardly gapes, it rather closes; whereas if the eye is slackened, the hinder or sclerotic lip is easily pushed back, and the anterior or corneal lip forwards, by the traction-instrument inserted, so as to allow blood to escape from the chamber, and to give ample room for even a bulky lens to emerge under the guidance of the tractor. The incision thus made is near to the equator of the lens, which is thus more easily dealt with by the tractor, and fragments of it do not easily elude the instrument by slipping up behind the posterior lip of the incision.

The incision is faulty in direction if its ends leave the line of the corneo-sclerotic junction and curve into the cornea. It then describes an arc of a smaller circle. The flap formed is less roomy for the exit of the lens, and yet it is more disposed to gape and lie unevenly; moreover, the iris is less perfectly excised at the angles. It is more exposed to be bruised there, and lateral fragments of the lens to be entangled and left in, occasioning irritation afterwards. This faulty form of flap is familiar to operators by the old method, when the incision has not been a happy one. The *direction* of penetration should be the same at all parts of the incision, viz., from first to last in a plane nearly parallel to the iris, and just in front of it.

Extent.—A judicious operator will vary the extent of his incision by his estimate of the probable size of the firm cataractous substance which has to pass through it, always taking care that the opening is *large enough*. It is far better to have it too large than too small, and it is particularly desirable that it should be ample in such old cataracts as are supposed to have their margin easily detachable in small segments. For one of the disadvantages of this traction method is the liability to have the sides of the lens separated and left behind near the angles of the wound, giving rise to subsequent irritation; a too narrow path of exit disposes to this, and in such cases an incision of nearly one-third of the corneal circumference, especially when the cornea is small, is desirable. In old cataracts that have had formerly a softened surface, the remains of this seem often to accumulate at the equator, where they undergo fatty degeneration, and readily break off in chippy fragments.

Where the whole lens is firm and of nearly equal consistence up to the margin, as in some old cataracts, and also in those of slow progress in middle-aged persons, which have commenced with striæ near the surface, the opening should be proportioned to the size of the cataract, so as to give it room enough to emerge; but the largest-sized opening is nevertheless not required, as the margin of the lens does not break off, and the whole admits of being drawn out entire and with safety through an opening comparatively small.

The cases for which the smallest incision will suffice are those where we may be pretty sure that the nucleus, if hard, is small, and the surface of the cataract diffluent, or pulpy, or semi-fluid. The opening is then made to suit the size of the nucleus, it being certain that the surface matter, though abundant, will be effectually got out easily through the same channel. But it is important here to have regard to the exact consistence of this surface matter. For though very soft it may not be so loose and diffluent as to flow easily from behind the iris into the pupillary space, and so out at the wound; but, on the contrary, it may be glutinous, adhesive, and semi-transparent, and therefore much more apt to remain within the chamber if the harder nucleus has to emerge through a narrow wound, through which it can only come stripped of its surface matter; whereas if the opening be ample, the surface matter or the greater part of it may be made to emerge with the nucleus, without being much detached from it, if care be taken in using the scoop.

When it is wished to make an opening of the largest size, this may be best effected by enlarging the wound at one or both angles on withdrawing the knife, and to do so has the additional advantage of making the inner aspect of the section as wide as, or wider than the outer, which a lance-shaped instrument, if allowed to cut only in entering, could not accomplish; and this shape of the wound, irrespective of its mere size, is probably favourable to the escape of lens-fragments. Even if the knife has to be re-introduced, I prefer it to the scissors, which are apt to cut too much within the corneal margin, and not in the same plane as the knife has done; they cannot be made to cut conveniently at either angle indifferently as the knife can do, without the operator changing his own position; and I also think them more likely to bruise the iris.

As to the Iridectomy.—In dealing with the iris in this operation, the great object is to secure the eye against inflammation of that part of the iris which remains. If the nucleus of the lens is small and the surface of the cataract diffluent, so that the extraction by a small opening is easy, and the risk but slight of small glutinous or hard fragments remaining behind, the iridectomy need not be large. In some such cases I have so managed as that the pupil has remained afterwards nearly circular, the pupillary margin having been rather divided than excised, the iridectomy involving chiefly the upper part of the membrane. No doubt the less excised the better in an optical sense. But a far more essential thing is to secure the eye against inflammation of that part of the iris which remains; no portion of this should be on any account contused or rubbed against either the lens, the cornea, or the instrument. It is much better therefore to excise widely than by removing a smaller piece to hazard the bruising the edges of the remainder during the movements of extraction. It is better to err, if at all, on the side of an ample excision. The margins of the cut part are then also not so liable to entangle fragments so easily, or to become irritated by their presence if retained.

The iris is seized by the small curved forceps, drawn out, and snipped off by scissors. I used to leave its ciliary border, but I now much prefer to excise its whole breadth, as being the safer method. What remains is less on the stretch, yields more, and is less apt to become inflamed; moreover, with care there is then no greater risk of the escape of vitreous. The mode of seizing with the forceps influences the amount excised. If a small excision is desired, the blades should open very slightly and seize that part that lies opposite the centre of the corneo-sclerotic incision, and which is midway between the ciliary and pupillary margins, and it should not be drawn out much before the scissors are applied. If a large excision is intended, the blades of

the forceps should open more widely, and the iris may be dragged out further, and be thus torn from the ciliary attachment to the required extent before being cut off.

It may be cut off by a single movement, or divided first in the middle and then at each angle of the wound separately. A method, which I think a good one, and generally follow now, is to seize it and tear it a little from its ciliary attachment near the right-hand angle of the incision, and cut it across there on the right of the point held by the forceps; then tear it away as far as the opposite angle of the wound, and cut it across there also. By pulling it out at the angle of the wound, before cutting it off, the ciliary separation and the iridectomy may in this way be easily made more wide than the incision itself, which in some cases is advantageous, and is always safe, since the cut margins of the iris recede well away from the track of the instruments dealing with the lens, and cannot lie in the angles of the wound afterwards. I think it likely that when iritis follows this operation it is often an extension of irritation either from the ciliary edge of the iris left remaining at the site of the iridectomy, or from its cut margins bruised by the tractor, or entangled in the angles of the wound.

If blood flows into the chamber so as to disguise the iris and lens, either before or after the iridectomy, I prefer waiting till it ceases to flow, and to remove it, before proceeding with the next step. It should be removed as it flows, and before it coagulates, by inserting the point of the curette, if necessary, between the lips of the incision, while the forceps holding the lower rectus tendon slightly pull or compress the eyeball.

It is not often indeed that blood does flow to an extent to require this, but if it do, and is left in, it obscures the view, and thus not only renders the succeeding steps less delicately exact, but makes it less easy to spare the remaining iris from all injury, and to withdraw loose fragments of lens. Hence I insist upon this point of practice.

Opening the Capsule.—The next step is to open the capsule. This is done with one of the ordinary flat-ended prickers used in flap-extraction. I have only to intimate the extent of the opening and the precautions to be taken. The object is so to open the capsule as to destroy or effectually tear through *all* that part which lies in front of the attachment of the suspensory ligament, but to save the suspensory ligament, and all the capsule behind it, as a safeguard to the vitreous humour. Opening the capsule thus freely facilitates the escape of the marginal lens substance, and also lessens the chance of subsequent so-called capsular opacities in the pupil. Such opacities often depend on portions of the anterior capsule rolling up and enclosing its lining of cells or even portions of superficial lens-substance, and if the membrane has not been (so to say) comminuted, but only split open in one or two directions, these opaque portions are apt to form bands standing across the pupillary area, the too limited orifice made in the capsule at the same time contracting as the whole capsule falls somewhat together centrally towards the pupil.

If the opening has been made by comminution of the capsule, but to a too limited extent, corresponding perhaps only to the middle third of its area, or to the pupillary area at the time of operation, and has not been carried behind the iris as far as the vicinity of the suspensory ligament, the margin of the orifice is very apt thus to rise afterwards across the pupil, and to require to be dealt with on a subsequent occasion.

The pricker should therefore be at first passed flat behind the iris to the farthest part intended to be opened, then its point should be turned into the lens there and brought up with slight movements, first on one side, then on the other, comminuting the capsule in its course within the line of attachment of the suspensory ligament, under cover of the iris. It may then be made to tear the central part, and finally that part nearest the external incision, where the traction-instrument is about to be inserted.

Here it may be moved a little more freely, so as to break open a way for this instrument at the upper edge of the nucleus, especially if the nucleus is large, or its surface adherent to the capsule. It is essential that the pricker

should be used with a light touch, that the iris should not be bruised, and that the body of the lens should not be pushed backwards nor sideways by it, lest the vitreous should be burst into. The operator should bear in mind the delicacy of the sustaining tissue, and that his object is to lay open the capsule properly, without any displacement of the lens. To ensure this in the greatest degree, I always take care that the chloroform at this critical moment has made the eye quite motionless, and I hold the eye myself with the forceps both at this period of the operation and during the use of the tractor.

I attach considerable importance to another point of practice, viz., the loosening of the body of the lens from its capsule before introducing the tractor, whenever, either from the immaturity of the cataract, or the large size of the nucleus, and absence of superficial soft matter, unusual difficulty in the introduction of that instrument might be expected. In tearing open the capsule, the condition of the lens in this respect is probably ascertained. This loosening I effect either by the common pricker or by the fine needle already referred to (having the point bent at a right angle), which has long been in use at Moorfields, for pulling out the lens in ordinary flap-extraction, when it refuses to escape in the ordinary way, or has dropped back into the vitreous humour. The point of this is thrust back into the lens-substance near the margin (and particularly without pressing the lens backwards towards the vitreous, which might force this humour out through a rupture of the suspensory ligament) so as to get a hold of it without displacing it, and then the body of the lens is slightly rotated on its antero-posterior axis to and fro within the capsule until it is felt to be loose. In immature cataracts the loosening is often attended with a sudden falling forwards, which at once explains itself to the operator.

I have come to adopt this method in certain cases, from having witnessed the failures not infrequently attributable to the difficulty in passing the tractor behind the nucleus, when the body and capsule adhered. Every operator knows how much the risk of the ordinary flap-extraction is augmented by this adhesion of the body to the capsule in immature cataracts. The lens escapes with difficulty—more pressure on the globe is required to expel it—and portions of it are very liable to be left adherent to the capsule behind the iris, where they become a fruitful source of irritation afterwards. The same risk is incurred, but to a much less extent, where iridectomy is practised. As circumstances often are urgent against delaying operation till the cataract is more complete, I think in such cases, when the traction operation is chosen, this little proceeding is a useful addition to its safeguards; and it might be applied also, where called for in flap-extraction.

While opening the capsule the operator at once learns the consistence of the cataract with which he has to do, and knows better how to deal with it in the subsequent steps. If soft matter is abundant around the nucleus, and sufficiently diffluent, it is well to allow some of it to escape at once by the wound. Slight pressure on the globe facilitates this, and thereby more room is afforded for the traction-instrument within the eye.

The *use of the traction instrument* has next to be considered. The *course* it has to take—at first backwards and then downwards, so as to embrace the nucleus—has to be estimated with exact reference to the shape, size, and position of the lens, and it is by no means difficult for a young operator to commit an error in this respect. It must pass between the nucleus and capsule without pushing the nucleus from its place, and without breaking into the vitreous humour, or bruising the iris, and if the nucleus be not already loose from the capsule, one or other of these accidents may happen very easily. The more blunt or obtuse the end of the traction-instrument, the more likely is it to displace the lens in the attempt to get beyond the nucleus, and it is particularly apt to do so if it be pushed on too rapidly, or if it do not dip sufficiently behind it.

[Mr. Bowman here describes his spoon-like instrument, which differs a little from that of Mr. Critchett.]

The body of the spoon is very nearly flat from side to side, a little concave forwards from end to end, and the incurvation of the extreme end

is in fact a continuation of this concavity, though inclined to it at an obtuse angle. Where the end joins the sides of the spoon this incurvation gradually ceases, and the sides, except towards the end, have no edge above the general level. The whole is as thin as possible consistent with due rigidity, and the breadth about half or a third that of the lens. The stem is slightly curved, to adapt it to the position of the brow, across which it has to work in the ordinary iridectomy upwards.

I have found a spoon thus constructed to be very suitable in the great majority of cases, to be easy of introduction and manipulation, and also to take excellent hold of the nucleus when the handle is properly depressed at the right moment. It is also useful in withdrawing loose fragments after the main bulk has been got out.

Another modification of shape, however, seems even preferable, where, from the entire absence of soft surface matter, there is the least room for the insinuation of an instrument between the lens and its capsule, and where therefore it should occupy the least possible space. This form is nearly flat from side to side, and but slightly concave from end to end.

It is, of course, better to extract all fragments if it can be done with safety, and I may here mention my *suction-syringe*, with a larger orifice than usual, as of great use in this respect. Fragments which refuse to be brought out by the curette or spoon will often be completely manageable with the syringe. The sucking orifice admits of being turned in any direction, so as to seize such fragments without interfering with the iris at all.

[The after treatment does not demand any special remarks. It is generally more simple and attended with less anxiety to all concerned than that following ordinary extraction.]

On the whole, the experience I have acquired induces me to draw the following conclusions:—

1. The operation described may be applied to all cataracts of harder consistence than those which admit of being withdrawn through a puncture of the cornea.

2. The iridectomy, which is a necessary part of it, is no disfigurement, nor does it in any important degree affect the optical result.

3. This operation is superior to flap-extraction by being applicable under chloroform, by its diminished risks during convalescence, and by its greater average success, and by being now and then admissible where flap-extraction would be attended by so little prospect of advantage as to be hardly worth performing.

4. While therefore flap-extraction may still be preferable under certain circumstances and while it may be open to the future to render its success more secure under various conditions, a general adoption of the traction method would be likely, at present, to add considerably to the percentage of cases of cataract in adults recovering sight by operation.—*Ophthalmic Hospital Reports, Vol.* 4, *Part* 4, 1865, *p.* 332.

74.—ELECTRO-THERAPEUTICAL AND PHYSIOLOGICAL RESEARCHES ON PARALYSIS OF THE OCULAR MUSCLES.

By Dr. Moritz Benedikt, of Vienna.

When, at the suggestion of Professor Arlt, I first undertook the treatment of paralysis of the ocular muscles by electricity, I soon convinced myself that the production of contractions of the paralyzed muscles was not necessary to a cure, and was not often possible. I soon convinced myself, also, *that the cure was dependent, generally speaking, upon reflex excitation through the fifth pair of nerves, and not upon direct excitation of the nerves supplying the muscles themselves.* In most cases, *a curative action was only produced when the excitation was relatively weak*, and when no trace of muscular contraction was produced by the electricity. If, for example, during a sitting, an increase of mobility became apparent, and the operator then

applied a strong current, the improvement forthwith diminished. *The proper measure for the strength of the current is always furnished by the sensitiveness of the fifth pair.* For example, in a patient suffering from complete paralysis of the third nerve, improvement first appeared when the battery was reduced to three of Daniell's elements. The fifth on this side was very sensitive. In patients with abnormally insensitive trifacial, it is often necessary to use fifteen elements before any benefit is obtained.

The intensity of the current must be such as to produce slight sensation in the parts excited.

These data prove that we have not to deal with a physical current, acting upon the nerves of the ocular muscles, in which case these would be independent of the degree of irritability of the fifth nerve; but with a physiological reflex excitation, having its course manifestly through the ganglia of the spinal cord, the connection of which with the ganglion of the fifth is partly anatomically proved, and partly rendered probable by analogy.

According to my observations, the results of electrical excitation of subjective visual and auditory sensations are wholly analogous to the above. Their occurrence and intensity always bear a direct relation to the sensitiveness of the trifacial.

A third important principle in the electrical treatment is that the *excitation should only continue a very short time* (about half a minute) at each sitting.

The operator may easily convince himself that, when improvement becomes manifest under the influence of very feeble and soon discontinued excitation, this improvement will be endangered if the excitation be continued. I therefore usually excite for a few seconds, then test for improvement, and then excite again, until I observe that the improvement ceases to make progress, when I abstain. *In the greater number of cases the improvement takes place instantaneously.* When no immediate improvement occurs, a longer continuance and increased strength of excitation are not indicated. In the curable cases improvement occurs, after some time, by the repetition of excitations becoming weaker and shorter at each sitting; while by longer and more powerful excitation the original paralysis may easily be aggravated. In the rare cases in which, after sufficient perseverance, no improvement is gained in the manner described, it is useless to practise the dangerous longer and stronger excitation. I have myself hitherto always used the galvanic current. That the operators who use Faradization have, in general, hitherto obtained no very gratifying results, is probably due to their having attempted to act *directly* upon the ocular muscles, as well as to the undue continuance and excessive power of their applications. Whether the principles above laid down would lead to the successful use of Faradization also, can only be ascertained by a parallel series of experiments. *The improvement in most cases occurs instantaneously, and is chiefly shown by an increased mobility of the eye, and by a diminution of the field in which double images arise.* At the beginning of my researches I believed the above to be an invariable rule in curable cases; and I discontinued the treatment whenever no improvement was obtained during the first eight days. I was taught better by the case of a medical man from Cracow. He suffered from paralysis of the third nerve following a severe cold; and under ordinary treatment making but little progress towards cure—Professor Arlt sent him to me. Although some part of the contractility of the paralyzed muscles was then restored, the patient saw double in the greater part of his field of vision; and during the first fourteen days no improvement was produced. *All at once the region in which double images were visible became greatly narrowed, although the power of moving the eye by means of the affected muscles underwent no increase.* Subsequently, however, this power improved, and the patient made a complete recovery. When the paralysis has been unaffected by fourteen days of treatment, I have, as yet, seen no benefit from its longer continuance.

In order to assign a marked effect to any therapeutic agent, it is necessary first to inquire whether it will, with any certainty, effect a cure in cases that have resisted other methods of treatment; and also whether cases, that have not been cured or improved by it, may be cured by other means.

In respect of vesicants, strychnia, secale cornutum, vapour baths, and section of the antagonistic muscles, I possess no little experience, since a large part of my cases were brought to me after these more common methods had proved of no avail.

I have at present succeeded in curing, or in greatly improving, many cases in which the above-mentioned remedies had been applied without result; and I have not hitherto met with a single case, that resisted electricity, in which these remedies have afterwards effected a cure. With regard to the treatment by iodide of potassium, I have not sufficient material to institute a similar comparison.

The next question is, whether certain branches of the trifacial nerve bear a definite relation to the motor fibres; that is, whether the excitation of only some sensitive twigs will be followed by the restoration of mobility. With regard to this, I have tried experiments in a spirit of pure empiricism; and have based upon them the methods following:—Paralysis of the abducens is best treated by placing the copper pole upon the forehead, and moving the zinc pole over the neighbourhood of the cheek-bone. In mydriasis, the copper pole should be placed on the closed eyelid, and the zinc pole applied as before. In ptosis, the copper pole may be either upon the forehead, or may be applied, by means of a short catheter-like rheophore, to the mucous membrane of the cheek, while the zinc pole is drawn over the lid. For all the other branches of the third nerve, the copper pole is applied as above. Then, in order to act upon the rectus internus or obliquus inferior, the zinc pole must be drawn over the skin of the side of the nose, near the inner angle of the eye, and, in order to act upon the rectus inferior, over the lower margin of the orbit.

In paralysis of the superior oblique, I have succeeded best by placing the copper pole on the forehead, and by drawing the zinc pole on the inside of the nose, near the internal angle of the eye.—*Ophthalmic Review, July* 1865, *p.* 143.

SYPHILITIC DISEASES.

75.—ON SYPHILIZATION.

By Professor Boeck, Christiania.

[M. Ricord says "Syphilis is certainly one of the diseases over which medical art has the most power." Professor Boeck's experience is that the mercurial treatment gives the worst and most melancholy results possible. He says:]

"Syphilis is certainly one of the diseases over which medical art (mercury) has the *least* power." But we do not want opinions; we want facts: and I have therefore, in my work "Recherches sur la Syphilis" (Christiania, 1862), collected all the cases of syphilis treated in the hospitals of Christiania from the year 1826 to 1856; and I shall here repeat the principal results, and add the information that has been communicated to me since the appearance of the said work.

Patients to the number of 3561 have been treated for constitutional syphilis, and of those 3087 have been treated with mercury. Of these 3087, 978 have had relapses, or nearly 32 per cent., many of these having had more than one relapse; the total number being 1532, and the average time of treatment for each patient having been about 125 days.

However, it is not the time required for the treatment, nor the number of relapses, that forms the principal question when we talk about mercury being advantageous or injurious as a means of cure for syphilis: it is the general health of the patient after the treatment. I have done all I could to ascertain the patient's fate in later years, but this, as may be supposed, has been no easy task, and out of the 3087 I have been able to trace only 655. The principal diseases from which these have suffered are as follows: Paralysis, of which 18 individuals have died, and from which 32 are still suffering; 9

have been attacked by paralysis after treatment, but have been cured: this makes a total number of 59 attacked by paralysis, and one is at this moment suffering from ataxie locomotrice progressive. 8 have died from, and 2 are being treated for, paralysis generalis. 42 have died from, and 17 are being treated for, phthisis. 9 have died from, and 1 is being treated for, morbus Brightii. 4 have died from, and 6 are being treated for, mental alienation; 3 have been cured. 52 persons have died from different chronic diseases; the average length of time that has elapsed before death after their treatment for syphilis has been about twelve years and a half. 56 have died without my having been able to ascertain from what disease; the average length of time that has elapsed before death after their treatment was fifteen years and a half. 186 persons are still in apparent good health. With respect to further details I must refer the reader to the above-mentioned work.

I do not mean to say that all these cases of paralysis, paralysis generalis, &c., are the inevitable consequences of the preceding syphilis; but I affirm that among 655 individuals who have not had syphilis you would not find by a long way such a percentage of these diseases. If any one still believes that mercury deserves any credit, I would just advise him to go into an hospital for syphilitic patients, where old cases are treated as well as recent ones, and he would find plenty of fallen noses, open palates, caries in several of the superficial bones, and many of these persons also in a cachectic state.

It may, perhaps, be said that the treatment in the hospitals of Christiania may have been bad, that mercury may have been given in too great doses, &c. I have treated but very few of the aforesaid patients myself, but the treatment has been going on in my time and I can attest that it has been conducted with the greatest care and by skilful men. The fact is, the same results would be found everywhere if they could only be traced. But in most cases a physician in a large town cannot control his patient after the treatment; he does not even know whether he has a relapse or not, for he who has a relapse generally consults another medical man rather than return to the one who treated him for the first affection. Still less will he be able to know if his patient is afterwards attacked by another disease in consequence of syphilis; because, if he should get paralysis, or disease of the liver or kidneys, he will never think of consulting the same medical man that treated him for syphilis. Lest I be misunderstood, I will at once state that I by no means consider the diseases that arise after syphilis treated with mercury as mercurial diseases, as some people have done of late. They are all, in my opinion, consequences of syphilis, and may appear after any treatment whatever, as well as if no treatment has been used at all. On the other hand, experience has most decidedly proved to me that relapses are more frequent and of more redoubtable appearance, and the above-mentioned chronic diseases oftener take place, in cases where mercury has been used. I am prepared to find that the majority of my readers will differ from me in this opinion, which, however, I believe I have sufficiently sustained by facts; but experience has shown me that there is always a strange blindness prevailing with regard to the destructive effect of mercury. More than once, when I have gone to see services for syphilitic patients, individuals have been shown to me that were perfect human wrecks, without nose, without palate, or with exanthematic or serpiginous affections that were devouring the body in a most fearful manner, and at the same time I have heard no end of praise of the excellent effects of mercury, and decided condemnation of syphilization.

I commenced practice as a medical man, as already stated, in a circle so small that it was easy for me to observe my patients afterwards, and to become aware of the disastrous effects of mercury when used against syphilis. Accordingly, when I went to the University of Christiania, where I lecture upon syphilis, and also have a service at the hospital for syphilitic patients, I thought it my duty to try syphilization, however great a paradox it seemed to me; and I shall now, after this introduction, explain my proceedings a little further.

In my last paper I gave a brief account of the principal phenomena occasioned by continued inoculations with syphilitic virus taken from primary ulcers, laying special stress upon the fact that after continued inoculations the organism enters into a state of immunity to this virus. By going on inoculating, a period will be arrived at when no more effect will be seen from inoculation of the syphilitic virus. The organism has, during two or three months' time, gone through a cyclus with respect to the reaction against the syphilitic virus, and will afterwards never go back to the state in which it was with regard to that virus before the inoculations were begun. This is just the point of the matter. Here we find an analogy with all the acute exanthems. He who does not look upon syphilization from this point of view ought never to practise it.

It was this analogy with the acute exanthems which led the discoverer of syphilization, M. Auzias-Turenne, first of all to propose its employment as a prophylactic, like vaccination. This suggestion, however, met with a perfectly justifiable opposition, which did not cease even when he gave up that idea, but which exists to this day in nearly its full strength. From the moment I commenced practising syphilization I always most decidedly objected to its being used as a prophylactic, being of opinion that such a proceeding would be immoral, nay even punishable. It is exclusively as a curative method that syphilization ought to be used, and even then only in cases where constitutional syphilis is sufficiently developed to leave no doubt of its existence; but then it ought to be commenced at once. I have often been asked whether it would not be advisable to begin even when the primary sore was there, and was such as to show with certainty that constitutional syphilis was about to follow. To this I answer that there are many cases in which we dare not take upon ourselves to decide that constitutional syphilis will manifest itself, and that consequently it is our duty to wait until we are convinced that the dyscrasia is there.

My treatment of the primary sores is very simple. I cover them with lint dipped in sulphate of zinc solution (two grains to the ounce of water), and now and then I touch them with nitrate of silver, just as I would treat common ulcers. To use mercury is of course quite out of the question, and as a general rule I do not undertake to treat persons by syphilization who have been treated with mercury for their primary ulcers.

To commence syphilization, I take some matter from a primary syphilitic ulcer, usually from an indurated chancre, or else I take it from an artificial ulcer on some one who has been treated by syphilization, and on whom the ulcers result from an indurated chancre. I inoculate this matter on both sides of the chest in a similar manner to that in which vaccine matter is inoculated, being, however, more careful to bring it into the skin, because it does not take with such facility as does vaccine matter. I apply three inoculations on each side; not that I would not have the same result from one inoculation alone, but it sometimes happens that single inoculations do not take, and therefore I generally make three. However, if it is decided to avoid having too many cicatrices, one inoculation may be made at a time at first, and afterwards more, when the pustules and ulcers have become smaller, and when also the matter will take with less facility by reason of its being a later period of the treatment.

After three days I inoculate again, taking the matter from the pustules that were developed from the first inoculation, and being careful to apply the second inoculation at a good distance from the first one, in order to avoid the possibility of the ulcers running into one another, and thus causing large cicatrices. This precaution ought to be observed for the six or seven earlier inoculations.

At the end of three days I make the third inoculation with matter from the second one; and so I continue inoculating every third day, always using matter from the last pustules until it will take no more. Then I employ fresh matter, generally from another person who is being treated by syphilization, and now continue with this matter to inoculate both sides in the same way as with the first. This second matter will bring forth smaller pustules

and ulcers than the first, and the series of inoculations will also be shorter. When this matter also refuses to take any longer, I use a third one, and go on in the same way, still inoculating at the sides. This third matter will produce but very little effect at the sides, and only take for a short series of inoculations. When it takes no longer at all, I begin with a new matter to inoculate on the upper arms, where the pustules and ulcers will not be very small; and at the same time I go on inoculating at the sides of the chest, where the effect will be quite indifferent. It will not be possible, however, to make so long a series of inoculations in the arms as were made at the sides. When the first matter that was inoculated at the upper arms will take no longer, the inoculations must be continued at this place with a new matter, in the same way as I have described for the inoculations at the sides; and when the same result is attained on the arms as at the sides, I go to the thighs and proceed there just as in the other places. Most frequently it will still be possible to get some well-developed pustules and ulcers in this last place, but it will not be long ere a negative result is obtained, and new matter required. As general immunity is approached, it will be found that every fresh matter will take for a shorter series of inoculations than the last. At length it will perhaps just take once or twice. On the thighs it will generally be possible to inoculate several times with fresh matter, and with such a result. When no matter whatever will take any longer on the thighs, general immunity has supervened, and even if it should prove possible to get a positive result from inoculation in some place or other after this time, it is of no practical significance.

Before concluding my description of the way in which I make the inoculations, I must insist on the absolute necessity of the treatment being carried through with scrupulous regularity. Inoculations must be made every third day, and, in case they do not take, a new matter must be tried. The treatment is easy enough, but if the strictest regularity is not observed, and if one does not carry it through until immunity is obtained, the treatment ought not to be called syphilization. I cannot insist too strongly upon this point, because some people have commenced syphilization in order to cure syphilis, but have discontinued it after having made a certain number of inoculations, and have afterwards complained of their syphilization being a failure.

These continued inoculations will have occupied a period of three, three and a half, or four months. The time will occasionally vary, partly owing to the difference of intensity in the virus, partly to the individuality of the patient. Virus taken from the simple chancre or from the suppurating bubo has the greatest intensity. It may be inoculated through a longer series, and the time occupied by the treatment will be longer. It has occurred to me several times when I was using that kind of virus, that the phenomena continued unchanged for weeks, but this still remains to be more closely investigated.

The artificial ulcers that will come forth in consequence of the inoculations must be dressed morning and night with cerate, or else during the day with linen dipped in water; or, if there is any smell from the ulcers, in chlorine water. The patient can be out of bed during the whole course of treatment, and may move about in the open air as much as he likes, temperature making no difference—that is to say, he can stand 17° (Fahr.) below zero, and 86° of heat in the shade without any inconvenience to his ulcers; whether he might be able to stand a still higher degree of cold or heat without its doing any harm has not yet come within my experience.

With respect to the diet, I let the patients in the hospital have full allowance, and in my private practice I let them eat whatever they choose. As to their drink, I make some restriction: of wine I only let them have a mild claret in small quantity, and of the stronger liquors and of ale they are likewise allowed but a small quantity. Tobacco, for smoking or chewing, is not allowed during the syphilization, and for some time afterwards, because the irritation of the mucous membrane that it occasions will call forth and maintain ulcers.

It sometimes will happen during this treatment that mucous tubercles of

the anus, &c., do not vanish very quickly. I then have to touch them with nitrate of silver, and the nearer the case approaches completion the more speedily will these phenomena disappear. It will not unfrequently happen that a new exanthematous eruption will take place during the syphilization; but this is of no consequence. You go on inoculating quietly as before, and so you do also even if iritis occurs, the only thing that is requisite besides the inoculation being to drop a solution of atropia into the eye; leeches, cold-water cataplasms, &c., are quite superfluous. I never even till lately used to drop in atropia, but in one case I had such excessive exudation in one pupil that the sight suffered from it. Even now exudations sometimes occur, but they are so insignificant that they can only be seen with a magnifying glass, and do not at all hurt the sight.

Often during syphilization swelling of different articulations will take place, especially of the wrist and of the ankle, and often of more of these articulations at the time. These articulatory swellings are not accompanied by much fever, and never by any heart-complaint. Another phenomenon observed during syphilization is an erythema which is very like erythema nodosum, only each surface is of a less regular shape than those of erythema nodosum, and the hue is more of a purple-red. This erythema will be found mostly round the wrist, on the forearms, on the forehead, and on the neck. It generally lasts for one or two weeks.

After syphilization there will often be eruptions of tubercula mucosa on the genital organs or anus, or slight ulcerations upon the mucous membrane of the mouth or throat. These will generally disappear if left alone, or else they may be made to do so by being touched with nitrate of silver, and are very seldom of long duration.

I now come to a point which is of considerable practical importance—viz., the general health of the patient during and after syphilization. At the commencement of the treatment, it must be readily understood that many patients will be in a very bad state; they are weak, anæmic, suffering from headache and sleeplessness. These phenomena disappear by degrees during the progress of the syphilization, and at the end of the treatment it will always be found that the patient is doing very well; and sometimes in my private practice my patients have even declared that they felt better than they did before they contracted the syphilis. This state of good health will continue also after the treatment, even if the patient should be exposed to cold, damp, and all sorts of fatigue.

Now, the question is, how large the number of relapses will be in proportion to the number of individuals treated by syphilization, and I will state the numbers that are at my disposal. In my section of the hospital at Christiania there have been treated during the period from the month of October, 1852, till the end of July, 1865, 441 individuals suffering from constitutional syphilis. In my private practice I have treated at least 250, but these I am not going to mention, because they have not been under the control of other physicians. Out of the said 441 patients, 47 have returned to the hospital with a relapse, which makes little more than 10½ per cent.; but as very likely there will be some relapses amongst those that have been treated during these last years, I will suppose the number to rise up to 12 or 13 per cent. But then we have to inquire—what is generally understood by a relapse? I have already observed that many of the patients, towards the end of the treatment or soon afterwards, would get excoriations in the throat or single tubercula mucosa of the genital organs, which symptoms, however, will very soon disappear when touched with nitrate of silver. If these symptoms happen to arise after the patient has left the hospital, he will come back on account of them, and the said phenomena are then set down as relapses. The majority of the above-mentioned 47 cases of relapse belong to this class, and consequently have taken very little time for treatment. Others, however, have been real relapses, and have had to undergo a repetition of the former treatment. Thus 14 patients have undergone syphilization a second time, and 3 have been treated by iodide of potassium. Those who have been treated by iodide of potassium have suffered from tertiary symptoms,

especially orchitis, which I have seen two or three times after syphilization, and insignificant exostoses. With those that have been syphilized a second time, it has been observed that the ulcers will never become so large nor so deep as those of the first syphilization, and that it is impossible to carry the matter through so long a series of inoculations as at the first time—that is to say, *the organism never returns to the same point with respect to the reaction against the syphilitic virus as it exhibited before syphilization.*

However, as I have said before, I do not consider the relapses to be the chief point. I should not have so many objections to the mercurial treatment as compared with syphilization, if it were only the relapses that were to be taken into consideration; but the general health of the patient during the treatment is of far greater importance. I have pointed out how the matter stands after the use of mercury, and I should think we have had time enough to form a well-grounded opinion upon that subject. As to syphilization, we have had only thirteen years of it, but this is a sufficiently long time to judge tolerably well of the question. When I take the patients that I have treated myself, I can only say that I meet several of these every day, and that they continue as well as possible; but it is not, of course, to be expected that this method should be perfection, the less so as we are only yet at the commencement of its development. I will quote the cases that I know of which may be put in connexion with syphilization.

A man who was under treatment eight years ago is still complaining of constant headache and weakness. This man, however, has a large family, and is very badly off; and for a long time after the treatment he used to live in a cold damp cellar. In Dr. Gjör's practice, in the course of the last few weeks, a middle-aged woman has had an attack of apoplexy while being syphilized, succeeded by hemiplegia; but when I saw her a few days ago she had almost quite recovered, and syphilization was continued in the usual manner. This woman has been in very distressed circumstances of late, and has suffered a great deal from domestic troubles.

Of the above-mentioned 441 patients treated by syphilization, two women died whilst under treatment. One of these, Martha G., 50 years of age, entered the hospital on the 30th May, 1859, with secondary symptoms, a maculo-squamous exanthem, and angina. During the treatment she was attacked by diarrhœa, which changed into a dysenteric colic. She died on the 11th of August following. A post-mortem examination was made by Prosector E. Winge. Besides the changes in the colon caused by the dysentery, there were found amyloid degeneration of the spleen and kidneys, and partial granular atrophy of the kidneys, which, according to Dr. Winge's suggestion, would probably be of an earlier date than the syphilis.

The other patient was Andrine J., aged 23 years and a half, who entered, pregnant, with secondary syphilis. During her confinement she was attacked by puerperal fever, and died. Dr. Winge states that at the post-mortem examination nothing syphilitic was found in the interior organs.

One of the patients treated by myself in the hospital died afterwards of heart complaint (valvular affection)—namely, Marie N., aged 32 years. At the dissection made by Dr. Winge, besides the affection in the heart, there were found perihepatitis adhæsiva, cirrhosis ovariorum, and metritis chronica. Whether these changes were exclusively the consequences of the heart-complaint, or whether they owed their origin to syphilis, cannot be stated with certainty.

A patient suffering from phthisis, treated in my private practice for constitutional syphilis, died, a year and a half after treatment, from meningitis. At the post-mortem examination Dr. Winge found no syphilitic changes.

Another syphilitic patient died from pneumonia and meningitis, and another by poison; but these not having been treated by myself, I shall say nothing further about them.

To pronounce in a few words my individual opinion upon syphilization, I would say that I never could have thought it possible to cure syphilis with such certainty as we find ourselves enabled to do by means of syphilization

Here I must, however, make the restriction, that it is necessary, in order to obtain this favourable result, that the patient should not have been formerly treated with mercury.

[In patients who have been previously treated with mercury, we have not the same certain development of phenomena when we employ syphilization, and we cannot look forward with certainty to a happy result of the treatment.]

It is interesting to find that long ago an experiment was made, not of regular syphilization, but of inoculation, with fresh syphilitic matter, in order to cure an individual who had been treated in vain with mercury. This experiment was made by the French military surgeon, M. Percy. He examined, in the year 1777, two patients suffering from old-standing syphilitic disease, that would not disappear even after a long and scrupulously executed course of treatment. Tired of the physic as well as of the strict diet, they went and exposed themselves to new infection, whereupon the old as well as the new disease disappeared by merely recommencing the same treatment that had hitherto been employed without success. From this he concluded that the new infection had so modified the organism as to make it anew susceptible to the ordinary curative means. The following year, upon being consulted by an individual suffering from constitutional syphilis that had defied all the different mercurial remedies, M. Percy resolved to try an artificial infection. He therefore made three inoculations on each arm, and allowed the syphilitic pustules to be gradually developed. Six weeks later, when the ulcers of the right arm were healed and those of the left nearly so, he commenced a mercurial treatment that completed the cure. It must be observed, however, that the formerly existing symptoms were not improved by the inoculations; on the contrary, the ulcers of the tonsils were still more extensive. Percy's experiment was not repeated by one else, but there were those who, without making any experiments for themselves, condemned his, and he was obliged to leave them alone for the future. Percy's own words concerning this affair were: "Ce n'est pas une découverte, c'est seulement un aperçu—c'est, si l'on veut, une délire de mon imagination."

Percy's observation had been long forgotten when syphilization appeared, and now it was not merely a single inoculation that was made, but a whole series, as before described. The new method was employed at random, as well with patients who had been formerly treated with mercury as with those who were being treated for the first time; and as to myself, I had no idea whatever when I began that a previous mercurial treatment could have any influence on the result. Accident here helped me to overcome some of the most formidable obstacles to the treatment by syphilization. I commenced by taking under treatment only two individuals, in one of whom the constitutional symptoms had recently appeared, whereas the other had undergone several mercurial treatments. While the first patient improved gradually and at last got rid of all syphilitic phenomena, the other was still covered with a syphilitic exanthem. From this and other cases that came under my observation, I soon saw that mercury had a decidedly important influence upon the result of syphilization. If this fact had not from the very first been so evident to me, I should certainly have chosen mercurialized individuals for my experiments, and the less perfect results that I should in this way have obtained would have given me quite a wrong idea of the whole subject of syphilization. Once having the influence of mercury so evidently shown, I had a rule for my guidance, and the relapses which occurred among patients who had formerly been treated with mercury could not shake my faith in syphilization.

Although it is sufficiently apparent that syphilization does not act in a similar manner with those who have been treated with mercury as with those who have undergone no treatment, yet the practical physician will want to know, if, notwithstanding, syphilization is preferable to other modes of treatment, and he will also want to know whether, in case no other treatment should be possible, syphilization might still be of some use. I have not such rich material at my disposal for answering these questions as I had for showing the salutary influence of syphilization when no other treatment had been for-

merly tried. I have had but 72 patients in the hospital, and about 30 in my private practice: these latter cases, however, I shall not cite, since they have not been subjected to the control of other physicians. 22 persons out of the 72 have returned with relapses, or 30½ per cent.

This, however, does not make us much wiser. We require to know what the symptoms have been from which they were suffering. I shall classify them in the following manner:—

1. Early constitutional (secondary) cases, in which, after the mercurial treatment, affections of the skin and mucous membranes have appeared, like those against which the mercury was first employed. (In one case mercury had been used for the primary ulcer.) Among this class I reckon 28 individuals, of whom 8 have had a relapse, or 28⅔ per cent. Five of those have had slight relapses, but in the other three the relapse has been of a more advanced form; and the one that had been treated with mercury for the primary sore had had several relapses of a rather obstinate nature. It must be owned that this man had been given to drinking habits, which are known to make the syphilitic dyscrasia worse; but it is also not unlikely that the use of mercury at so early a period contributed to make the form so bad. It is my personal experience, and it is also proved by my before-mentioned statistical work, that the worst forms will most frequently be developed after the early use of mercury, especially when it has been employed in primary cases; and I will set it down as a rule that "the later, in the course of development of constitutional syphilis, mercury is given, the less destructive it will be to the organism." In most of the 20 cases syphilization has produced as certain an effect as in cases not treated before.

2. Late constitutional cases (tertiary forms) for which 42 individuals have been treated. Out of these 14 have had a relapse, or 32⅓ per cent.

These tertiary forms may, however, also be divided thus:—(*a*) The tuberculo-serpiginous syphilides, or the more or less excavated ulcers after suppurating gummata. These ulcers and syphilides are very slowly cured; the tuberculo-serpiginous syphilides will disappear by degrees, but sometimes not entirely. (*b*) There may be swelling of the bone or periosteum, accompanied by dolores osteocopi, and there may be caries. Swelling of the bone may be removed by syphilization, but only when it is quite recent; and dolores osteocopi will not be much the better for it, whereas it will do more good to caries. Thus I have seen more than once carious parts of the palatum durum removed and the opening filled up again, in cases where syphilization was somewhat advanced when the removal took place. (*c*) The nervous symptoms developed in consequence of syphilis will not be removed by syphilization unless they have recently appeared, and even then not with any kind of certainty. In cases of mental alienation that have arisen in consequence of the syphilitic dyscrasia, syphilization has as yet never been tried, but there is reason to believe that it would do no more good here than in nervous affections generally.

In many of the tertiary and in some of the secondary cases I have given iodide of potassium in conjunction with syphilization, and it might perhaps be concluded that it was the iodide of potassium, and not the syphilization, which had produced the result. This is, however, not the case. The effect of syphilization is never more plainly seen than in cases of old-standing disease, where every means has been tried, until the organism has succumbed to mercury and iodine. Syphilization is tried, and it will often be seen how the local symptoms are influenced in different ways; but if there is little or no effect to be observed from the continued inoculations, and iodide of potassium is then tried, it will be found to act with its usual rapidity, whereas previously it was inoperative.

It is not, however, merely the effect syphilization has upon each symptom, but its effect upon the whole organism, which renders this treatment worthy of consideration. In this respect its power is most wonderful. The strength is returning, the weight is increasing, and the mental condition of the patient is being improved at the same time. This is not, however, an invariable rule; cases sometimes are met with in which we can do but little. Nevertheless, if

we look upon the matter in general, I firmly maintain that, even for relapses after mercurial treatment, we can do no better than use syphilization. If hitherto I have not always followed this rule in my own practice, it has been because syphilization is still comparatively new, has been reluctantly received and still awakens considerable resistance. If after syphilization there has been a relapse nobody inquires if mercury had formerly been used; and as the relapses will always be much more frequent after mercurial treatment than in cases where no mercury has been employed, I have hitherto seldom used syphilization for mercurialized individuals.

With regard to the process of syphilization in persons who have been formerly treated with mercury, it requires to be noticed that there often will be no regular series of ulcers produced after the inoculations, especially in cases of old tertiary complications. It sometime may be very difficult to make the inoculations take at all, and, if they do take, the effect will be but trifling and the series of inoculations short. It is also not certain that the first series of inoculations will be the longest; it often happens that the second, the third, or a following matter can be carried through more successive inoculations than the first. It will often happen that a few inoculations will be attended with a perfectly negative result; but after some weeks a new inoculation will take, and even with more effect than the first. It is evident that the organism is influenced by the first series of inoculations, short though it be. It will also happen that by giving some iodide of potassium between the application of the two matters the effect of the latter will be a great deal stronger. On the whole, the treatment of these old forms requires no small amount of patience and perseverance.

It will be found in some cases that, even with those who have used mercury, the inoculations run through their normal course, especially when the syphilitic phenomena are still limited to the secondary forms. In such cases there will sometimes be a considerable development of the ulcers.

There are persons who maintain that syphilization should not be resorted to until every other means had been tried in vain; until nose or palate was lost, and caries had destroyed different bones, especially those of the cranium; when phagedenic ulcers were spread over different parts of the body; when interior organs began to suffer, and a cachectic state announced the approaching end; then, and only then, they say, syphilization is indicated. When we think of the happy results that syphilization will give us in cases where no mercury has been formerly used, and when we also observe that those dreadful forms of syphilis often witnessed after mercurial treatment are hardly ever seen after syphilization, I do not see the reason why we should defer its adoption until we have, so to speak, artificially called forth those severe and almost hopeless forms. Why not commence by using the method by which we do not run the risk of evoking them at all?

These are briefly my views upon syphilization, such as I have observed and practised it. The observations are, however, not all my own, although I have not quoted other names. They are partly those of MM. Auzias-Turenne and Sperino, and partly those of men who have contributed to the practice of syphilization later than myself—viz., MM. Gjöt and Bidenkap. It may readily be understood that in so wide a field, in which so few explorers are engaged, many things must have escaped observation. To do this work effectually would require the united efforts of a great many, and I think it would be well worth while to examine the syphilitic as well as other animal viruses. This examination of the syphilitic virus ought, however, to be made exclusively by those who are convinced of the destructive effect of mercury in syphilitic diseases; and if the method of syphilization is adopted, it ought to be carried through according to what I have indicated here to the very minutest detail: not that I assert my method to be the best one in existence, but because I know with certainty that syphilization carried through in the way I have described leads to a successful result. If any one has tried and has become convinced of this, then I shall be glad to hear that he is endeavouring to find a better way, which may very well be possible.—*Lancet, Sept.* 9, *and Oct.* 14, 1865, *pp.* 284, 425.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

76.—ON THE FACILITATION OF THE FIRST STAGE OF LABOUR

By Dr. Andrew Inglis.

[Many different means have been used for favouring the progress of the first stage of labour. Simple dilatation with the finger and the use of india-rubber dilators are the principal local means, whilst chloroform, antimony, bleeding, and emetics, have been employed with the intention of acting upon the uterus, through the general system.]

To show that there is good reason for believing that another means—namely, separation of the membranes for some distance round the os—will promote dilatation much more efficiently than any hitherto proposed, is the object of this paper; and I believe that where attempts at direct dilatation have been successful, the success has been in great part owing to unintentional separation of the membranes. In pursuance of my object, I shall call attention to the effect on the character of labour of such a separation, either as occurring spontaneously or effected artificially for its induction or facilitation.

In the first stage of labour coming on and proceeding without interference, there are two opposite conditions of the passages—one in which there is a copious discharge of viscid mucus, and which is often called a "wet labour;" and another, in which there is hardly any, and labour is called "dry." At the full time, the first seems to occur normally in the cow, mare, bitch, &c., and, I am inclined to believe, is natural also in the human female. I consider the following as being the natural process in women:—The ovum having become ripe, the membranes separate from the cervical portion of the uterus, if not from the whole surface. They then by their weight press more heavily against the cervix, even when the patient is lying down; and as the pressure is soft, equable, and continuous, the cervix gradually yields to it and becomes quite slack, and this takes place without the occurrence of pain. Next, when relaxation has become complete, the mucous discharge commences, proceeding from the uterus. Finally, a pain comes on and terminates the first stage. That the discharge comes from the uterus is shown by its protruding from the os previous to its appearance in the vagina. Besides, it is only found when the membranes are already separated, and is very often tinged with blood before pains have been felt. A process resembling this form of the first stage may be seen where the other muscular canals are concerned, and perhaps most prominently in the case of the rectum. If the finger or bougie is gently and cautiously inserted past the sphincter ani, and kept there for some time, complete relaxation gradually ensues, a profuse discharge from within the sphincter comes on, and, if the bougie is allowed to remain long enough, the muscular fibres above begin expulsive action.

The foregoing explanation seems equally applicable to "wet" cases of abortion or miscarriage in the human subject, except that in such cases healthy ripening of the ovum cannot be said to be the cause of the separation of the membranes.

Since I have begun to pay attention to the subject, I have met with many examples where the first stage has been sudden and short, on account of pre-

vious separation of the membranes. The following are details of three of these:—

In November last, I was sent for to attend a patient who had felt a subsidence of the uterine tumour, and a slight bearing down, affecting the bladder and rectum, but without pain. Examination showed the vagina to be in a normal condition, but the os was as large as a shilling and quite thin. The finger could be pushed a good way into the cervix, and the membranes were found separated as far as could be reached. There was no discharge. This condition of the parts continued for about sixty hours after I saw her first. When I called on her on the fourth morning, no pains had yet appeared; but there was a quantity of thick mucus around the os, and, on pressing the abdomen, the membranes distinctly yielded and came down a little. Soon after my visit, two or three pains in close succession came on, and the child was born before any one could leave the house for assistance.

At the last meeting of the Society, I mentioned a case where, very shortly after labour had begun, and while the patient had as yet had very few pains, I found the os dilated and the membranes protruding beyond the external parts. Though she had not sent for any one, she had felt the pressing down for days before; and as the membranes were displaced to the extent I have mentioned, a minute or two after the first pain had been felt, they must have been previously separated. It is worthy of notice that in this case there was great deformity of the pelvis.

About the beginning of October, I was called to a case where labour had not been expected by the patient for some weeks, but where the membranes had ruptured in consequence of violent bodily exertion. I found the liquor amnii had only partially escaped from a tear in the membranes, and was still coming away every time she moved. There was a quantity of thick mucus around the os, which was slightly open, and moreover showed by its character that the ninth month was nearly if not quite complete. The pains came on in about twelve hours, and labour was over in less than a quarter of an hour. In such cases, where the liquor amnii comes away before labour, I believe our fears as to the character of the labour should be at once allayed if we discover the thick mucous secretion, holding that its presence is always proof of previous separation of the membranes accompanied by a soft and dilatable state of the os.

The other form of the first stage, which I have designated "dry," is very different. Preliminary to labour there is no separation of the membranes, and consequently no slackening of the cervix, and there results a great increase in the amount of uterine exertion required, even where nothing worse is entailed; for, though separation and dilatation generally at last take place, they do so only at the expense of multiplied and more distressing pains. At best, the pains may stop for a time after causing some separation, and labour on their return may thereby then come to take on the "wet" form; but very often no such intermission occurs, and dilatation costs a long series of wearing-out pains. Moreover, the membranes may rupture, making dilatation have to be effected by the fœtus, and therefore even slower and more painful. Again sometimes the imperfectly dilated cervix is forced into the pelvis along with the head, and becomes jammed between the two, increasing greatly the pain and delay. Lastly, the resistance to dilatation may cause permanent cessation of the pains, or the cervix, or body of the uterus, or both, may rupture.

For some time when first in practice, I was in the habit of inducing premature labour in cases requiring such interference, either by introducing a catheter between the membranes and the uterus and leaving it there, or by using sponge tents. In these cases, labour often came on very soon, though a few resisted for a long time the action of both means combined, and the membranes had to be ruptured before pains could be made to come on regularly; but in all cases, whether labour followed at once or came on only after great expenditure of time and trouble, the first stage was slow and painful, and in some the pains continued a few days before dilatation was complete.

About eighteen months ago, I attended a case where Dr. James Sydney

brought on premature labour by the late Dr. Hamilton's method. By means of Dr. Hamilton's uterine bolt, he separated the membranes some inches round the cervix, and the instrument was passed in and turned round every twelve hours. About forty-eight hours after the first separation of the membranes, the os was soft and dilatable, and a little discharge was apparent at the top of the vagina. Six hours later, I found the discharge copious; a few minutes after I had ascertained this by examination, labour came on suddenly; and in three or four pains the first stage was completed, and the head well down in the pelvis.

Soon after this I induced premature labour in the same manner in another case. In about forty-eight hours the discharge was copious, and the os soft and dilatable, but no pains had appeared; but immediately after my examination one pain came on and completed both stages.

Since the first of these cases I have never employed any other means for inducing premature labour, being convinced that it is the easiest for the patient, though not always the speediest. The first stage of labour induced by it seems identical in all respects with that of the "wet" form of labour occurring spontaneously.

The circumstance of the first stage of labour, so induced, presenting so favourable a character has led me to use that separation also for the facilitation of labour already begun in cases rendered tedious by so-called rigidity of the os. The number of cases in which I have had recourse to this mode of procedure is perhaps not yet sufficiently considerable to entitle me to lay much stress on these as bearing on my argument; but I may state that all of them which can be considered as affecting the question seem to support the practice I am advocating. The following are particulars of a few:—

On the evening of 5th November, I saw a patient in the fifth month of pregnancy, who had had strong pains all day; she thought the child was dead, and I could discover no fœtal sound. The os was firm and barely admitted the finger, and there was no discharge. I ordered some opium, and called the next morning. The os was then nearly in the same state, but the pains were more severe. At night she was much worse, the opium having failed to stop the pains. The os was still firm and the discharge absent. I therefore separated the membranes from the cervix as far as I could reach, after which the pains ceased; and, after waiting for about two hours, I left her till morning. About ten hours afterwards I was sent for. She had not had any more pains, but felt something pressing down at the vulva. I examined and found that the entire ovum had slipped down into the vagina. The child had evidently been dead for some days.

On the 2nd of February, I was sent for to attend a woman at the sixth month. There had been some strong pains; the abdomen was hard and contracted, and very tender to touch; and the os would not admit the finger. The pains continued for two days and two nights without producing any local change. As the fœtal sounds were normal I continued to give sedatives regularly during that time, but without any apparent effect on the pains. Vomiting then came on; the pulse rose to 130; and the tongue became brown and coated, like that of a patient with fever. I then tried to introduce my finger to separate the membranes; but found the os too firm to admit it, and therefore had recourse to a sound. The placenta being at the fundus, I separated the membranes all round to about the middle of the body of the uterus. The intervals between the pains then became longer, and a large bag of membranes gradually protruded, and in less than two hours the patient was delivered. The child was alive until immediately before birth, but never breathed.

On the 7th February, one of the New Town Dispensary pupils went to a patient in labour who was in the seventh month of pregnancy; and after she had been about fifty hours ill he sent for me. The os was only about the size of a shilling; and as there was no chance of labour going back, and moreover, the child seemed to be dead, I separated the membranes with the finger as far in as I could, with due regard to the position of the placenta, which was near the os, and had therefore to be avoided. After separation, the

pains, though stronger, began to come on at longer intervals, and I believe would, in the absence of further interference, have died away; but, as I feared to risk longer delay, they were kept up by mechanical irritation of the uterus, and the child was extracted an hour and a half after my arrival. It was much decomposed.

On the 17th of May, I was called to see a patient in the eighth month. She had severe purging and vomiting. She suspected the child to be dead, and, as she was very dangerously ill, I considered it advisable to encourage the labour, which she thought threatening. I therefore separated the membranes with the finger as far as I could reach, and, about ten hours after, a putrid fœtus was unexpectedly expelled by a single pain.

In one of the cases I reported to the Society last month, I examined and found labour coming on at the full time while the membranes were still adherent. I separated them, and gave a large opiate. The pains ceased, but returned in about twelve hours, and though the pelvis was badly contracted, the first stage was got over with great ease.

To recapitulate—

1st, The easiest form of the first stage of labour is characterized by protrusion of the membranes and a copious discharge.

2nd, These are always direct consequences of separation of the membranes.

3rd, The result of artificial separation seems precisely similar to that of spontaneous.

The following is the practice I would recommend in regard to separation of the membranes:—1st, That it should always be the initial measure in the induction of premature labour; and that, until complete relaxation of the os has resulted from it, there should be no further interference of any other kind; 2nd, That when labour has begun without previous separation of the membranes, and these are still adherent, they should always be at once separated, as the best means of overcoming the rigidity of the os, and the painful and prolonged first stage, which almost invariably accompany such a state of matters.

In the latter of these two cases, if the pains are severe and ineffective, I should be inclined to recommend sedatives to be given at the same time, in the hope of gaining time for painless relaxation to take place.

With regard to the means of separating the membranes, I have in most cases been able to do it with the finger, though in one or two an instrument was required. In one case, which I have not reported, I could not at first get the finger far enough in to effect any extensive separation; but the small portion round the os, which was thus denuded, was so relaxed an hour after, that I was able to insert the whole finger and separate to the desired extent.

Where I have used an instrument it has been Dr. Hamilton's uterine bolt, which I have already mentioned. From its rounded form at the end there is but little fear of rupturing the membranes with it.

Since reading the foregoing I have added the following as manifestly supporting my views:—

Dr. Trask, in his prize essay on Placenta Prævia, has shown, on the one hand, that where the placenta is not much separated the os is almost always rigid, and that the force used to procure dilatation is the cause of death in a large proportion of the fatal cases of placenta prævia; and, on the other hand, that those cases in which separation has taken place are specially characterized by speedy delivery by the natural efforts. He mentions also that the effect of artificial separation in such cases, as regards speedy expulsion, is nearly, though not quite, as good as that of natural separation.

Moreover, in all the cases I have met with myself, I have found delivery take place naturally and easily where separation of the placenta had *previously taken place* to a great extent; while, after slight separation, extraction of the child had always to be effected by artificial means.

The usual history of primiparous cases seems also to be confirmatory of the views I have now advocated. The duration of the pregnancy being gen-

erally shorter, labour commences before the separation of the membranes has occurred, and the first stage is in consequence much slower than in subsequent labours. This is commonly supposed to be the result of the absence of that mucous discharge already referred to, but I think, in reality, chiefly arises from the want of previous separation of the membranes, of which the mucous discharge is only a symptom.—*Edinburgh Med. Journal, July*, 1865, *p.* 24.

77.—THE ACTIVE CONSTITUENTS OF ERGOT.

Mr. Wenzel, a chemist, states that he has succeeded in isolating from ergot two vegetable nitrogenised alkaloids, which he terms respectively "ecbolina" and "ergotina." He calculates that half-a-grain of ecbolina produces the same effects as half-a-drachm of the ergot—judging from comparative experiments performed upon himself. It induces a specific action on the spinal column and muscular system, which leaves behind a great amount of debility. Ergotina seems to be less active than its congener, and induces some cerebral excitement. It has been used in several cases of uterine hemorrhage with satisfactory results, but from the distressing nausea and vomiting, with intense headache, it gave rise to, its employment had to be suspended.—*Méd. Times and Gazette, Sept.* 2, 1865, *p.* 263.

78.—ON THE USE OF ERGOT IN CASES OF TEDIOUS LABOUR FROM INEFFICIENT ACTION OF THE UTERUS.

By Dr. D. Dyce Brown, M. A., Fellow of the Obstetrical Society of London.

The reason of ergot being inadmissible in so many of the cases of labour is, the exceedingly powerful effect which is apt to be produced by the 3ss. doses of the powder—excessive and often continuous pains coming on, even after a single dose. This result is, as is well known, most dangerous to the mother, unless the case be what one might call a typically normal case (barring the want of uterine action), and to the child, unless the labour be completed in about an hour and a half after. But if we could give the ergot in such a way as to avoid this excessive action, and produce pains, when they are absent or inefficient, closely imitating, or not to be distinguished from, natural pains in their strength and period of intermission, it is obvious that not only will it be much safer and better to use it thus in cases where there would be no risk with the usual doses, but its range of action will be much increased, so as to include cases where it is inadmissible in the usual mode of exhibition. Such an object can be obtained by the very simple plan of giving the ergot in doses of five or six grains of the powder. I mention the powder (which ought to be fresh ground), as I always use it in preference to the liquid preparations, on account of its certainty.

The idea of using these small doses first occurred to me from observing the frequent unmanageable action of the usual doses; and with the object of trying to get a mild action such as I have above described, I used gradually decreasing doses, till I came to about 5 grs. Ten grains seemed to produce, in the cases in which I used it, a safe, moderate, and natural amount of action; but as some constitutions are certainly more susceptible of the action of ergot than others, it is quite possible that even 10 grs. might in some other cases have produced more powerful action than I wished to bring on. I therefore think 5 grs. is the best dose to use, as it cannot have a violent action, and there is no good in using a smaller dose than this, as it is uncertain in its action, though I have succeeded with about 3½ grs. Given in this manner, in the majority of cases, after a single dose, uterine action becomes increased in from five to twenty minutes, and the pains exactly resemble normal uterine pains in their strength, duration, and period of intermission; in fact, they are what might be called excellent, laudable pains, and no more.

Sometimes the first dose seems inoperative, or at least not sufficient to bring the uterine action up to the required standard. In this case, after an interval of a quarter of an hour or twenty minutes to watch its effect, the dose may be repeated a second time. Rarely have I found that a third dose required to be given; while, if no action is produced by the third dose, I believe the constitution will be found to be unsusceptible of the action of ergot. I before remarked that different constitutions differ in their susceptibility to this drug, and I am satisfied that this sometimes goes to the extent of complete unsusceptibility. Dr. F. H. Ramsbotham suggests the possibility of this idiosyncrasy. After stating ("Obst. Med. and Surg.," p. 210) that its alleged inefficiency in certain cases may be owing to the powder not being fresh. he goes on to say:—"It may also be partly owing to the constitution of the patient not being susceptible of its peculiar action. We know that some persons are not susceptible of the peculiar action of mercury; and we may easily believe that in the same manner some constitutions may be unsusceptible of the peculiar action of the ergot of rye." The plan of administering ergot in the small doses already mentioned I take the liberty of bringing before the profession, as it is recommended in no book I have seen, and from what I can learn from personal intercourse is next to unknown. Dr. D. D. Davies ("Operative Midwifery," p. 77), indeed, speaking of ergot, says that it is used in America in doses of a tablespoonful every ten minutes of an infusion of a scruple of the powder in an ounce and a half of water—that is, about seven grains of the powder to the dose. Dr. Simpson ("Obst. Works," vol. i., p. 372) says, in reference to this remark of Davis's, which is founded on a statement in Prescott's work on Ergot, that "assuredly either the dose is accidentally understated, or it is in itself far too little to be of any avail." The substance of the last part of this sentence expresses the notion which is generally held by practitioners, namely, that ergot in smaller doses than a scruple in infusion, or corresponding quantities of the liquid preparations, are useless in labour, and that 3ss. is about the proper average dose. Dr. Simpson himself, however, in his memoir on "Anæsthesia in Labour," brings forward evidence of its power in inducing or quickening uterine action when received into the system in small quantity and in a more uncertain manner than when given by the mouth, namely, by inhalation. I may be allowed to quote the passage, as it is extremely interesting. After pointing out that labour pains continued as regularly under anæsthesia as before the inhalation was begun, Dr. Simpson goes on to say:—"Indeed, in some cases they have appeared to me to become increased as the consciousness of the patient became diminished. This has more particularly occurred with one or two patients who breathed ether combined with tincture of ergot, or containing a solution of its oil. A woman was brought into the Maternity Hospital on January 28, after being in labour for thirty or forty hours. It was her second child. Subsequently to her entering the hospital at 7 p. m. scarcely any decided uterine action could be said to take place. The os uteri was well opened, but the head was still high up in the pelvis, and when I saw her at 4 a. m. of the following morning—nine hours after her entrance into the hospital—little or no advance whatever had been made, and the case was becoming an anxious one. She was then made to inhale equal parts of sulphuric ether and tincture of ergot. In the course of a few minutes a series of extremely powerful uterine contractions supervened, and the child was born within a quarter of an hour of the commencement of inhalation. Dr. Simpson goes on to ask, "In this case was the re-excitement of strong pains the result of the action of the sulphuric ether, or of the ergot, or of both? or was it a simple but very strange coincidence? More facts than I yet possess are necessary to decide such a question, but I have seen some cases which lead me to believe that other therapeutic agents besides those I have named may be readily introduced into the system by means of pulmonary inhalation." From this last sentence it is evident, I think, that Dr. Simpson inclines to answer his own question by attributing the increased action to the ergot. And from the cases in which I have used the ergot in small doses, and on the results of which I found this paper, I believe that

this is the proper explanation of the sudden accession of pains. Its success, also, in doses of a few drops of the liquid preparations in checking excessive lochia and other discharges from the uterus gives evidence that it is capable of action on the uterus even in very small doses.—*Medical Times and Gazette, Nov.* 4, 1865, *p.* 492.

79.—ON THE INDUCTION OF PREMATURE LABOUR IN A CASE OF DEFORMED PELVIS.

Under the care of Dr. Graily Hewitt, at the British Lying-in Hospital.

[The patient was 28 years of age, and seven months advanced in pregnancy of her first child. Her shortness of stature and general appearance induced Dr. Hewitt to examine the pelvis per vaginam. The pelvis was found considerably contracted in its antero-posterior diameter; the conjugata was as nearly as possible three inches.]

After giving the case due consideration, Dr. Hewitt came to the conclusion to induce artificial premature labour at seven months and a half, and to attempt delivery at that period by the forceps or by turning, in order to give the patient a chance of bearing a live child.

On Jan. 20th, 1865, this project was carried into execution. The patient was now, however, about a week over the calculated seven months and a half. The head presented. At ten a. m. the os was gently dilated by means of the finger, and one of Dr. Barnes's india-rubber water-pressure dilators introduced. In an hour the os was enlarged to the size of the thumb. A No. 2 dilator was then introduced, and allowed to remain until three p. m., the dilatation being increased by injection of more fluid from time to time. At three p. m. it was withdrawn for a time. Pains had now set in periodically, at ten minutes interval; but they were feeble. The dilator was reintroduced, and the patient left again until eight p. m. At this time the pains were pretty strong, and tolerably regular. The membranes were unruptured, the head presenting; but still quite above the brim. The head appeared well ossified. The more complete examination, now possible, confirmed the accuracy of the previous measurement of the pelvis.

The os being now well dilated, the membranes were ruptured at half-past eight p. m. After waiting for nearly an hour, it was found that the pains had no effect whatever in inducing descent of the head. Chloroform was accordingly administered, and the forceps introduced. One blade of Dr. Graily Hewitt's eight-inch straight forceps passed readily, but the second blade could not be applied. No better success attended the use of a pair of curved forceps; and finding the head very hard, larger than had been anticipated, and the pelvis appearing also a little contracted in its transverse diameter, Dr. Graily Hewitt decided to relinquish further efforts to apply the forceps, believing it impossible to drag the head through even after their application. The next alternative was turning. Without further delay this operation was performed at ten p. m., the left leg being first brought down. At this moment the child was alive. The second leg was secured, but very great resistance was encountered in bringing the breech through the pelvic brim; so much so, indeed, as to show that hopes of securing a live child were quite gone. By the time the breech had passed the pelvic brim the cord had ceased to pulsate. The arms were brought down one after the other by the blunt hook, and the shoulders extracted; but, as had been anticipated, there was not room for the head to pass the brim. It being manifestly better to empty the head than to further bruise the soft parts of the mother, the occipital bone was accordingly perforated, and the head (occiput looking directly backwards) extracted by the craniotomy forceps at half-past eleven p. m. Pressure was immediately made on the uterus by the matron, Mrs. Firth, and the placenta thrown off spontaneously in the course of less than five minutes. The uterus was well contracted. The next day the patient felt comfortable, and had slept well. The catheter was necessary to empty the bladder, and the labia were rather more swollen than usual; but she was

free from pain. The diet ordered consisted of meat, eggs, and milk daily, and six ounces of brandy. Some slight disturbance attended the formation of the milk, which soon subsided, and the patient is now doing well.

The child was well formed, a female. Examination and measurement clearly showed that the patient must have been mistaken in her idea as to the date of the commencement of pregnancy; for the child measured seventeen inches and a half in length, was well developed, without any sign of immaturity. What offers, however, more conclusive proof of the age of the fœtus is the condition of the lower epiphysis of the femur. Ossification of the lower femoral epiphysis only begins in the thirty-sixth or thirty-seventh week of gestation, and the size of the osseous nucleus is only about three lines in diameter when the fœtus reaches maturity. In the case of this fœtus the osseous nucleus measured over two lines in horizontal diameter, and there can be little doubt that the full term of gestation had only been anticipated in this instance by a week or two at most, instead of five or six weeks, as had been intended. It is unfortunate that the mistake occurred, though that mistake was almost inevitable, for it of course rendered futile the attempts to deliver the woman of a live child. If labour is induced on a future occasion in the manner which was intended at not later than seven months, the patient may probably have a living infant. With respect to the labour itself, it was completed in a little over twelve hours. The use of the india-rubber bag was of great service in dilating the os and rendering further manipulations easy, and, considering the difficulties of the case, the patient made an excellent recovery. This result is doubtless in great part due to the short time the soft parts were subjected to severe pressure.—*Lancet, June* 3, 1865, *p.* 593.

80.—ON ABORTION IN THE EARLY MONTHS OF PREGNANCY.

By Dr. Charles Bell.

It appears that it is impossible to guard against abortion in every instance, as for the most part the causes take effect suddenly and unexpectedly. Our object therefore should be more particularly directed to its treatment, so as to diminish as much as possible the effect on the constitution, and to place the patient in the most favourable position for regaining her health. These objects will be most satisfactorily attained by ascertaining early the true condition of the ovum, and assisting the womb to expel it without delay, if there is reason to believe that it is blighted, in place of vainly attempting to check the natural efforts of the womb to free itself of what has actually become a foreign body and a source of irritation and disease.

The symptoms which seem to indicate the death of the early fœtus are a blighted and shrunk condition of the papillæ of the breasts, and a relaxed and flabby state of the nipple, accompanied by a watery or rusty discharge, coming from the womb without pain or uterine effort, which are generally attended by extreme lassitude and exhaustion, amounting in some instances to faintings. In such cases, I should advise, even where there has been no flooding, the use of the ergot; and if it failed to promote the action of the uterus, then I should recommend the introduction of sponge-tents, which will be sure to accomplish the object. Should flooding come on, which is almost certain to occur to a greater or less extent, it may be necessary to employ plugging along with the external application of cold. To these remedies may be added the internal use of large doses of the elixir of vitriol or the gallic acid. When uterine action is attended by severe pain, opiates or chloroform may be given with advantage. In order to restore the tone of the system after abortion, I have found chalybeates with port wine the most useful tonics.

In making these practical remarks, I am fully aware they contain nothing that is new or original; but I am induced to urge their application in consequence of having observed the absurdity of attempting to prevent abortion taking place after it had become obvious that the fœtus was blighted; and I

have known tedious bad health produced by the blighted ovum being retained in the womb.—*Edinburgh Medical Journal, Aug.*, 1865, *p.* 132.

81.—CASE OF RUPTURE OF THE UTERUS; ABDOMINAL SECTION; SUBSEQUENT PREGNANCIES.

By Dr. Samuel S. Dyer, Ringwood.

[When first seen by Dr. Dyer, rupture of the uterus had already occurred. The patient was quite sensible, the pulse quiet and fluttering, the breathing short and hurried, and the skin cold and clammy. The fœtus could be distinctly felt in the abdomen, the uterus being firmly contracted.]

Mr. Clifton told me he had been called this day at six o'clock in the morning, when he found the os uteri about the size of a shilling. The presentation was natural; the pains trifling and ineffectual. He waited two hours; and, as no progress was being made, he gave a dose of ergot, and continued in attendance another hour. But no increase of pains in force or frequency took place; and he left for his surgery and to see other patients, promising to return; and said that, if wanted before doing so, he was to be sent for. He heard no more of the case till the afternoon, and then drove down to Mockbeggar, taking his forceps with him. He was then told that no pains of any consequence had come on till the middle of the day, since which they had been steadily increasing in force and frequency. The os was now fully dilated, and the head at the brim of the pelvis. He went down stairs, determining to give more time for the natural termination of a case which now seemed to be going on favourably, leaving two female neighbours with the patient. In about an hour, he was called hastily, and found the woman fainting, blood issuing from the vagina; the head higher than it had been, and rapidly receding. The bystanders said that there had been a violent pain, during which they heard a noise as if something had given way.

The result of our consultation was, that but one thing could be done to save the woman's life; and that it was our duty to give her this chance. I went back to the room to tell her our decision, to which she readily assented. Mr. Pridham of Broadway, my assistant at that time, drove back to my house, a distance of three miles, for instruments, chloroform, and appliances; and immediately on his return—four hours and a half from the rupture of the uterus—we performed the operation of gastrotomy for the removal of the child and placenta. The patient being well under the influence of chloroform, Mr. Clifton made an incision through the skin and cellular tissue, from an inch above the umbilicus to two inches above the symphisis pubis—the bladder having been previously emptied; he then divided the linea alba, and afterwards the peritoneum to the same extent, using the forefinger of the left hand as a director. The child and after-birth, with membranes and coagula, were quickly removed; and the uterus was seen to be well contracted. We exposed and handled the contents of the abdomen as little as possible, and brought the edges of the wound together, introducing silver-wire sutures through all the structures from the skin down to and including the peritoneum at short spaces. We applied long and broad straps of adhesive; dry lint, wadding, and a broad abdominal bandage.

When the patient recovered consciousness from the chloroform, she expressed herself as quite relieved, and feeling comfortable. We left a mixture of opium and brandy to be given at intervals during the night, and arranged to meet the next morning.

July 30th, 9·30 a. m. On entering the bedroom, we were as much astonished as gratified to see M. A. W. with a smiling, cheerful countenance, and to hear that she had passed a comfortable night, and felt as well as she had done after her former confinements. Such was her statement. The pulse was 110. She had no pain, and very little tenderness. Her urine passed freely, and there was some lochial discharge. We desired that the brandy

should be continued in arrowroot, and the opium only given as occasion might require.

Aug. 1st. Mr. Rake met us. Matters were still favourable, though there had been some vomiting of fluid, which had a stercoraceous appearance; and, but for the absence of any concomitant symptom, we might have feared some impaction or gripping of intestine in the uterine rent.

Aug. 2d. The patient was going on well; she had no further vomiting. The dressings were removed, and the wound looked well. The strapping, lint, and bandage were reapplied.

I will not prolong the details of this case, which went on to recovery most favourably. The sutures were not removed for a week or ten days. The wound healed entirely by the first intention. The patient sat up in her room in three weeks, came down stairs, and went out of the house at the end of a month; from which time she resumed her household duties.

On September 9th following, Mr. Clifton fully reported this interesting and instructive case at a meeting of the South Hants Medico-Chirurgical Society; and in that month he sent the history of it, as embodied in his paper, to the Lancet. Its receipt was acknowledged in the "Notices to Correspondents." When a year had passed without its insertion in the pages of that journal, the editor was requested to curtail it as much as he liked, provided he would record the facts of the case. The non-compliance with this request has enabled me to publish it, with the interesting addenda of subsequent pregnancies, in the pages of the British Medical Journal.

1863. August 8th. As medical officer of the union in which Mary Ann Woods resides, I was summoned to attend her. She told me she had menstruated regularly from the previous November till May, but not since. She now had somewhat copious discharge, accompanied with intermitting and evidently uterine pain. Early on the morning of the 9th, she miscarried.

1864. June 11th. I again attended her in a miscarriage—on this occasion, with a four months' fœtus.

1865. June 6th. M. A. W. came under my care for some functional disorder, when I found, too, that she was more than seven, if not eight, months advanced in pregnancy; and that she had obtained an order for my attendance upon her during her labour.

July 23rd. At 2·30 a. m. I was called to her, and found that she had been three hours in labour. The os was dilated to about the size of a half-crown. The membranes were not ruptured. The head was presenting, but high up. The promontory of the sacrum advanced into the pelvic cavity rather more than one is accustomed to find it. By four o'clock the os uteri was fully dilated; and now the liquor amnii escaped, but the head was still above the pelvic brim. The expulsive pains were acquiring considerable force: and, as a precaution, I at once applied the long forceps to lessen the strain upon the uterine fibres by a little leverage coincident with the pains. I cannot say that I did more than just assist a natural delivery. At 6.30, a fine living male child was born. The placenta was expelled in a few minutes; and there has been nothing unusual to check a rapid convalescence.

In the Edinburgh Medical Journal of August, 1864, a case of gastrotomy for rupture of the uterus is reported by Dr. Crighton; it is fully recorded in Braithwaite's *Retrospect*, vol. 1. In the remarks, it is stated that "the statistics of Dr. Trask of New York give great encouragement in undertaking the apparently hopeless operation of gastrotomy in cases of rupture of the tuterus; yet, since the date of the publication of these (1856), I cannot find any successful case recorded as having occurred in Great Britain."

It is a question of interest in the case of M. A. Woods, had the administration of ergot anything to do with the rupture? From the length of time which elapsed after it was taken before the accident, we might say certainly not. It is hardly probable that a drug which generally produces its effect so promptly could have had its action so long delayed as to be in operation nine hours after its administration. If we want a cause, I think we have it in the combination of prominent promontory of sacrum and thin muscular fibre in an ill-fed, ill-nourished pauper, who eats animal food about once a year; the

uterus falling somewhat forward on this account, and the axis of its outlet disadvantageously placed. Her rapid recovery after the operation is in great degree due to two things—the saving of any addition to the shock already existing by the administration of chloroform; and the use of metal sutures, producing as they do less irritation than those of other material; and our ability, therefore, of keeping them longer in the body. Operations of recent date—ovariotomy in particular—have proved how much more may be done within the abdomen by the surgeon than had been formerly supposed, without the risk of peritonitis; which fact, strengthened as it is by such cases as the above, should encourage us at once to act on the occurrence of like mishap, holding out, as we may do, to the patient and her friends, strong grounds for hope of recovery; and, with a view to extend still further this encouragement, I lose no time in making known the above facts.—*Brit. Med. Journal. Sept.* 9, 1865, *p.* 252.

82.—ON THE MANAGEMENT OF THE PLACENTA IN NATURAL LABOUR.

By Dr. James Patterson, Partick.

[The principal writers on obstetrics within the present century may be classed somewhat loosely into two parties—namely, those who regard the expulsion of the placenta and membranes as of such importance that they do not intermit their care over the womb until this process has been completed; and those who leave all to nature within certain limits of time, unless some emergency occur calling for more prompt assistance.]

The plan of maintaining the contraction of the uterus until the birth of the placenta is not a mere theory, which has no value at the bed-side. The practice is very simple, being merely to keep up such an amount of stimulation of the uterus as shall cause it to close around the placenta, so that the latter shall be ready to be acted upon by every succeeding contraction of the womb. The moderate pressure of the hand, applied over the abdomen during the descent of the child, is nearly always sufficient at this stage. When the child is born, the hand must follow the descending womb, and grasp it, so gently as not to cause pain, and yet with a moderate firmness. This pressure ought scarcely to be relaxed during the separation of the child from the mother. In a majority of cases it will be found that this simple plan will have assisted the partial extrusion of the placenta through the os uteri, so that when the practitioner's hands are again free after tying the cord, he will find, on introducing two fingers, that part of the after-birth is lying in the vagina. At this stage, to maintain still the pressure on the womb with the left hand, and straighten, without pulling, the cord with the right, by means of the two fingers in the vagina, will in most cases suffice to procure the expulsion of the placenta in about four to six minutes. I call it expulsion; for it is not a withdrawal of the viscus, it is a true expulsion. When the placenta is thrown off in a short period, the womb is much less liable to relax than when it has been longer retained. I have been struck by the fact that where a child had been born some time, say twenty to thirty minutes before my arrival, the uterus can often be made to contract with great difficulty; and I have found in such cases, that with all my care, I cannot stimulate the uterus to throw off its contents in anything like the same time that I could have done had I been present before the birth of the child, and this, be it observed, after the womb had had a considerable interval of rest for its natural functions. A series of propositions may cause these facts to stand out more satisfactorily:—

1. When the uterus continues its contractions after the birth of the child, so as to throw off the placenta in a short period, the chance of hemorrhage is very slight.

2. When unassisted, the womb generally discontinues its efforts, for a time, after the birth of the child, and during this rest the placenta cannot be expelled.

3. Hemorrhage occurs during complete or partial separation of the placenta, and while that viscus is still within the uterus; and when it does supervene, it is generally in proportion to the non-contraction of the womb, and to the time during which the placenta remains unexpelled.

4. To assist the uterus to maintain its contractions for a short time after the birth of the child, to procure the separation and expulsion of the placenta, is a rational method of diminishing the risk of hemorrhage, while, at the same time, it is the surest way to secure the permanent contraction of the uterus, and thereby conduce to a speedy recovery.

5. The method recommended above is almost painless, is safe, and in very many instances is quite successful.

In 305 cases of labour, consecutive in this sense, that they include all natural cases where I was present before the birth of the child, and where the plan named was adopted, I noted accurately the time elapsing between birth of the child and of the placenta, and here state the results:

In	1	case	the placent came	with	the child.	
"	1	"	"	at the end of	1	minute.
"	34	cases	"	"	2	minutes.
"	83	"	"	"	3	"
"	63	"	"	"	4	"
"	66	"	"	"	5	"

The per centages were:—

In 2 minutes,	11·1
" 3 "	27·2
" 4 "	20·6
" 5 "	21·6

Of the 57 cases which exceeded 5 minutes, the time varied from 6 minutes to 40, but even including these, the average time from the birth of the child till the expulsion of the placenta was only 4·89 minutes. In giving these tabular notes of time, I would merely remind you that they are the results of a certain mode of practice, and not the first object to be sought in any plan of managing the placenta. The only case where hemorrhage occurred was in one where the placenta had been rapidly expelled, and where a portion of the membranes had remained behind.

I need hardly mention that in speaking of the placenta I include the membranes; nor is it necessary to do more than allude to the extreme importance of making sure that *every* portion of the whole is thrown off.—*Glasgow Medical Journal, July*, 1865, *p.* 129.

83.—ON ELECTRO-MAGNETISM IN POST-PARTUM HEMORRHAGE.

By Daniel Walter Parsons, Esq., Liverpool.

[The patient was 27 years of age. She was delivered of twins, the labour was natural, but it was necessary to remove the placenta by the hand, owing to adhesion. This was preceded and followed by a good deal of flooding. A binder was firmly applied, and cold to the vulva, but still hemorrhage continued.]

Notwithstanding all his efforts to arrest the hemorrhage, Mr. Burrows saw that the patient was sinking rapidly. He tried ergot without producing any uterine contraction. He then removed the binder and grasped the uterus, directing the attendants to administer brandy freely. He tried the cold douche; but still the hemorrhage continued. He then made pressure upon the abdominal aorta, and for the first time he observed a beneficial result; the flooding was arrested. He kept up the pressure for an hour. I then arrived (at half-past three o'clock), and was shocked at the sight which met my gaze on entering the room. The patient, anæmic, cold, almost pulseless,

was lying in a pool of blood. There was only a slight draining from the uterine sinuses. I felt the uterus through the abdominal walls; it was large, flabby, and felt empty. I passed one hand into the cavity and removed a few small clots, at the same time keeping up pressure with the other hand. Finding that neither pressure nor the irritation of the hand in the uterus excited any muscular contraction, it occurred to me that electro-magnetism was our *dernier ressort*. I relieved Mr. Burrows in keeping up the pressure on the abdominal aorta, and directed him to drive to my house for my electro-magnetic apparatus. He arrived in a few minutes, and I proceeded without delay to apply one pole inside the uterus, while the other was being applied externally over the abdominal walls. The lowest power was first used, and then gradually increased to a medium. In a few minutes I felt slight contractions, and after continuing the current for half an hour I was enabled to grasp the whole of the uterus in my hand. I retained it in this manner for better than half an hour, and then applied a tight binder and pad. After clearing away as much of the *débris* as possible, Mrs. B. became quite conscious, and complained of being cold. I gave some brandy and hot water, and, fearing to continue hot drinks, I put extra clothing over her and directed a female to lie beside her. When she became warm she fell into a doze. I left her for a short time now, five o'clock.

At six o'clock very sick; skin warm; pulse 140; complained of binder being very tight. Ordered chloric ether and spt. ammon. co. every half hour; the binder to remain. No flooding.

Half-past nine.. Improving in every respect. Ordered the mixture every hour; cold beef-water for drink.

Mrs. B. continued to improve daily, and was afterwards enabled to nurse both children. She is now, August 16, 1865, as florid and healthy-looking as ever.

In this case the patient owed her life, in the first place, to the pressure which was maintained so persistently on the abdominal aorta, and most assuredly also to electro-magnetism. Had transfusion been used here, the vital fluid would have permeated the system only to find an exit through the patent uterine sinuses. I have used the electro-magnetic fluid in one other similar case with the like beneficial result.—*Med. Times and Gazette, July 26, 1865, p. 243.*

84.—REMARKABLE CASE OF DEFORMITY.

By James Paget, Esq.

The following remarkable instance was related by Mr. Paget as he went round the wards, as having taken place under his own observation:—A person when pregnant with her first child chanced to shake hands with a man, who by an accident some years previously had lost the middle fingers of the right hand, the index and little fingers, from long practice in grasping objects, having afterwards fallen almost into the shape of a lobster's claw. Being a nervous and very sensitive woman, she was startled and distressed when she became aware of the condition of the hand, and for several days she could not dismiss the recollection of it from her mind. When her child was born, it was found that the middle fingers of both hands, and the middle toes of both feet, were absent. After an interval of five years, during which she had had four perfectly formed children, and, being now pregnant with a fifth, she again encountered the man, and was again obliged to shake hands. She was so much disturbed by this occurrence, and so impressed with the idea that the child would be deformed, that in the evening she wrote down the event and her conviction regarding the child. When the child was born it was deformed exactly as the first had been.—*Medical Times and Gazette, Sept. 23, 1865, p. 333.*

85.—ON HYSTEROTOMY.

By T. Spencer Wells, Esq.

[This article is a comment on Dr. Marion Sims' mode of incising the os and cervix uteri. Mr. Spencer Wells considers that it requires careful consideration before we adopt it in this country. Mr. Spencer Wells then offers the following remarks on the subject.]

1. The *use of the speculum* is advocated by Dr. Sims because he never selects a plan of operating by the touch alone and in the dark "if it be possible to aid the manipulatory process by the sight." And he asks, if any of us wanted to cut off part of a long uvula, "would it be judicious to run one finger down the throat and guide by it some machine for performing the operation in the dark? or would it be more surgical and more precise to look into the throat, seize the part with a proper appliance, and amputate it where our judgment would determine to be right and best?" There is something so captivating in this style of argument that we may be led to assent at once, and overlook the essentially different nature of the two cases. It would be impossible to feel the uvula with a finger, and guide an instrument for cutting part away, without exciting such convulsive spasms of the fauces as would mar the success of the operation. There is no such difficulty encountered in performing any operation on the uterus "by the touch alone and in the dark." Nor is there any sort of objection to looking down a woman's throat. But it makes an immense difference in the sufferings of a modest woman if an operation can be done under the bed-clothes without any exposure, or if the speculum has to be used, and held, as Dr. Sims directs, by an assistant, the patient "having been, perhaps, *eight or nine minutes* (!) on the table." But I would go further than this, and say to any one who regards the fears of his patients little and their modesty still less, that the operation can be much better done with a proper instrument in a second or two by the touch alone than it can be by the complex process of speculum, assistant, hook, scissors, knife, and plugs, as advised by Dr. Sims, and I say this after trying both methods.

2. The *extent of incision* recommended by Dr. Sims is, in my opinion, too great, unnecessary, and the cause of the bleeding which leads to such precautions in his practice. The bleeding, he says, is "usually unimportant, but sometimes it is *furious*." He divides the whole of the vaginal portion of the cervix quite through on both sides with scissors, and then with a knife carries on the scissor-cuts on each side "up to the very cavity of the womb." This is a far freer incision of the cervix than that recommended by Dr. Simpson, and even his incisions appear to me to be unnecessarily free. I have seen many women in whom, after such complete incision, the vaginal portion of the cervix has curled up and almost disappeared, while the remains of the cervical canal or the os internum have become nearly impervious.

Yet, putting aside the cases where we incise the cervix on account of fibroid tumours, I believe all the good that can be gained by hysterotomy in the cure of uterine flexions and of obstructive dysmenorrhœa and the sterility which is so often associated with these conditions, is certainly gained by an incision which only passes through the mucous membrane and the innermost layer of muscular fibres. I believe it to be not only unnecessary, but dangerous and injurious, to cut into the thicker middle layer of muscular fibres —containing as it does large vessels, especially veins—and *a fortiori* into the outer layer which is so intimately connected with the peritoneum. Indeed, I cannot see any possible object that can be attained by making a freer incision than is necessary to admit a No. 8 catheter easily into the uterine cavity and to imitate the transverse fissure into which the circular os of the virgin is converted in a woman who has borne a child. By the small incision chronic congestion of the mucous membrane and follicles of the cervix is relieved, and any existing stricture is divided as well as the circular fibres immediately beneath the mucous membrane called by Kölliker the *sphincter*

uteri; while all important blood-vessels are avoided. I have incised the cervix very often, and I only remember one case in which there was any bleeding of consequence. In this patient I cut quite through the cervix as recommended by Dr. Sims, and plugged with cotton; but there was a dangerous recurrence of bleeding when the plug came away forty-eight hours afterwards. I have never seen a peri-uterine abscess in my own practice, but I have seen two fatal cases after free incision in the practice of others; and I have seen so many cases in which the cure of obstructive dysmenorrhœa, of obstinate flexions, and of sterility has followed the small incision, that I am confident very free incisions are as unnecessary as they are dangerous.—*Lancet, May* 27, 1865, *p.* 578.

86.—ON PUERPERAL FEVER.

By Dr. T. Snow Beck, F. R. S., London.

[There can be no doubt that one of the principal objects in the prevention of puerperal fever is to procure a complete and permanent contraction of the uterus after the birth of the child and the expulsion of the placenta. By many writers on the subject, this point is very slightly insisted upon.]

For some years past, I have carefully observed the state of the uterus immediately after childbirth, considered as a means of obviating that enlargement of the organ which too frequently follows childbirth or abortions, as well as preventing many of the ills to which parturient women are liable. And I am firmly convinced that many of the dangers and evils which women suffer after delivery may be prevented by procuring a complete and persistent contraction of the organ. It would appear that, when the vessels are at all pervious to the circulation of blood within them, the presence of this fluid interferes with those changes which are necessary to reduce this organ to its condition previous to impregnation; and, to this end, I have been led by experience to administer the ergot of rye in all cases after the removal of the placenta, even if its employment was not indicated before this period of the labour. The combination I have found most useful has been that of the tincture of Indian hemp, powdered ergot of rye, and borax, mixed with some stimulant and aromatic. One to four doses of this mixture has been given at one or two hours' interval, according to circumstances, and with the best of results; for, although they occasion a considerable amount of after-pains, yet the comfort afterwards experienced and the pleasing convalescence more than compensate for the pains induced.

I believe it is generally considered that, when the uterus is so contracted as to prevent any further hemorrhage, this is sufficient to ensure the safety and well-being of the woman. But I have found that it is necessary to carry the contraction beyond this point before we can consider that we have ensured the safety, much less the well-doing, of the parturient woman; and I have been led to think that this arises in many instances from a relaxation of the muscular tissue subsequent to the removal of the placenta, and after it has been fully contracted.

If these views be confirmed by the experience of others, the treatment of women immediately after delivery appears of much more importance than has been commonly supposed; and this extends to the diet, as well as to other matters. It would appear, by reason of a general belief that puerperal fever and other diseases of childbed women were purely inflammatory affections, which required to be treated by large bleedings and means calculated to prevent or reduce inflammation, that women during the puerperum were kept on a low and depressing diet. But these opinions are now generally considered to have been erroneous; and that, instead of keeping the female to a low and depressing diet, she requires a good diet and nourishing support, in order to relieve her from the physical fatigue and mental anxiety of the labour, to restore her health from the waste occasioned by the previous months of pregnancy, and to enable her to pass through those subsequent changes which have yet to take place. To this end, it would appear that a liberal diet is

necessary, and, in some cases, even the addition of a stimulant. Of course, the regulation of these matters depends much upon the previous habits and state of health of the woman, as well as upon the season of the year and other incidental circumstances; but I am convinced there is not that danger in a liberal supporting diet which was at one time so generally believed, and that a stimulant similar to the "caudle" of old may frequently be given with much advantage. This appears to be confirmed by the remark of Dr. Ferguson in his valuable essay. "The late matron of the hospital," he observes, "Mrs. Wright, remarked the effect of the hospital diet on the patients as often very depressing; and knowing, from the habits of this class of people, that gin was to many of them the common substitute for meat, she was induced to change the hospital dietary for a caudle in which gin was the staple. *We shortly after this had a marked diminution in the intensity of the epidemic.*"

The principles of treatment, which the review of these cases would suggest, would then appear to be: (1) to prevent the further injurious impregnation of the system, either by obstructing the further flow along the uterine sinuses, or by removing the noxious fluids from the interior of the uterus; (2) by supporting the system during the struggle in which it is engaged, and by meeting any accidental complication which may present itself; and (3) perhaps a further source of treatment is now afforded which may enable us to counteract, to some extent at least, the deleterious impregnation which has already taken place.

1. The metamorphosis or disintegration which takes place in the muscular tissue of the uterus after child-birth prior to its removal does not, it is said, commence before the fourth or sixth day; and, as the symptoms of injurious impregnation usually show themselves on the second or third day, it is not improbable that means might still be found to induce that further small amount of contraction in the organ which is necessary to close effectually the uterine sinuses. A continuous current of electricity, of low intensity, might have this effect. But I fear the other means already adverted to, do not afford much chance of success; for it is undeniable that a truly efficient agent to induce the permanent contraction of the uterus is still a great desideratum.

The removal of the noxious fluids from the interior of the uterus appears to afford an important means of treatment, and to furnish one of the objects mentioned by Cruveilhier. "I am certain that diseases resulting from purulent infection would not be stamped with the seal of incurability, and that nature, seconded by art, would triumph in the majority of cases, if the pus, which is incessantly renewed, did not incessantly renew the sources of infection." But, in using these means, the remarks of Dr. Denham, though written so long ago, should be borne in remembrance: "I have seldom attempted to inject medicines of any kind into the vagina and uterus; though, from a consideration of the state of these parts, and of the fœtid humours discharged from them, it is reasonable to expect that emollient or gentle detergent injections might be useful. However, if these are advised, there should be great caution both in the composing and administering of them."

On this subject, M. Piorry remarked, in the discussion on the treatment of puerperal fever before the Imperial Academy of Medicine at Paris in 1858: "It is indispensable to cleanse, by means of injections carefully employed, the uterine cavity from blood and putrid sanies which it contains. During five years I have not seen one female perish who has been attacked with puerperal peritonitis; and, in all women who have been confined, uterine injections have been carefully employed." (*Bullet. de l'Acad. Impériale de Méd.*, vol. xxiii., p. 461.) M. Hervez de Chégoin also observed: "The injections of the uterus, of which I have spoken, ought to be placed in the first rank (in the treatment); and, as I have said, their efficacy depends upon their proper use—that is to say, the care of the physician to seize the suitable moment. It ought to be directly upon the occurrence of the first indication, so slight and so important, and upon which I have fixed the attention."

The substances which offer the greatest probability of success are a weak solution of the sulphites or hyposulphites in tepid water. A long elastic tube

may be passed to the fundus of the uterine cavity, and the interior of the organ carefully washed out with a weak solution of sulphite or hyposulphite of soda, by means of the ordinary enema apparatus: care being taken that no force be employed, and that all air be excluded from the apparatus prior to its introduction, by passing the fluid through it several times. The noxious fluids may thus, to a great extent, be removed; and, should any of the injected fluid gain entrance into the uterine sinuses, it will probably be more beneficial than injurious. This washing out of the uterine cavity may be repeated each day, or at shorter intervals, according to the circumstances of the particular case.

2. The means to support the system during the struggle, and to meet any accidental complication, are too well known to require any remarks.

3. As Cruveilhier observed, after detailing the various means employed to combat purulent infection, "all these means have failed as signally in my hands as in those of others." But, since that period, the researches of Dr. Polli of Milan, and of Dr. Mariano Semmola, upon the action of the sulphites, have rendered it probable that these agents may be found of much value in the treatment of injurious impregnation of the system, even after this has taken place. Both these observers concur in the opinion that these agents possess a remarkable power in putrid infections of the system, when they are not the result of specific disease. They are given in divided doses, to the amount of from three to four drachms in the twenty-four hours; and the sulphites of magnesia or lime would appear to be the most suitable for internal administration. I have given lately these remedies in some cases, in doses of one scruple to one drachm every two or three hours, with encouraging results; but my experience of them is too limited to admit of any opinion. —*British Medical Journal, May* 13, 1865, *p.* 478.

87.—ON PUERPERAL FEVER.

By Dr. Robert Barnes, Fellow and Examiner in Midwifery, Royal College of Physicians, &c.

There is a condition of the blood, resulting partly from pregnancy and partly from puerpery, of which the chief feature is excess of fibrin. The blood when in this condition shows a remarkable tendency to coagulate in the vessels. The occurrence of what is called "phlegmasia dolens" is due to this condition. The febrile states associated with it I propose to call "Thrombotic puerperal fever." In their simple essence they are very distinct from the other leading types of child-bed fever; and frequently—perhaps more frequently than most other forms—the thrombotic variety occurs without any prominent complication: it therefore admits of clear definition. The foundation is undoubtedly laid during pregnancy. Perhaps it is, after all, only a sub-variety of the "excretory puerperal fever." Regarding the fibrin, after Mr. Simon, as an excrement, we must conclude that hyperinosis is the result of failure of the excretory apparatus. Now when there is excess of fibrin, there exists a proclivity to separation of it from the blood-stream. It is liable to be caught and deposited on any points where the general smoothness of the lining membrane of the vessels is broken: for example, on the valves of the heart. It is also subject to clot spontaneously wherever the blood-channel is unusually tortuous, where gravitation has to be overcome, where the vessels are exposed to compression, and the stream is consequently slow. Acute rheumatism—a disease which presents many instructive points of comparison with puerperal fever—is especially prone to precipitate fibrin upon the left cardiac valves. This is a form of *arterial thrombosis*. Probably the presence of a second morbid material is necessary to cause the fibrinous precipitation. The lactic acid of rheumatic blood may favour this process. In the great majority of puerperal cases the *thrombosis* is *venous*. And although clotting not seldom takes place apart from the obvious action of a second morbid material, in many cases it is highly probable that such a cause comes into operation. For example: a woman may

be going on smoothly up to ten or twelve days after labour, when some untoward accident suddenly disturbs the precarious calm—exposure to a draught, a severe mental shock or emotion, checks excretion, and a morbid material is thrown back into the blood. Within twelve hours of that accident pain is felt in the groin or calf, the pulse quickens, the skin is hot and dry, and the leg begins to swell. There is venous thrombosis. It is fortunate indeed that puerperal thrombosis is almost always venous, and so rarely arterial. The latter is, I believe, mostly fatal; the former generally ends in recovery. Why is it that arterial thrombosis is rare, and venous frequent? The explanation appears to me to be this:—A certain degree of retardation of the blood-stream offers the most favourable condition for that reaction which determines the precipitation of fibrin in the living vessels. Now the arterial blood not only moves more quickly, but it is not liable to local interruptions as is the blood in the veins; and it is also purer. Still the blood will sometimes coagulate in the arteries of the upper and lower extremities and of the brain. When it does so there is little time for the opening-up of collateral thoroughfares: the danger of gangrene or of paralysis and death is imminent.

More frequent than the thrombosis of arterial blood is thrombosis of the venous blood carried into the pulmonary arteries. As Virchow has shown, this is frequently capillary. It may even in some cases start from embolism—that is, from plugging of the pulmonic capillaries or smaller vessels by minute fragments of clot brought from a distance.

Whatsoever its origin, it is a very formidable affection. It explains a large proportion of the cases of sudden death that occur during the latter months of pregnancy, at the time of labour and in child-bed.

Less unfortunate is it when the obstruction takes place in the veins of the limbs. In this case no vital organ is concerned; there is generally time for secondary branches to dilate, and to compensate to a great extent for the incapacity of the trunks. The great majority of cases of phlegmasia dolens recover. The fact is, that although the blood is in a pathological condition, still the departure from health is not so great as it is in most other forms of puerperal fever. In the more fatal kinds—for instance, the septicæmic—one source of danger is the want of that very property, plasticity, which is here in excess. The empoisoned and degraded blood is carried to every tissue, every molecule of the body, producing fever and extensive phlegmasiæ of low type. The occurrence of thrombosis may therefore be regarded with satisfaction, as indicating a state of blood not apt to produce the severer forms of peritonitis, metroperitonitis, or suppurative phlegmasiæ in the limbs.

The peculiar character of the swelling, the tenseness, the indisposition to pit, may be accounted for partly by the increased proportion of fibrin in the serum effused, coming as this does from hyperinotic blood, and by the suddenness of the effusion.

Such is the view of simple thrombotic puerperal fever or phlegmasia dolens which appears to me the most consistent with clinical observation. Loss of blood during labour greatly predisposes to this affection. It occurs frequently in pluriparæ. It is very apt to attack the same woman in successive labours—that is, it is prone to arise in those who have already venectasis of the lower extremities, or a marked disposition to this state.

A few words as to the issues and treatment. Of late years the danger of detachment of portions of the vein-clots, and of these being carried to the heart and pulmonary arteries, has been recognised. I have endeavored elsewhere to collect what is known upon Puerperal Thrombosis and Embolia. Dr. Willoughby Wade has also added an interesting illustration of this subject. I do not think the danger of embolism is great in the pure class of cases of crural thrombosis. Still the risk is serious enough to serve as a caution against premature bodily exertion, or resort to friction of the affected limbs. Either may promote detachment of clot. But the risk of embolism increases when the thrombosis is complicated with marked blood-dyscrasia. Then there is a tendency to rapid disintegration of the clot, and fragments

large enough to be arrested in the pulmonary capillaries may be carried to the heart. In the cases thus complicated, however, the toxæmic character so predominates that I prefer to consider them under the distinctive head of septicæmic puerperal fever.

The treatment may be discussed under the various heads: prophylactic, local, general, and restorative. The sketch I have given in previous lectures of the causes which promote adulteration of the blood in pregnancy and puerpery will serve to indicate what is to be done with the view of counteracting these causes. Local treatment is required in order to relieve the swelling and pain. The leg must be kept slightly raised on an inclined plane, and flexed inwards, so as to take off tension from the affected vessels. Leeching in the seat of the pain and swelling is commonly of great service, especially during the acute inflammatory stage. The limb should be wrapped up in cotton-wool surrounded with oil-silk. This promotes local and general diaphoresis. After the subsidence of inflammation, blisters in the vicinity of the thrombosed veins are very useful. When absorption is resumed, flannel-rollers should replace the cotton-wool. The indication to give external support to the weakened vessels is obvious.

As more or less paralysis always attends, can anything be done to help the recovery of the power of motion? After the acute stage, frictions with oil or camphor liniment are useful; but the friction should be very gentle, and not prolonged. There may be danger of breaking up a clot, and so giving rise to embolism. Galvanism may be tried. It is unwise early to press the patient to get up and exert herself to walk; when the time for this arrives, it will mostly be well for her to wear elastic stockings. The general treatment that appears to me the most successful consists in the administration of salines with sedatives in the early or inflammatory stage; then bark or ammonia, and later, iron. Generous diet is called for. Chicken, game, mutton, if the patient can take solid food, as she generally can after the first few days, should be prescribed. Wine, either port or good claret, is necessary. It is desirable here, as in all other forms of puerperal fever, to keep the circulating system well supplied with generous aliment. If this be done, the door is greatly closed against the entry of noxious matters.

Recovery is always tedious. The duration of phlegmasia dolens may extend to four or even to eight or ten weeks. To indulge in the hope of much accelerating recovery might, by suggesting active measures, lead to serious mischief. It is eminently a case for wise faith in the restorative work of Rest and Time. It is doubtful, indeed, whether veins which have undergone the distension and tortuosity consequent upon phlegmasia dolens ever perfectly regain their original soundness. Some degree of venectasis commonly persists. The parts so affected are liable at periods remote from the labour, indeed long after convalescence and presumed recovery, to become the seat of secondary thrombosis. In a minor degree the phenomena of phlegmasia dolens may be reproduced. Inflammation, sometimes erysipelatoid, springs up around the knotted and thrombosed veins. Febrile reaction ensues. And sometimes sloughing takes place over the seat of the obstructed vessel, and a deep, indolent ulcer is formed. A process strictly of necrosis has occurred as the consequence of arrested circulation and nutrition in the part. The predisposing cause of this secondary series of events is manifestly the loss of the physical integrity of the veins. But I believe an exciting cause is often supplied by the accidental complication of a degraded or adulterated condition of the blood favourable to coagulation.—*Lancet, Sept.* 30, 1865, *p.* 367.

88.—ON PUERPERAL TETANUS.

By Dr. George Hare Philipson, M.A., Physician to the Newcastle-upon-Tyne Dispensary and Fever Hospital, &c.

[At a varying time subsequent to the completion of the process of parturition, delirium, or even convulsions may occur, and occasionally tetanic symp-

toms. Spasm of the muscles of mastication is not unfrequent; but rigidity of the whole body is very uncommon. The following well-marked example, however, has occurred in the practice of a friend of the writer.]

On the 19th of February, 1865, Mrs. B., aged 39, was attended in her third confinement by Mr. Thompson. The presentation was natural, delivery being easily completed. The placenta was adherent, necessitating detachment, in the accomplishment of which a rather large amount of blood was lost. The advance towards convalescence was rapid; for on the eighth day she was able to leave her bed, and on the ninth to engage in her household duties. On the 1st of March, ten days after delivery, some difficulty was experienced in opening the mouth, and in turning the head from side to side. As she had been sitting near a window imperfect in obstructing the external air, the symptoms were attributed to the effect of cold, and were not regarded with much concern. The parts were well fomented, and a stimulant application applied.

By degrees the rigidity increased; and, on the morning of the 5th of March, the mouth became closed, and I was requested in consultation. The patient was in bed, on her left side, inclined to the prone position, with the head supported towards the chest by a pillow placed behind the occiput. When undisturbed, she was free from pain; but, on the least movement, felt oppression at the chest and difficulty in breathing. She had obtained sleep during the previous night. The surface of the body was cold, and bathed in perspiration. The pulse was 100, small, regular, and easily compressed. The jaws were immoveably fixed, the upper teeth slightly covering the lower. The temporal and masseter muscles were firmly contracted and hard. On being raised up, the head was forcibly drawn backwards, and instantly she complained of pain, and implored to be laid down again. The muscles attached to the sternum and scapulæ were rigid; those of the abdomen and extremities were perfectly flaccid. The fundus of the uterus was able to be felt just above the brim of the pelvis; but no pain was experienced on deep pressure. The lochial discharge was slight, and not offensive; the secretion of milk was small; the urine was natural, and had been voided without difficulty; the bowels had not been moved for two days. A pill of calomel and croton oil was introduced behind the molar teeth; and, two hours later, an enema of turpentine and croton oil administered. Wine, beef-tea, and milk, at short intervals, were also ordered.

When visited the same evening, she was still lying on the left side, more prone, and with the thighs flexed on the abdomen. The pulse was 90, and quite regular. Since the forenoon, fully a pint of beef-tea and three glasses of wine had been swallowed. After the injection the bowels acted freely, the urine being passed at the same time. The stools were very offensive; but did not contain any joints of tapeworm. During the afternoon, she was thrice severely convulsed; in the words of her husband, "jerked upon the bed." Deglutition was difficult, respiration impaired, and the lips slightly turgid. The jaws were unable to be moved or separated, even in the smallest degree. The muscles were more rigid, especially those of the back. The croton oil was repeated; and a blister applied to the nape of the neck.

About 12 p. m., she was again severely convulsed, and died very shortly afterwards. Her friends would not allow an examination of the body.

The convulsive spasm and the tonic contraction were well marked. At first, the muscles of mastication were alone influenced; but during the last day of life, the muscular system generally became affected; rendered evident by the difficulty in respiration and deglutition.

The entire absence of symptoms directly connected with the nervous centres render it highly probable that the irritation was eccentric in its source. Of such causes of idiopathic tetanus, irritation of the intestinal canal from tapeworm is by far the most common. In this case, no such evidence was obtained. Hence, it may be supposed, that the nervous excitement was the result of uterine irritation, perhaps caused by a portion of the placenta having remained adherent to the uterine wall. Thus, a close resemblance to

ordinary traumatic tetanus may be recognised.—*British Medical Journal, Sept.* 2, 1865, *p.* 232.

89.—CASE OF PUERPERAL CONVULSIONS SUCCESSFULLY TREATED BY THE INHALATION OF CHLOROFORM.

By Dr. Jas. Cuthill, Parochial Surgeon, Denny and Dunipace.

[The patient was a primipara, and aged 19 years. She had œdema of the feet and legs, and complained of great pain in the region of the vulva, the labia being hot, swollen, and painful to the touch.]

At 6 p. m. on the 9th, I got an urgent summons to attend my patient, which I immediately obeyed. In not more than five minutes I was at her bedside, in time to witness the onset of a tremendous convulsive attack. When the spasms reached their height, which they did in about two minutes, the spectacle presented was truly frightful. There was complete fixation of the respiratory muscles, such as I once witnessed in a dog poisoned by strychnine; the face was literally black and hideously distorted; bloody foam issuing from the mouth; whilst so complete was the opisthotonos, that the body bent like a bow, rested on the heels and occiput, producing such curvature and tension of the distended abdomen as to threaten rupture of the uterus. I at once sent off for chloroform, but before the messenger arrived, which was in about ten minutes, a third attack, equally severe, came on—the one I describe having been the second. I immediately put her under the influence of the chloroform, administered from a folded towel, and by periodical re-applications kept up the anæsthesia for three hours, all the while there being no return of the spasms. In the interim I had ascertained that there was no dilatation of the os, or other symptoms of labour. However, as the case seemed likely to prove a serious one, I desired a consultation with Dr. Hamilton, of Falkirk, who arrived at 9 p. m. The inhalation was now stopped, and for three quarters of an hour there was no return of the convulsions, when a comparatively mild attack supervened. It was agreed to keep up the anæsthesia at intervals: at the same time, if any symptoms of labour occurred, or if the tendency to the recurrence of the attacks remained persistent, to puncture the membranes and hasten on the labour, as the only way of radically removing the primary cause of the ailment. I waited with my patient the whole night, mild convulsive attacks recurring at irregular intervals, which I as regularly combated by repeated exhibition of chloroform. At 10 a. m. on the 10th, indications of labour pains came on, and examination detected dilatation of the os uteri, to the size of a shilling. As the tendency to the convulsions still continued, I punctured the membranes with a silver catheter. The labour-pains gradually increased in frequency and intensity, until at 11.30 they had acquired a regular bearing-down character, and the patient began to complain of pain in the back. No more chloroform was given, and as the labour-pains increased in severity, the convulsions did not return. At 12 the first stage was completed, and at 12.30 p. m., as the patient seemed worn out and exhausted, I applied the forceps, and assisted the natural expulsive efforts, until at 1.10 p. m. she was delivered of a son. The placenta was expelled an hour afterwards. The patient remained in a dreamy confused state for three days, but her recovery was rapid, and unaccompanied by a single bad symptom. On the 20th I found her in her room thoroughly convalescent, and with girlish pride trimming a cap for her baby, in thorough oblivion of all her past troubles.

The baby, although rather small for a boy, is lively, and seems likely to to do well. I may also remark that the patient's urine throughout was normal in quantity and specific gravity, and showed no trace of albumen. The propriety of giving a purgative was discussed, with the view of removing any intestinal irritation, but as the bowels had been regular before her illness, another dose of castor-oil was only given on the day after her delivery.

The prompt and decisive way in which the convulsive attacks were controlled by the chloroform justly suggests the possible impropriety of, under

any circumstances, resorting to bleeding, tartar emetic, opiates, and the other remedies recommended by the older writers. It at the same time shows the tolerance of anæsthetics under such circumstances, twelve ounces of chloroform having been expended in the case from first to last.

The propriety of the induction of labour is also an important question. As the fœtus, having only arrived at the eighth month, was most probably not viable, and as there were no symptoms of labour, interference would have been injudicious, but when labour did supervene, with the os in a dilatable condition, the course pursued seems to have been a proper one.—*Edinburgh Med. Journal, June*, 1865, *p.* 1105.

90.—CASE OF PUERPERAL TETANUS, FOLLOWING ABORTION AND PLUGGING OF THE VAGINA.

By Joseph Blackshaw, Esq., Stockport.

[The patient was 48 years of age, and of a highly nervous temperament. It was necessary to plug the vagina, in consequence of profuse hemorrhage from threatened abortion. She rallied in the course of the following day, and recovered nicely.]

On the day following (Tuesday), just ten days from my first visit, I was unexpectedly requested to see her. She thought she had taken cold, and was then complaining of great stiffness of the deep-seated muscles of the neck and throat, with difficulty of swallowing, and was unable to open her mouth perfectly. As there was some febrile excitement, she was ordered to remain in bed, and to take a saline mixture with an aperient; to use hot fomentations; afterwards hot moist bran; to steam the fauces; and, if possible, to use a gargle.

On the two following days, Wednesday and Thursday, the painful tension of the masseters, as well as the muscles of the neck and throat, had so greatly increased as to prevent deglutition and bring on a state of perfect trismus. On Thursday afternoon and evening, the tetanic seizures became increasingly frequent, and producing great muscular rigidity, contortion of features, and slight opisthotonos. During the paroxysm, the pulse was small and feeble; but the consciousness was entire throughout. She continued in this state until Saturday evening, the tetanic spasms and opisthotonos gradually becoming more severe, when she died from exhaustion, five days from the first setting in of the tetanic symptoms.

Owing to the clenched state of the jaws, little was done in the way of treatment, beyond a little counter-irritation to the spine; the administering of a turpentine enema; and the removal of fæcal accumulations, which might prove a possible source of spinal irritation.

Puerperal tetanus is a very rare species of that disease in this climate; and this must be my apology for this communication. I have been in practice in this town for more than thirty years, the greater part of which I have been medical officer to a large district of a Poor-law union, including the union workhouse, and must have attended three or four thousands of women in labour at different periods of utero-gestation; and this is the first case that has occurred in my public or private practice, or, so far as I can learn, in that of the oldest obstetrician either here or in Manchester, with the exception of one case mentioned by Dr. Whitehead. Dr. Radford considers it a rare disease, but has known it to occur. Mr. Roberton, of the same city, also says that obstetric tetanus is a novelty to him, and that he has never seen it during a long and laborious practice. Nor is the disease one usually recognised in treatises on midwifery and the diseases of lying-in women. The extreme rarity and infrequency of this disease, notwithstanding the various lesions from the application of instruments, manual interference, turning, and other violence that the uterus must often sustain in protracted and difficult labours, points to a very different state of the nervous system from that which gives rise to traumatic tetanus in the various external inju-.

ries to which the body is subjected. Physiologists attribute this to the uterus receiving its nerves from the great sympathetic. It may be so. But of what that peculiarity of the nervous system consists in these cases, where the irritation from the uterus, as in the case of Mrs. H., is propagated to the cerebro-spinal system, is still shrouded in mystery. We can only say that, in the case of my patient, cold applied to the body, previously lowered by mental anxiety and loss of blood, may have given rise to a state of reflex spinal irritation, followed by tetanus; though probably, in ten thousand other instances, the same exciting causes would produce no such effects.

I regret that chloroform was not tried, as, besides the good effects it is said to have in convulsive diseases, it presents facilities of application where no internal remedy can be given.—*British Medical Journal, Sept.* 9, 1865, *p.* 252.

91.—ON DYSMENORRHŒA.

By Dr. KIDD. (Abstract of a paper read before the Obstetrical Society of Dublin.)

Dr. Kidd read a paper on this subject, premising that painful menstruation was a symptom common to so many affections, that to discuss the subject fully would be to enter on the consideration of the whole range of uterine and ovarian pathology. In fact dysmenorrhœa was not a disease *per se*, but a symptom, which might arise from one of several causes, and as such no more entitled to a place in our nosological list than (for instance) dropsy; but as dropsy was often the most prominent symptom, and one of which it is sometimes difficult to ascertain the cause, so dysmenorrhœa was very often the symptom which led the patient first to seek the aid of the physician, and on examination it was either found to arise from some organic derangement of which the sufferer was previously ignorant, or it may be impossible to detect any definite cause for its production. Cases of this last description are, however, becoming more rare, and there are good grounds for expecting that, as our knowledge of uterine diseases becomes more extended, we shall be able to trace all cases of painful menstruation to their cause, and, by directing our treatment to the removal of this, make it more successful than it can claim to be at present.

I do not, then, mean to attempt a full exposition of the causes of dysmenorrhœa, but rather to relate briefly some of the more typical of the cases, of which I have records in my note-book, with such observations as they may suggest.

Case 1.—The first case I shall mention is that of a young English lady who came under my care in 1860, while on a visit with some relatives in Dublin. When I first visited her I found her in great pain: her face livid and dusky, her features contracted, surface of body cold, pulse small and quick. She lay writhing in bed, straining herself with ineffectual attempts to vomit, and every other minute dashing off the bedclothes and throwing herself from side to side in paroxysms of pain. In this state she continued several hours, notwithstanding the use of opiate enemata, hot fomentations, the exhibition of opiates and stimulants by the mouth, and ultimately the inhalation of chloroform; but this latter, I must say, I did not push to its full physiological action, being deterred by the state of the circulation and the colour of the face. I learned from her at my visit the following day that she had been in the habit of suffering in this way at each menstrual period for some years past, though not generally to the same degree as on this occasion. When she first began to menstruate she felt no inordinate pain; but about three or four years after its commencement, and four or five before I saw her, she got a chill one day while menstruation was going on, by which it was suddenly suppressed. At the next menstrual period she felt some pain, and the discharge was so excessive that medical advice had to be obtained, and for a period of ten months after this she did not menstruate at all. After this

period the function was re-established, but its recurrence was ever afterwards attended with pain, although during the intervals she supposed herself to be in good health. By the use of warm hip baths, counter irritation over the iliac regions, leeching at the arms, tonics and aperients during the intervals, and the early use of opiates when the pain set in, the three or four succeeding catamenial periods passed without much suffering. She then returned to England, but her sufferings soon became as severe as ever.

I next saw this lady in November, 1862. She had then been married some months, and had come to reside in Dublin. The pain was now worse than before, her general health was suffering, and there was a good deal of muco-purulent discharge during the intervals, often of a brownish red colour, as if mixed with blood. She then suffered considerable pain at about the middle of the intervals between the periods, but without menstrual discharge. I now made a digital examination, when I found the uterus low in the pelvis, soft, large, and retroflexed. The os was so large as to admit the entrance of my fore-finger. On introducing a speculum I found the lips greatly congested and swollen, so as to evert the lining membrane of the cervix, and appearing to be so softened that the finger might be with ease pushed into their substance. From the interior of the cervix a long string of clear ropy mucus hung down into the vagina, and on attempting to remove this the surface was very easily made bleed. The uterine sound passed freely, and the retroverted fundus was easily replaced, but fell back again on the sound being withdrawn. Here, then, was a case of inflammation of the cervix extending into the canal of the neck; and the most prominent symptom—in fact almost the only one complained of—was pain during menstruation, and certainly the constitutional disturbance produced by that pain was greater than it has ever been my lot to see under any circumstances—the system being brought on many occasions into a state resembling the collapse of cholera.

By the use of frequent leeching to the cervix, counter irritation, painting the cervix with strong tincture of iodine, and long-continued rest, with appropriate constitutional treatment, the condition of the cervix was improved, and considerable relief from pain was given; but she still remained sterile, and after the lapse of some time all the symptoms returned, and I was again called to see her on 15th September, 1864, when I found her suffering nearly as much as on any previous occasion. As soon as the catamenial flow was over I examined the uterus, when I found the cervix again inflamed, and assuming the same spongy condition as before, which perhaps had never been entirely removed, and I determined, after subduing the acute symptoms, to try to modify the condition of the tissues by the deep action of the potassa cum calce. With this, after the lapse of a few days, I formed a deep eschar on each lip, and at the same time I passed the caustic into the interior of the neck, so as to act superficially on the mucous membrane lining it; when the eschars separated, the ulcers produced by the caustic healed rapidly, but required the occasional application of nitrate of silver to check exuberant granulations. The cervix became firm and small, the secretion of tenacious mucus from the cavity of the neck ceased, and the os closed to its natural size. The succeeding period passed without any pain whatever; the lady became pregnant, and is now in her seventh month, and in the enjoyment of good health.

Case 2.—The next case is an illustration of that form of dysmenorrhœa in which the body of the uterus is the part engaged, the cervix often remaining healthy, or being but very slightly affected. Fortunately, this is not so common a form of the disease as that which existed in the preceding case, for, from the affected part not being so accessible to treatment, its cure is much more difficult. I have met the disease in two forms—1st, as an effect of deficient involution of the uterus after delivery; and 2nd, as the result of chronic internal metritis. The following case is an illustration of the first of these forms:—Mrs. W., a widow lady, consulted Dr. Banks for palpitation and a variety of dyspeptic symptoms, and on tracing these to their true source—a uterine affection—he placed her under my care. She was the mother of four children. Her husband had died of fever shortly before she

was confined of her last child, now ten years old. She had a bad recovery from this labour, and was not able to nurse the child. Ever since that time she had suffered from pain in menstruation—the usual course being to have pain for some hours before the menstrual fluid appeared; the discharge then came on scantily, and afterwards gradually increased, when the pain subsided. She now remained free from pain, but on the third day the discharge generally increased greatly in quantity, and on many occasions the hemorrhage was so great as seriously to alarm and weaken her. During the intervals of menstruation she suffered chiefly from palpitations and flatulent dyspepsia, but sometimes she had severe pains in the back, extending down one or other leg, and making her almost unable to move. This was attributed by a medical friend in the country to a falling backwards of the uterus, so as to press on some of the sacral nerves, and was relieved almost instantaneously by him by placing her in the erect posture, passing his finger into the vagina, and tilting the uterus forwards. On examination I found the cervix uteri a little larger than normal, but presenting its natural colour and consistency, with a smooth surface, and the os of its normal size. A sound was introduced and passed in to the extent of four inches, being an inch and a half more than it ought to do. On placing my hand on the hypogastric region I could detect the body of the uterus very much enlarged and tender to the touch, but the enlargement was uniform; there was no tumour in any part of the uterine walls, or localised swelling. This, with the history of the case, the absence of any unhealthy uterine discharge, and the comparative freedom from constitutional suffering, induced me to attribute the enlargement to deficient involution after labour rather than to chronic metritis. I may here remark that hemorrhage on the 2nd or 3rd day after menstruation is, I believe, an almost constant symptom in cases of enlargement of the body of the uterus. It will be observed that there was, in both of the foregoing cases, great pain before and at the commencement of menstruation, which was relieved when the discharge flowed freely; thus resembling cases where there was a mechanical impediment to the escape of the discharge, and yet during the intervals a sound passed freely into the uterus, showing that the canal was quite pervious. Two explanations of this fact have been offered. Dr. Graily Hewitt and Dr. Meadows believe that, though the canal is quite open during the intervals between successive menstruations, it is closed at the commencement of the process by the congestion and erectile enlargement of the uterus which takes place at this time, and that the pain arises from the efforts of the uterus to expel its contents. Scanzoni, adopting a similar view, suggests that "the abnormal nervous irritation of the uterus may give rise to reflex movements of the organ, and to a convulsive contraction of the cavity of the neck. Hence the escape of the blood which is already effused is impeded by an obstacle which irritates in its turn the motor nerves of the body and fundus of the uterus. This (he says) is what to us appears particularly to take place in cases where, after intense expulsive pains, lasting perhaps several hours, the patient suddenly ejects a considerable quantity of blood, partly liquid and partly coagulated, and finds instantaneous relief." Dr. West, on the other hand, attributes the pain altogether to the congestion. "The congested womb aches (he says) until nature bleeds it, just as the head aches till the cupping glass or the leeches have relieved the overloaded cerebral vessels." My own observations incline me to conclude, with Dr. Graily Hewitt and Dr. Meadows, that in these cases there is partial, but temporary, menstrual retention, and to attribute this to the mechanical closure of the passage by the erected and congested state of its walls.

Acting on this hypothesis in the case now under consideration, I recommend the application of two or three leeches to the anus, that, by emptying the hemorrhoidal veins which have a direct communication with those of the uterus, the pressure on these might be relieved. The effect was most beneficial. The pain at the commencement of the process has been greatly and almost immediately relieved, and there have been no hemorrhages, so that, instead of being weakened by the leeching, there has been a great saving in

the amount of blood lost, and in the strength of the patient. The administration of Indian hemp contributes to the same end, as it lessens the tendency to hemorrhage, and acts at the same time as a sedative. A combination of ipecacuanha with sedatives is also useful, the nausea caused by the ipecacuanha perhaps serving to relax the spasms, or reducing the erection of the os. In some of these cases the warm hip bath, especially after leeching and allowing the leech bites to bleed in the bath, gives more prompt relief than any other means.

The enlargement of the body of the uterus, or the chronic metritis, must be attended to during the inter-menstrual periods, but unfortunately both affections are very little amenable to treatment. In some cases of metritis the retention of the secretions within the uterus keeps up the irritation, so as to render all treatment useless, until the passage is fully dilated.

Dr. Simpson was, I believe, the first eminent practitioner who adopted the plan of dividing the cervix, for doing which he uses a bistourie caché, furnished with a screw by which he can regulate the extent to which the blade protrudes from the shaft. This instrument he introduces in its closed state into the os, guiding it with his finger; he then presses on the handle, causing the blade to protrude; he then withdraws the instrument, dividing in doing so one side of the cervix. He then turns the instrument, reintroduces it, and divides the opposite side. He directs us in performing this operation to divide the os internum to a slight degree, and to slit completely the portion of the cervix within the vagina, so as to make a funnel-shaped opening. Nitrate of silver is then applied to the angles of the wound so as to prevent union by the first intention, and the parts are to be painted over with a solution of perchloride of iron to prevent hemorrhage.

Our late president, Dr. Beatty, has suggested the use of the four-bladed speculum in this operation. In one case I adopted this plan, but found it very difficult to make sure of the distance to which I had introduced the metrotome into the uterus, owing to the way in which the polished surface reflected the light, so that in subsequent operations I have always used Sims's duck-billed speculum, which enables me to see and touch the os at the same time.

Various objections have been made to this operation both as to its details and to its principle. I shall first consider the improvements that have been suggested as to the details. In the first place, it is objected by Dr. Coghlan of Wexford (who has written some valuable papers on the subject) that the incision is unnecessarily large, and he has suggested the use of a cutting gorget with a probe point to make an opening of a definite size by being pushed into the uterus, the probe point keeping it within the canal; he then introduces a roll of lead to keep the wound from closing, and he has published several cases successfully treated on this plan. In using this instrument the finger is placed against the os uteri, and the gorget glided up on the finger until the point enters the os. It is then pushed on in the axis of dividing the cervix in its passage to the extent of its own breadth only. Dr. Coghlan believes the probe point is sufficient to keep the gorget within the canal of the cervix, and that from the limited extent of the incision there is little or no hemorrhage. He also states that this instrument may be used in cases where the os is too small to admit Simpson's instrument, as the probe point can pass into the smallest-sized os, and the blade makes an opening for itself. It is also claimed by Dr. Coghlan that the incision made by this instrument is quite large enough for all practical purposes, and that it does not require to be made so large as is done by Dr. Simpson's instrument, which, it is alleged, frequently creates hemorrhage to a serious extent. Having made the incision, Dr. Coghlan takes some fine thin lead and rolls it round the point of another instrument, which he calls a dilator. This instrument is furnished with a screw, by which it can be contracted or dilated, and the lead having been rolled round its point it is introduced in the closed state into the uterus, and then by pressing on the handle the instrument is expanded, and the lead opened out. This being done the instrument is closed and withdrawn; leaving the leaden tube in the canal of the cervix. In some cases,

where the contraction is not very great, Dr. Coghlan proposes to dispense with the cutting part of the operation altogether, as by introducing the dilator into the cavity of the os it may be dilated sufficiently, and by letting the lead remain within the opening for some time the dilatation may be rendered permanent. I believe this plan to be often very useful, especially after the use of sea-tangle or other dilating tents.

Another instrument for performing this operation has been proposed by Dr. Greenhalgh of London. Unlike Dr. Coghlan's, this instrument has a blunt point, and in this I think has an advantage in point of safety. In some cases Dr. Greenhalgh's instrument would not in the first instance pass into the uterus, but this is easily remedied by in the first instance slightly dilating the os by the use of sponge or sea-tangle. Dr. Greenhalgh's instrument is furnished with two blades, concealed in a metallic sheath. It is introduced in its closed state into the uterus, the handle is then withdrawn, which causes the blades to protrude, one at each side, slowly expanding in proportion as the instrument is withdrawn from the uterus. In this way both sides of the os are cut by one operation. It may be observed that the degree to which the blades protrude is, at first, very small; but as the handle is withdrawn the blades become gradually divaricated. Thus the os internum is divided very slightly, and the os externum completely, making the funnel-shaped incision recommended by Simpson, but doing it all at one operation. I believe this to be an improvement on Dr. Simpson's instrument, inasmuch as it divides the cervix at one operation, and divides the two sides equally. In using Simpson's instrument the back is firmly supported during the dividing of the first side of the cervix, but in dividing the opposite side the back of the metrotome slips into the incision already made, and the second side is either divided insufficiently, causing the operation to fail, or it may be divided too extensively, giving rise to profuse hemorrhage, or some of the other accidents that have followed the operation. But Dr. Greenhalgh's instrument seems to me objectionable, and that for two reasons—first, it attempts to do by mechanical arrangements what should be done by the hand and eye of the operator—viz., to regulate the extent and direction of the divisions, and it is consequently not adapted to uteri of varying size; second, the bulk of the sheath obstructs the view of the parts to be divided.

The instrument I have used in all my late operations is the one known as Dr. Savage's. It is like a scissors, and is composed of two blades with their cutting edges directed outwards. When the instrument is closed the blades overlap each other, and thus the cutting edge of each is protected by the back of the other, so that it can be introduced into the cavity of the cervix without any additional protecting sheath, such as is used in Dr. Simpson's and Dr. Greenhalgh's instruments. It is introduced into the uterus within the os internum in the closed state, the canal being first dilated with sea-tangle if necessary. The blades are then opened by pressing on the handle, and then withdrawing the instrument you divide the cervix; the extent to which the blades are expanded being regulated by a screw. In using this instrument I have always placed the patient in the prone position, and introduced the duck-bill speculum, which is held of course by an assistant. I then pass the instrument into the uterus and place my finger on it close to the os. The advantage of this is, that you can both see and touch the os at the same time, and tell with accuracy the distance to which you pass the blade into the os, so as not to pass it farther than requisite. In withdrawing the instrument I expand the blades in the first instance very slowly, so as to make the incision in the os internum very narrow; and I divaricate the blades as I withdraw them, so as to divide as much of the cervix as I think necessary, which can be done with great accuracy, as the operation is under the guidance of both finger and eye. I am generally in the habit after the operation of introducing a roll of thin lead, as recommended by Dr. Coghlan; but Dr. Coghlan's instrument for introducing the lead seems to me to be too small, and I generally use an ordinary dressing forceps by rolling the lead round the forceps, and then passing it into the wound, when by opening the blades of the forceps I expand the lead, taking care to do so more below than

above, so as to preserve the funnel-shape of the incision. I then withdraw the forceps, leaving the lead in the wound. I then place a piece of cotton wool in the upper part of the vagina, so as to keep the lead in its place, and to control bleeding, if there should be any. The lead may be withdrawn after a few hours, as there will be no risk of union by the first intention, but in some cases it may require to be reintroduced at a subsequent period to prevent contraction of the cicatrix.

In a paper recently published in the 'Lancet,' Dr. Marion Sims advocates very strongly the slitting of the os uteri in cases of dysmenorrhœa, and describes the mode he adopts, which is different from any of those I have alluded to. He places the patient in the prone position, and, having introduced the duck-bill speculum, he seizes the anterior lip of the uterus with a hook, so as to steady it and draw it downwards, and then he divides with a scissors the sub-vaginal portion completely through on each side. Having done this, he passes a knife, and divides the os internum on each side, but to a less degree of course. The knife he uses in this stage of the operation consists of a long blunt stem, with a small, curved, and movable blade attached at its point. I have not seen the operation performed in this manner; but, judging from the description, it does not seem to have any advantage over the less complicated method with Savage's hysterotome.

So much as to the various instruments which have been devised for performing this operation. The objections which have been made to the principle of the operation itself are founded—firstly, on the hemorrhage which it produces. As to this, I believe that with Dr. Savage or Dr. Greenhalgh's instruments the liability to hemorrhage is very much lessened, and, at all events, it can, I believe, in all cases be fully controlled by plugging the vagina. Another objection, and perhaps a more serious one, is founded on the liability to the setting in of pelvic cellulitis after the operation, or of inflammation extending to the peritoneum, and giving rise, either to acute peritonitis, or (as occurs more frequently) the subacute and chronic form of the disease. This objection is, no doubt, a very serious one; but I think that by selecting a proper time for the operation—*i. e.*, when the patient is in good health, and by insisting on a sufficient amount of rest after the operation, the danger is very much lessened. There is no doubt that in cases where this operation has been performed in the study of the surgeon, and the patient allowed to walk home and go about her ordinary avocations, this danger is very much increased. Dr. Gream of London has lately proposed a new objection to the operation, but he has surely exaggerated. He states that in a case in which the operation was performed and the patient subsequently proved pregnant, he found the os, at an early period of the pregnancy, completely open and the membrane exposed, as if in the first stage of labour. The result was, that though the lady proved pregnant on several occasions, she always aborted in the third or fourth month. I have attended a lady in several pregnancies who had been operated upon by Dr. Simpson, and there was no tendency to abort, nor was the os dilated to such an extent as to expose the membranes. —*Dublin Medical Press, June* 7, 1865, *p.* 536.

92.—ON THE DYSMENORRHŒA, METRORRHAGIA, OVARITIS, AND STERILITY ASSOCIATED WITH A PECULIAR FORM OF THE CERVIX UTERI AND THE TREATMENT BY DIVISION.

By Dr. Robert Barnes.

(Abstract of a paper read before the Obstetrical Society of London.)

The author described and figured the form of cervix uteri which projected into the vagina as a conical body, the vagina appearing to be reflected off at a point nearer the os internum than normal. The os externum was usually minute, scarcely admitting the uterine sound. This (the os externum) was the real seat of constriction. The os internum normally was a narrow opening; and in these cases of dysmenorrhœa and sterility it was commonly found

to be of normal calibre. It was therefore unnecessary to divide it. It was, moreover, dangerous to divide it, on account of the close proximity of the large vessels and plexuses running into the uterus on a level with it. The author maintained that this form of cervix was a cause also of retro- and peri-uterine hæmatocele, and of peritonitis. All these consequences might arise in single women. In the married state the evils enumerated were aggravated, and new ones arose. Women with this peculiarity were generally sterile; and if they became pregnant it was early in life, before the further consequences were developed. These were flexions, deviations, inflammation of the cervix and body, hypertrophy. Discussing the question of treatment, the author showed that dilatation was unsatisfactory; that incision of the os internum, as practised by Dr. Simpson's single bistourie caché and by Dr. Greenhalgh's double bistourie caché, was unsafe and superfluous. He objected to the latter instrument, especially, that it must cut as it was set —that it was too much of an automatic machine, not leaving scope for the judgment of the operator. His (Dr. Barnes's) own instrument, constructed like a pair of scissors, acted on the same principle as Dr. Sims's: it divided only the os externum, so as to open the cavity of the cervix; the part to be cut being first seized between the two blades, the operation was perfectly free from risk; the hemorrhage was usually slight; and a good os was made. He had performed the operation many times, both in hospital and private practice, and was well satisfied with the results. One advantage of incision over dilatation was, that it relieved the engorgement and inflammation.

In illustration of the behaviour of the conical cervix uteri under labour, two cases were narrated. In one, the cervix and os uteri had returned to their original state, although a fœtus of four-and-a-half or five months' development had been expelled through them. In the other case it was necessary to open the cervix artificially by means of the author's cervical dilator and incisions in order to deliver a full-grown child. In both cases pelvic cellulitis followed labour.

Mr. Baker Brown thanked Dr. Barnes for having brought the subject forward. He agreed with most of what had been stated in the paper; was opposed to dilatation, as being inefficient and temporary. He described his mode of operating, which was to place the patient in the lithotomy position, and, having introduced a bent speculum, he seized the os with a pair of forceps, and divided it with Simpson's hysterotome. He never divided the internal os; always used a plug of oiled lint to prevent hemorrhage. He regretted that the operation had lately been condemned by a high authority, but believed it was the only efficient and permanent remedy for these painful affections.

Dr. Greenhalgh was surprised to hear the President's opinion that the seat of stricture in these cases was mostly at the external os. He (Dr. Greenhalgh), on the contrary, expressed his conviction that in the great majority of cases the stricture is situate at the internal os, and consequently he recommended division of the internal as well as the external os. After division he usually introduced one of the bilateral expanding stems, which keeps up steady dilatation, and prevents contraction. As regarded hemorrhage, which some appeared so to dread, he had never but once met with it, though he had operated in nearly one hundred cases. He always used his own bilateral instrument, which cuts both sides at once. The advantages of his plan of operating were, he believed, extreme exactitude, facility, painlessness, and the avoidance of any personal exposure. He expressed his surprise at the remark of Mr. Brown that he never divided the internal os, when he (Dr. Greenhalgh) had seen him on several occasions freely incise the internal os in the cases under consideration.

Mr. Baker Brown, in answer to Dr. Greenhalgh, said that that gentleman must be mistaken in what he had seen at the London Surgical Home. He repeated that he never in cases of dysmenorrhœa cut through the internal os. Dr. Greenhalgh was evidently confounding this operation with that for fibrous tumour, retroversion, retroflexion, &c., of the

uterus, in which he (Mr. Brown) incised freely, and generally through the internal os.

Dr. ROUTH fully confirmed Dr. Greenhalgh's view. For his own part, he believed in by far the majority of cases the obstruction was at the *inner* and not the *outer* os; although he did not deny that in some cases of conoid cervix it was present at the external os. He agreed with Dr. Gream in believing that Dr. Sims's plan of operation would occasionally leave a deformed cervix for life; and he did not think it was necessary to cut through the entire cervix. The instrument Dr. Greenhalgh had invented obviated all danger from hemorrhage. The same was true of his (Dr. Routh's) instrument, which he, however, preferred, because on the bend, and, therefore, more easy of application in *flexion* cases. A little bleeding was salutary. In most of the cases there was a complication of congestion, which the very incision by the subsequent hemorrhage relieved. But there was no doubt that such incisions, however freely made, had a tendency to contract again. Hence it was necessary to keep the cut made patent by some internal uterine pessary, and for some time, it might be for months, so as to allow it to become properly lined with mucous membrane and incontractible. He knew several persons now walking about London with these. In other cases their removal had been followed by conjugal relations and pregnancy, though previously sterile for years. Of the use of sponge-tents and other modes of artificial dilatation in these cases he spoke disparagingly. He had seen cellular abscess and death follow their use. They should be used with the greatest caution. He also believed cases of dysmenorrhœa were more common than was generally supposed. Not only was the seat of obstruction more frequently at the *internal* os than the *external*, but, indeed, in many cases the external os was patent and abnormally so, as shown by Dr. Henry Bennet. And there were many, and by far more numerous, cases of dysmenorrhœa which were in no way due to stricture at either os. As these cases were not, however, referred to by Dr. Barnes, he did not allude to them further.

Dr. GRAILY HEWITT thought that the two questions of the treatment of dysmenorrhœa and of sterility by means of incision of the cervix uteri had been too much mixed up together. He would say a few words first respecting dysmenorrhœa. He believed that in bad cases of dysmenorrhœa the condition present was frequently retention of the fluid in the uterus, and that this retention caused the pain; and he had been at some trouble to prove this. But, on the other hand, he also thought that the condition was capable of being relieved, in most cases, without resort to mechanical treatment of the cervix uteri. The great thing was to diminish the flow of blood, and this could be regulated by general measures; but that there were a few cases in which such general measures were useless he admitted. He differed from the President in reference to the most common seat of the constriction; for although there were cases in which the os uteri was congenitally extremely small and narrow, yet in the larger number of cases of dysmenorrhœa the impediment was situated at the junction of the cervix with the body of the uterus. With regard to the best method of applying mechanical relief when such was required, he thought that cases must be treated on their own merits. Where the cervix uteri was hard and dense, the cutting operation was most indicated, the difficulty being here the greatest; but under other circumstances he preferred the use of tents as dilators. The sea-tangle tent was, he considered, a perfectly safe means of dilating the cervix uteri; but, he would repeat, the cases were few requiring this treatment. As to the mode of incising the cervix or os uteri, here again the operation must be selected according to the case: no one operation could be suited to all circumstances. He would next say a few words on the subject of sterility. It was undoubted that in certain cases the cure of sterility could be effected by dilating the cervix uteri, and much had been said as to the superiority of one mode of dilatation over another. The fact was, that so long as the canal of the cervix was a little enlarged, whether by incision or dilatation, the necessary end would be served. The great object was to secure a tolerable patency of the canal at about the menstrual period, when conception was most likely to oc-

cur. Supposing the sterility to be cured, the dysmenorrhœa which might be associated with it would be also, in all probability, permanently relieved.—*Lancet, July* 15, 1865, *p.* 67.

93.—ON THE SURGICAL TREATMENT OF PAINFUL MENSTRUATION.

By Dr. J. Henry Bennet, late Physician-Accoucheur to the Royal Free Hospital, &c.

Menstruation may be painful, even actually painful, from its dawn to its close, without any mischief or impediment existing of any kind whatever. I believe there is a tribe of females, as it were, in whom menstruation is thus attended with pain, from physiological uterine sensitiveness, throughout all the phases of life, before and after marriage and child-bearing, and in the absence of any morbid condition. Only these women are very liable to uterine diseases; with them the organ is a weak, sensitive one.

[In his clinical notes Dr. Marion Sims does not allude to Dr. Bennet's discovery of the existence of a sphincter at the os internum, separating the cavity of the cervix uteri from that of the uterus itself.]

This sphincter is formed by the circular fibres of the cervix, is very vital, opening and closing in connection with menstruation, and probably with emotional conditions. It is generally closed during the interval of menstruation, so much so that a sound *does not* naturally pass into the uterus. It stops one inch and a half from the external os, and generally some force is required to overcome the resistance even in the healthiest females. I believe that the non-recognition of this internal sphincter is at the bottom of the very mechanical views which I now oppose. I may add that contraction of the cervical os is rare and easily remedied, and the cavity of the cervix is scarcely ever contracted without severe disease.

Dysmenorrhœa showing itself in women who have not had it before, or aggravated when constitutionally present, is, I firmly believe, very much more frequently the result of morbid conditions, of chronic inflammations of the cervix and of the body of the uterus, than of physical obstruction in the cervical canal. When that obstruction or contraction exists, it is usually the mere result of the swelling of chronically inflamed or hypertrophied tissues, and disappears without any operation when the inflammation has been removed, and the uterus has had time allowed it to fine down. So far from fibroid growths, which develop themselves in the walls of the uterus, and increase its volume, producing contraction of the cervical canal and orifice, and thus impeding the exit of the menstrual fluid, I have nearly always found that they dilate and open the os internum. Thus the free and easy passage of a large bougie into the cavity of an enlarged uterus at any time is, in my eyes, a strong reason for believing in the existence of a fibrous growth. The dysmenorrhœa in such cases is usually the result of the irritation produced in the uterus by the presence of the tumour.

The constitutional form of dysmenorrhœa, when not very severe, in my opinion requires neither medical nor surgical treatment: merely care, rest, and warmth at home. Marriage and child-bearing sometimes modify the organic state of the uterus, and cure it naturally. If very severe, so much so as to cast a gloom over life, either in unmarried or in married women, it is usually connected with inflammatory disease of the uterus, which is also generally the cause of the narrowing of the cervical canal. The inflammation must of course be treated; and that subdued, the patient should be left to herself for some months, for the engorgement or hypertrophy of the uterus, which it has produced, to subside. As it does subside, generally speaking, the cervical passages open, and a natural cure is produced. If they do not thus open, dilatation really becomes necessary, and should be carried out in one way or another.

To accomplish this purpose—merely to establish a free communication between the cavity of the uterus and the vagina—I cannot see why, rationally, such severe operations as are now proposed and practised should be resorted

to. I eliminate the idea of danger, because I think there is very little in the hands of experienced practitioners like Dr. Simpson and Dr. Marion Sims. But the operation as described by Dr. Marion Sims, and as practised by him and others, is a formidable one, which can and will only be undertaken by exceptionally skilled and experienced men. Is an operation which must remain in the hands of consulting practitioners the rational one, or necessary one, for the cure of a common, every day uterine condition? Such is the question that I would propose. Again, Why should the entire vaginal portion of the cervix be bifurcated down to the attachment of the cervix to the vagina, to remedy the narrowing of a small canal which passes through its centre? Why should the integrity of the circular fibres of the cervix, which have certainly something to do in pregnancy and parturition, be so seriously compromised—be cut right through in two regions? We must remember that, anatomically, as soon as the os tincæ has been passed, the sound enters a real cavity—the cavity of the cervical canal. The os itself can be dilated, or even slightly cut through if necessary, and then easily enlarged to almost any extent with the greatest ease by bougies; and that accomplished, there is no contraction usually found until the internal sphincter or its vicinity be reached. The bifurcation, or even the simple division as Dr. Simpson practises it, of the cervix down to its vaginal attachments seems to me, in these cases, as unreasonable as it would be to divide the entire thickness of the gland of the urethra, in the male, down to the corpus cavernosum for a stricture of the outlet of the urethra.

Once the os internum or its vicinity has been reached, and it is judged that contraction really exists, and not mere spasmodic closure of a sphincter, what is to be done? Division is here very feasible and easy; but I do not see why it should be carried beyond a line on each side. Such a division gives very much more space than exists naturally, and allows a good-sized bougie to pass freely into the uterus. Twenty years ago Dr. Simpson, then in town attending a noble lady in her confinement, gave me his recently invented metrotome, which he praised in no measured terms. I thought myself that he had given me the philosopher's stone for dysmenorrhœa and sterility, and at once set to work. I divided the os internum, as above described, in very many cases, and never had any hemorrhage or accident. Indeed, I believe the hemorrhage is more the result of the slashing division of the vaginal cervix than of the division of the os internum. I was delighted with the success of the operation, and expected great things. But, to my regret, I found that when these patients came to me six months or a year after, the contraction was all but invariably worse than ever. The incisions had healed, contracted, and matters were worse instead of better.

I then adopted another plan. I divided as before, daily introduced a good-sized bougie to keep the parts open, and, after a week, introduced and left in the cervical canal one of Dr. Simpson's metal-stem pessaries as modified by myself. I curved them to suit the anterior congenital curvature of the uterus —which, I believe, I was the first to recognise in the living—and diminished the size of the foot. I used to leave them twenty-four or forty-eight hours, then remove them for twenty-four or forty-eight hours, and so on for about three weeks more, to prevent contraction. In this way I obtained good and permanent results.

The above process, however, I found troublesome; it involved the slight operation, and the wearing the metallic pessary for several weeks, during which time I always felt rather nervous about the patient. So I reverted to the sponges, which I have mostly used ever since, and without one single accident, although I have applied hundreds and hundreds. But then I use precautions, which the physicians Dr. Sims quotes do not appear to have used. I *never* apply a sponge without previously ascertaining by a speculum examination that the cervix and its cavity are quite healthy. To dilate an inflamed cavity is to produce a storm. I never leave the sponge more than some twenty hours *in situ*, making the patient withdraw it herself by means of a string. I never use them more than an inch or an inch and a quarter in length, so as not to pass into the uterine cavity. I always leave one, two

or three days for the parts to recover between each application. I use very small tents, much smaller than those Dr. Simpson recommends: and I stop if the slightest irritation is produced. By these means I generally succeed, in the interval between two monthly periods, in opening out the entire canal, the os internum giving way last. The effect of this treatment, being gradual, is, I believe, more permanent.

I discard all metal dilators as irrational, and calculated to do more harm than good. It must be remembered that the cervix is more than a quarter of an inch thick, and of very firm, unyielding structure. What can be expected of an instrument that is intended to open a canal running through such structures by the expansion of two metal blades, if not that it should crush the tissues that give way? And such I believe is the case. I have seen many cases in which harm had evidently been the result of their use.

By using all the above precautions, as stated already, I have never had any accident of any description either by the sponges or by division.

I fully admit the necessity of the occasional dilatation of the cervix uteri; but adhere to the opinion expressed in my work seventeen years ago, that careful dilatation by prepared sponges is the safest and easiest plan, quite as effectual as the division, and has the advantage of being so simple that it can be carried out by any well-informed practitioner. As either this plan of treatment or simple linear division will do all that is required to re-establish the uterine passages when closed, I cannot accept for that purpose the more severe and serious operation now proposed, save as a very exceptional measure.—*Lancet, June* 24, 1865, *p.* 673.

94.—ON THE OPERATION FOR THE RELIEF OF A LACERATED PERINEUM AND SPHINCTER ANI, &c., WITH SOME OF ITS COMPLICATIONS.

By Thomas Bryant, Esq., Assistant Surgeon to Guy's Hospital.

The principles of practice which have been laid down for plastic operations in the vagina, for the relief of the vesico- or recto-vaginal fistulæ, are in a measure applicable to cases of lacerated perineum, whether simple or complicated, for the objects of the surgeon in both classes of cases are very similar, and they are to be secured by the same ends, the means for obtaining them requiring only such modifications as the change in the situation of the parts affected necessarily demands.

The points which claim attention in this operation are not numerous, although they are most important; success being secured only by their close observance.

They may be described as follows:

The surgeon having carefully examined the parts, and determined the important question of how much of the lacerated perineum requires closure, should place his patient on her back in the lithotomy position, and map out the portion of the membrane which he intends to remove, with a sharp scalpel.

He must remember, as a point of primary importance, that the surface of the wound should be *clean and regular;* for it is essential to secure primary union of the two surfaces; the new perineum also, to be of permanent service, must be *firm, strong, and thick;* for if otherwise, the success of the case will be but partial or of little permanence.

To secure these two objects, a free and clean section of the lacerated perineum is absolutely essential; it cannot be made too smooth, nor can the surfaces to be placed in apposition for union be too broad posteriorly towards the rectum; at least an inch of raw surface is not too much, when it can be secured, the width of the bared surface diminishing slightly towards the anterior portion of the labia. The best plan to obtain these results is to perforate the parts towards the centre, and, as it were, slit them up with one

sweep of a sharp scalpel; the right half of the perineum being first treated, and the left subsequently, the whole area having been mapped out previously by a bistoury.

This *first* step of the operation having been performed, the second, which is of no less importance, remains to be carried out, and that is the bringing of the parts into apposition and keeping them there by sutures. Wire sutures are probably the best for these purposes, for if they do not cause less irritation than smooth silk they are at any rate more manageable. Each one should be inserted at least half an inch, or, better still, an inch from the margin of the wound, and brought out at its posterior border close to the vaginal mucous membrane; it should be then reintroduced at a corresponding point on the opposite side, and brought out at a spot similarly situated to the one at which it was introduced. When the recto-vaginal septum or sphincter ani has been lacerated, the introduction of the posterior suture is of critical importance, for it is an essential element of success that this septum should be likewise included and drawn forwards to the raw surface of the perineum by the suture. To secure this end the suture must be introduced, as already described, at a point situated on a horizontal line passing through this septum; the needle is to be dipped well into the thickness of the tissues, and instead of making its appearance at the posterior margin of the wound, close to the vaginal mucous membrane, is to be made to pass through the thickness of the recto-vaginal septum, and then out of the right buttock, at a point corresponding to the one at which it was inserted in the left side. When this is well done the wire or silk is buried completely in the tissues, and on being tightened the whole parts are drawn together, as it were towards a centre, corresponding to the anterior portion of the anus and posterior of the perineum. The other anterior sutures may be applied as already directed.

With respect to the necessity of dividing the sphincter ani in this operation, I will only add that I have not yet met with a case requiring such treatment.

The sutures need not be removed at too early a period, the eighth or tenth day being generally the best, but when good union has taken place, the practice of leaving them in position is certainly unnecessary, if not injurious. The *urine* should be drawn off at regular intervals, and the *bowels* kept quiet by means of opium. The ten-grain suppository of the compound soap pill, after the operation, is a valuable practice in this as in all cases of abdominal surgery, for it allays local spasms, keeps the bowels in a quiescent condition, and secures rest to the stomach, which, after chloroform, is so apt to be irritable. Perfect *cleanliness* is essential during the whole treatment of these cases; and the horizontal posture is to be maintained, the legs being tied together. The patient's diet should be liberal and much as usual.

Success in these cases depends much upon what are termed small matters, and it is of great importance to give them due care and attention, for recovery may, as a rule, be secured in all these cases, however complicated.—*Guy's Hospital Reports, Vol.* xi., 1865, *p.* 270.

95.—TWO CASES OF OPERATION FOR THE CURE OF LACERATION OF THE PERINEUM.

Under the care of JAMES LANE, Esq., at St. Mary's Hospital.

[In the first case there was not a vestige of perineum to be found, the sphincter having been completely torn through. The patient was otherwise in fair health.]

The operation for restoration of the perineum was performed by Mr. J. Lane on the 5th of April, 1865. A portion of mucous membrane and skin was dissected off on each side at the entrance of the vagina, in a position corresponding to the original situation of the perineum. The raw surfaces thus made were about an inch and a half in length, about an inch in width inferi-

orly, but tapering to about half an inch in width superiorly. They were made to be continuous with each other posteriorly, at the point corresponding to the angle where the laceration had ceased. Four stout silver-wire sutures were then passed across deeply from one side to the other, and by means of these the deep portions of the denuded surfaces were firmly held together on the principle of the quill suture. The quills were represented by two ivory bars, perforated with four holes, through which the wires were passed and fastened. Four superficial sutures of thin silver wire were then applied to the anterior edges of the wound, in order to retain them in accurate contact. The sphincter was then divided freely in a direction outwards and slightly backwards on the left side.

For a week after the operation a grain of opium was given night and morning, for the purpose of keeping the bowels quiet; and the urine was drawn off twice daily with a catheter. Forty-eight hours after the operation the deep sutures were cut and removed together with the quills. Some slight œdematous swelling of the parts had taken place, but this soon subsided after their removal. The superficial sutures were not removed till the eighth day after the operation. At this period the union appeared to be tolerably firm. A free aperient was therefore given, and the bowels were emptied on the following day without any injury to the newly united parts. After this, the tenderness of the parts rapidly subsided, and she was discharged from the hospital quite well on the 27th April, twenty-two days after the operation. There was then a firm and thick cushion in the natural situation of the perineum. Control over the motions was restored, and she expressed herself as having regained the natural sense of support and security in the perineal region which she had lost since the accident happened.

Case 2.—A. R., aged 35, was admitted into St. Mary's Hospital on the 24th February, 1865, under the care of Dr. Tyler Smith, but was afterwards transfered to Mr. J. Lane for operation. She had been married six years, and had had three children. The rupture had occurred with the second child, when she was in labour for thirty-two hours. The head was very large, but instruments were not used. A detailed description is unnecessary, as the rupture was almost an exact counterpart, as to its extent, of that met with in the first case. A similar operation was performed on the same day (April 5th), with precisely the same after-treatment, and with a like result. She left the hospital quite well on the 28th April, twenty-three days after the operation.

Mr. J. Lane, in his clinical remarks, stated that these cases furnished excellent examples of complete rupture of the perineum. When the sphincter was torn through, as in both these cases, very great care and attention were required to retain the parts in accurate contact, because the action of the muscle was continually tending towards their separation at the lower angle of the wound. For this reason it was very desirable to paralyse its action for the time by an incision; but he believed it was sufficient to divide it on one instead of on both sides, as had been recommended. It was especially necessary to secure accurate union at the angle corresponding to the termination of the rupture, where the two raw surfaces became continuous with each other, otherwise there was the risk that an aperture might remain at this spot behind the new perineum; and though the perineum might be effectually restored there would be the inconvenience of a recto-vaginal communication behind it, a condition scarcely less annoying to the patient than that which the operation was intended to remedy. Mr. J. Lane said that he had used the wire suture and the perforated ivory bars for the last five years, during which time he had operated on, he believed, about thirty cases of rupture of the perineum of every degree of severity. He had every reason to be satisfied with the mode of operation, as he had not failed to obtain complete union in a single instance.—*Lancet, Sept.* 9, 1865, *p.* 289.

96.—ON VESICO-VAGINAL AND RECTO-VAGINAL FISTULA.

By THOMAS BRYANT, Esq., Assistant Surgeon to Guy's Hospital.

In the treatment of recto- or vesico-vaginal fistula there are two chief objects which the surgeon has in view.

First, to pare with nicety and accuracy the whole margin of the fistula; and *secondly*, to bring into, and to maintain in, close apposition the raw and incised surfaces. If these two ends be secured, the means by which they are attained are comparatively of small importance, although their simplicity is a point of considerable value. The chief difficulties of the operation are it is allowed, generally met with in carrying out the first object we have mentioned, and its ultimate success most unquestionably depends upon the mode in which this step has been performed; for, however well the second step in the operation may be executed, failure must ensue if the first has not been fairly accomplished. The hope and aim of the surgeon in these cases is to secure union of the pared edges of the fistula by primary adhesion, and to obtain this result two clean and even surfaces must be placed and kept in apposition. Surgeons who have attempted to pare a fistulous opening, situated either on the surface of the body or in the vagina, well know what care is demanded to prevent any irregularity or raggedness of the wound's surface, and they well know that, if this irregularity exists, primary adhesion of the edges cannot take place. It is this difficulty which, I believe, too often necessitates the repeated operations we hear of for the cure of a vaginal fistula; and it was to obviate such that I was led some four years since to the construction of instruments (modifications of that formed by Mr. Hilliard, of Glasgow), and which subsequent experience has fully proved to be of great value. I have reason to know that in other hands as well as in my own they have been equally serviceable, and that they have tended to facilitate the performance of a difficult operation, and render it more certain and satisfactory.

[The instrument consists of a handle to which are adaptable six guides, three for transverse and three for vertical fistulæ. In each of these two sets there is one with three prongs, one with two, and one with only one prong. In the set for transverse fistulæ these prongs are in the same line as the handle, whilst in that for vertical fistulæ they are at a right angle. In both sets the prongs are set in a bar in the same manner as the prongs of a rake.]

The advantages which are gained by the use of the instrument are, *first*, the certainty with which the whole margin of the fistula is incised; and, *secondly*, the accuracy with which the width, length, and evenness of the incision is secured.

The following are the directions for its application:

Assuming that an operation for vesico-vaginal fistula is to be performed, and that the fistulous opening is an inch long, the guide with three prongs should be selected, its width being sufficient to include the whole upper margin of the opening. The extent of surface to be pared should then be mapped out with a scalpel, a third or half an inch of raw surface being desirable when it can be obtained, and the prongs of the guide inserted at the edge of but not through the mucous membrane of the bladder, and passed *between* the tissues, beneath the vaginal mucous membrane to the required extent; the ends of the prongs should then be made to perforate the mucous lining of the vagina, at the line of incision already made by the scalpel, and with a blunt hook the tissues thus included should be well pressed down to the prongs' base; the whole of this surface thus raised by the guide is then to be excised by one sweep of the scalpel, passed along close to the posterior margin of the instrument.

By this means the cleanness of the wound is guaranteed, and its extent accurately determined, not the slightest fingering or chance of causing any irregularity in the wound's surface is experienced; indeed, with the exception of the subsequent introduction of the needle, no instrument need touch the surface of the wound a second time. The lower margin of the fistula should

then be treated in the same manner as the upper, thus completing the first and most important step of the operation.

The second step of the operation remains to be described, and although it may be equal in importance to the first, it is not one of difficulty, yet at the same time it requires some nicety in execution. The object is to bring the raw surfaces of the fistula into close apposition, and to maintain them there. This is to be carried out by means of sutures, the wire being the most convenient. The sutures should be sufficiently close to ensure the accomplishment of the object for which they are introduced, and may be fastened in any way which the operator prefers. The perforated shot applied to the twisted end of the wire answers every purpose, both fixing the wires and protecting their points. There is one point, however, of essential importance to be remembered in this, the second step of the operation, and that is the distance at which the sutures are to be inserted from the margin of the wound; as a rule, the greater the distance the greater the advantage to be gained—a third, or half an inch or more being desirable. The edges of the wound are not to be adapted too tightly, for swelling will occur, and, as a result, ulceration in the line of suture is sure to follow. This point is as essential in all plastic operations as in the one we are now considering.

The sutures may be left in for six or eight days or more; but I believe it best to remove them as soon as the wound is healed, no good object being obtained by leaving them longer, and in some cases ulceration may be set up, which may interfere with, if not mar, the ultimate success.

The urine should be drawn off periodically after the operation, but it is not necessary to irritate the bladder by retaining the catheter in it.

A good opiate suppository should be given to relieve pain or local spasm, and perfect cleanliness is to be enforced during the treatment.—*Gray's Hospital Reports, Vol.* xi., 1865, *p.* 259.

97.—A RADICAL OPERATION FOR PROCIDENTIA UTERI.

By Dr. Thomas Addis Emmet, Surgeon to the State Woman's Hospital.

In September, 1862, Mrs. C., aged 30, the mother of four children, was admitted to the hospital with complete procidentia of ten years' standing. She had been frequently irregular, and during the first examination, the attending physician having passed the sound, a miscarriage took place soon after at the third month of pregnancy. On the 10th of October following, I proceeded to operate, by removing the mucous membrane from the base of the bladder, in two broad bands, extending from the point B, a little behind the urethra, to A and C, on each side of the cervix, as recommended by Dr. Sims.

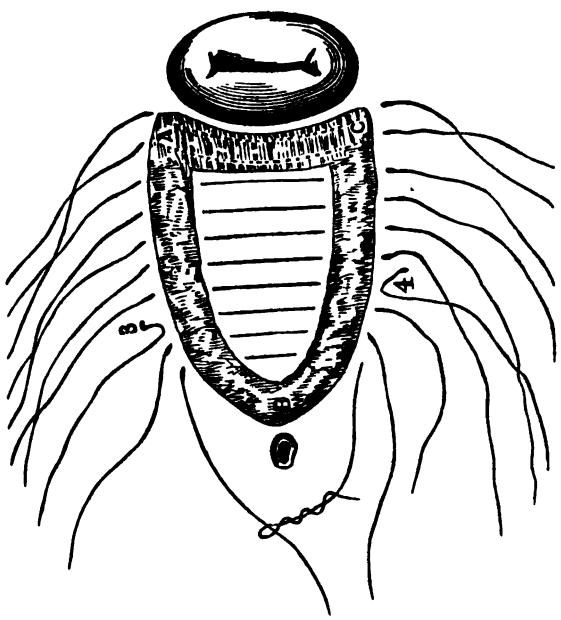
The scarified surfaces were then connected from A to C, just in front of the cervix uteri, for the purpose of closing the pouch; thus forming, when brought together, a firm, unyielding support, and rendering it impossible for the neck to slip behind the septum.

The two sutures nearest the cervix uteri were passed as a running stitch, so as to include as nearly as possible the entire scarified surface of the transverse section. The others, from above downwards, were made to skip over the undenuded triangle, and only to embrace the opposite scarified surfaces. The last one, however, at the neck of the bladder, passed from side to side, including the whole tissue, as at the cervix uteri. Thirteen interrupted silver sutures were used; they were removed on the thirteenth day, and the case discharged cured November 2.

Ten months afterwards she was delivered of a very large child, and had a natural labour. Several months after her delivery, I made an examination, and found the organ in a healthy condition, with no trace of the operation remaining in the vagina. By passing a sound into the bladder, and the finger approximated by the vagina, it was demonstrated that the base of the bladder had regained its natural thickness.

On inquiry, I learn that, up to the present time, she is in perfect health, and has continued to follow her occupation as a washerwoman, with no recurrence of the procidentia.

Mrs. P., aged 63, the mother of eleven children, was admitted December 31, 1862, with complete procidentia of twenty years' standing. She was operated on the next day, as in the previous case, using eighteen sutures. She proved very unruly, refused to take any opium, and constantly made the most violent efforts in straining, with the idea that a movement of the bowels was imminent. The sutures were removed on the fourteenth day, a perfect union had taken place, and she was discharged cured, February 14, 1862.



Eighteen months after the operation she visited the institution for examination. It was found that all traces of the operation had disappeared, with the exception of a small fold remaining in front of the cervix uteri. Up to the present time there has been no return of the procidentia.

May 6, 1863, Mrs. H. (the case already mentioned as having been previously operated on by Dr. Sims) again presented herself for relief. It was now impossible to disengage the neck, as the septum had become stretched sufficiently to admit behind it a large portion of the uterus.

May 11, the case having been prepared for the operation, the septum was divided (about two inches in the median line), from above downwards, sufficiently to extricate the neck. A surface was then denuded in front of the cervix (from A to C, as shown in the diagram), connecting the divided edges of the septum. The uterus was then replaced in position, and the surfaces brought together by fourteen sutures. They were removed on the eighteenth day, and the case discharged cured, June 14.

A year afterwards she reported herself well, with a promise to return on any recurrence of the difficulty.

Mrs. C., aged 26, was admitted June 22, 1863. The procidentia was of five years' standing, and complete. Three months after the birth of her only child, the uterus escaped from the vagina, and had never been returned. June 29, the operation was performed, using sixteen sutures. They were

removed on the eleventh day, and the case discharged cured, July 20, 1863.

Mrs. W., aged 64, a nurse by occupation, was admitted Nov. 24, 1863. The procidentia was complete, and for many years had been unreduced. It came on immediately after the birth of her only child, at that time about 25 years of age. Thirteen sutures were used; they were removed on the ninth day, and the case discharged cured, December 18.

This patient, in excellent health, reported herself for examination, December 15, 1864. As in other cases, no trace remained of the operation, with the exception of a slight ridge or fold in front of the cervix, and this even could only be detected by means of the finger.

Mrs. O., aged 27, the mother of two children, the eldest being six years of age, was admitted January 5, 1864. A few hours after delivery, she got up for the purpose of washing her clothing. She soon felt the descent of the uterus, but the procidentia did not become complete for six months afterwards. In 1862, she again conceived, and states positively that the procidentia remained unreduced from the first occurrence, until the latter months of pregnancy, and returned immediately after delivery. On admission, the sound passed nearly four inches to the fundus, and the neck was immensely hypertrophied. January 16, I operated, using seventeen sutures. They were removed on the 12th day, and the case discharged cured, February 14, 1864, with the promise to report herself if any recurrence took place.

Mrs. S., aged 64, admitted January 25, 1864. She was the mother of eight children, the youngest being twenty years of age. On the eighth day after delivery, she got up, and shortly afterwards began to suffer from a partial prolapse, but it was several years before the uterus escaped entirely from the vagina. For five years previous to admission, it had been unreduced, and during that time the necessity always existed for a partial reduction, before the bladder could be evacuated. January 25, the operation was performed, using fourteen sutures. During the night, she got up and walked about the ward for several hours, and continued, in spite of all remonstrance, to follow her own inclination. On the twelfth day, it was discovered that four sutures (near the neck of the bladder) had torn out, and through the gap a portion of the relaxed base of the bladder protruded. The sutures were all removed at the time, and every hope of success abandoned. Before her discharge, it was found on examination that the entire line of union had gradually parted, with the exception of the cross scarification, in front of the cervix uteri. The fold thus formed (as in a sling) had retained the organ perfectly in place, although below a cystocele existed. The result was attributed to the fact of her being of a spare habit, with a vagina short, in consequence of her age. I have been informed within a few weeks, that there has been no return of the displacement, and, although partially in her dotage, she continues still to lead an active life.

Future experience must demonstrate how far the formation of this fold can alone be relied on under other circumstances; yet it is evident that in many cases this will prove all that is necessary to retain the uterus in situ. When atrophy of the organ has taken place, and the vagina shortened consequent upon a change of life, it is believed, after this fold is formed, that no great amount of the vaginal wall can prolapse.

Mrs. D., aged 42, the mother of eight children, was admitted February 8, 1864. On the third day after the birth of her first child (fourteen years previous to admission), she got up, and the uterus came down to the outlet of the vagina at once. Complete procidentia gradually resulted, and continued unreduced, except during pregnancy. A change of life had already taken place some two years previous to admission. The operation was performed February 10, using sixteen sutures; they were removed on the tenth day. The four lower ones were found to have cut out. The surface, however, gradually healed by granulation, and the case was discharged cured, March 1, 1864.

Mrs. P., aged 39, the mother of two children, was admitted April 5, 1864. Four years previous to admission (while pregnant with her last child), the

uterus remained very low down in the vagina. At the eighth month it partially protruded, and remained in this position until a few hours before delivery. She confined herself to her bed for six weeks afterwards, but immediately on getting up the procidentia became complete. Operated June 12, using seventeen sutures. They were removed on the tenth day, and the case discharged cured, July 4.

Mrs. S., aged 41, the mother of two children (the youngest twelve years of age), was admitted May 12, 1854. The procidentia had been complete for nine years, but she was able to reduce it herself, and was comfortable by retaining the uterus within the vagina by means of a bandage. The operation was performed May 24, using sixteen sutures, and the case discharged cured, June 20 following.

Since October, 1862, I have operated by this method in public and private practice on seventeen cases successfully, and with every reason to believe that there has not been a single recurrence of the difficulty. I have avoided giving the history of any but of those residing in the city or immediate neighbourhood, where a frequent examination has been possible, and their exact condition known at the present time. In other cases failure has resulted; but in every instance it was in consequence either of great tension, from including too much tissue at the time of operating, when the result was often anticipated, or to a want of necessary care on the part of the patient afterwards.

Previous to the operation, if much excoriation exists, it is necessary to reduce the procidentia, and keep the patient in bed for a few days. One or two applications of a weak solution of the nitrate of silver will usually heal any excoriated surface; and large vaginal injections of warm water, night and morning, will be sufficient in a short time to remove the dry and scaly condition of the long-exposed surface. If more than this is necessary, after each injection a portion of damp cotton should be used, thoroughly saturated in glycerine, and carefully spread out so as to come in contact with the entire canal, the whole being kept in situ by a bandage. This will, by the watery discharge induced, greatly lessen the congested condition of the blood vessels, and so corrugate the vaginal walls, for the time being, as to afford quite a support to the uterus.

The patient, having had the bowels thoroughly moved the night before, should be placed for the operation on the left side, in Sims' position for the operation of Vesico Vaginal Fistula, or uterine examination, and his speculum used.

It is often exceedingly difficult to keep the proper line for scarification, in consequence of the mobility of the parts. The process, however, is greatly facilitated by an assistant firmly depressing the prolapsed base with a sound, directly in the median line—a resort often made use of by Dr. Sims. But, even when the assistant is accustomed to the operation, the sound will frequently slip to either side of the cervix uteri. This being generally hidden from view, and having no other guide, the scarification will be found deflected. To obviate this, I often bend the end of a flexible plated copper sound, in a circle sufficiently large to slip over the neck as a collar, and again properly curved on itself, so as to keep up the base at the same time. I am now having an instrument made for the purpose of disconnecting the collar from the shaft. After the two sutures nearest the neck have been introduced, the passage of the others would be greatly facilitated by preserving the relative position of the parts until all have been passed. It can then be disjointed and withdrawn, as a staff, without difficulty, and the sutures secured. On referring to the plate, it will be observed that the usual pressure exerted by the instrument has been lessened for the purpose of showing its application; by so doing the parts have rolled out, and the scarified surfaces have become proportionately separated. To appreciate the condition represented, it must be borne in mind by those who are not familiar with the position, that the patient is represented as lying on the left side.

It is evident that the sulcus produced by this instrument must be very nearly in the median line, and the cervix held in position at the end of the

vagina. Along the edge of the sulcus formed, a broad strip of the mucous membrane should be denuded, extending (as we have shown) from the neck of the bladder to just beyond the cervix on each side. The instrument is then removed, and the uterus drawn down with a tenaculum, for the purpose of making the transverse or connecting line of scarification in front of the neck.

For removing the mucous membrane, I have been in the habit of using various curved scissors. As the hemorrhage is sometimes excessive, the surface can be thus removed more rapidly than with the knife.

The sutures in the transverse section should be passed with great care, and as nearly continuous as possible. The surface is too long a one to be embraced by a single needle, so that at each stitch the point must be inserted again near the exit of the preceding one. Otherwise, instead of smooth surfaces being approximated, they will be brought together as by a purse string.

I have had a needle made for use in this operation, as well as for vesico

vagina fistula, short, round, and slightly curved near the point, of different lengths, which experience has demonstrated to be preferable to all others. In this form a punctured wound is made, and with no hemorrhage, as often follows the flat needle, with a cutting edge.

It is well, immediately after the silk loops of the first sutures have been passed, to attach the silver wire and draw them through. From the length of the operation, the silk, from being so long saturated with blood, becomes weakened, so that if the passage be delayed until all have been introduced, it will often break, from the resistance in drawing it through so continuous a line. The remaining ligatures should then be passed in rotation (at an average of four or five to the inch), each skipping over the undenuded triangular space, until the apex at the neck of the bladder has been reached.

It is worthy of note, that the want of a uniform success in the application

of silver sutures, may be attributed to the use of too small a number, the insufficient amount of tissue included by them, and to strangulation. Each suture should be passed, as to the points of entrance and exit, as nearly parallel as possible to the previous one. But the same care is not so necessary in relation to the relative distance from the edge of the scarification, provided the sutures are afterwards properly secured. Reference is here made more particularly to this operation; for it is exceedingly difficult always to bring the point of the needle out, exactly at the same distance from the edge as that of entrance on the other side. Consequently, the amount of unscarified tissue approximated with the denuded surface must be in proportion to the difference, and either no union takes place or to an extent insufficient to be permanent. Before twisting, this may be obviated by bending a shoulder on the suture at the same point as would correspond with that of entrance from the edge, on the opposite scarified surface. This will be more readily understood by reference to the diagram. It will be observed that the sutures (3 on one side and 4 on the other) have been bent in this manner, and what the obvious result would be, if twisted together, without this expedient.

In some cases, from fifteen to twenty sutures are required, with double the number of ends projecting from the vagina; they become tangled together and prove a source of great embarrassment to the operator, long before the requisite number have been passed. Dr. Sims' usual method was to make a number of incisions into the side of a piece of soft wood, and, in rotation, from one end to the other, each pair of sutures were embedded in a separate slit. He sometimes also used a small comb for the same purpose. The facility thus given by some such expedient can alone be appreciated by the operator, as the order is preserved, and each suture remains undisturbed, and until required in turn to twist.

I prefer, however, to pass each wire through a loop made at the other end, as shown in the diagram, by the suture nearest to the neck of the bladder. It is then gently drawn through, until the looped extremity is about the necessary length to twist, and the single end is then passed behind the upper side of the speculum, to the assistant holding it. By passing the sutures from the above downwards, each is thus gotten entirely out of the way. It is true that the same order is not exactly preserved. But, by beginning to twist the sutures in turn, from below upwards, each, when needed, can be readily separated from the others by a little traction on the wire, just above its loop.

They should be twisted just sufficiently to approximate the edges, and never in the line by which they have been passed, or strangulation of the parts cannot be always avoided. Having been grasped by the forceps and drawn firmly over Sims' Fulcrum, they should be bent at nearly a right angle over it, and twisted only up to this point.

They are then to be cut off at about half an inch in length, and bent over flat, alternating to the right and left. This order is of some practical importance as the sutures frequently become imbedded, and, if not all removed, may become, months afterwards, a source of inconvenience.

After the patient has been placed in bed, the bladder should be emptied. The after treatment is confined to giving opium in sufficient quantity to constipate the bowels, and to regulating the diet with the same view. The urine should be drawn off regularly, and, after the second or third day, the vagina carefully syringed, night and morning, with tepid water.

The sutures are generally removed from the tenth to the fourteenth day, and may be allowed even to remain longer, if a large surface has been turned in. The patient should be confined to the bed for about three weeks after the operation, and the bowels constipated, if possible, for the same length of time. After a dose of castor oil (before a movement of the bowels takes place), it is advisable to throw into the rectum a few ounces of sweet oil or flaxseed mucilage. When it has been deemed judicious to remove the sutures at an earlier day, the same necessity for constipating the bowels no longer exists.

The term "narrowing the vagina," in connection with this operation, is objectionable, as only the surplus tissue is included by the sutures. The vagina is in fact only restored by the operation to its natural size. For if, by accident, the scarification is extended beyond a given point, the tension is so great that the sutures invariably cut out. The only change made from a normal condition, is the formation of a fold in front of the cervix uteri. This, however, gradually disappears, and, as we have shown (even soon after the operation), offers no impediment to the progress of labour. The parts turned in, being relieved of all tension, retract and become blended together, so that the natural thickness of the base of the bladder is gradually regained.

By the same principle, cystocele and rectocele (in fact often but the first stages in procidentia) can be even more readily relieved. The form of scarification, however, is somewhat modified. For rectocele, the form is an ellipse, extending from a point within the fourchette, as far upward as necessary. For cystocele, the scarification is more that of an oval, and should be extended from the neck of the bladder to the cervix uteri.

In presenting this method to the profession (a natural sequence to the previous labours of others), it is not advocated that all cases of procidentia should be subjected to this rather formidable operation; for it is a well-known fact, that many cases of recent origin recover perfectly by rest in the recumbent position, astringent injections, and other suitable means. But it should be resorted to beyond this point; when the uterus has become hypertrophied, and before it has remained outside of the vagina for years, unreduced; before the vagina has lost entirely its contractile power, through the usual systematic course of stretching by pessaries, each from necessity being larger than the previous one in use. After the condition has become hopeless, all efforts for relief have been abandoned, and the patient has become more or less incapacitated for the performance of her daily duties—in such cases (so long an opprobrium to the profession), it is believed that at length a definite result has been gained for this otherwise intractable condition.—*New York Medical Journal, April*, 1865, *p.* 1.

98.—ON A CASE OF OVARIOTOMY IN WHICH THE CYST WAS IN A STATE OF GANGRENE.

By Thomas Keith, Esq., Edinburgh.

[Though the practice in the following case seemed at the time to be rash and scarcely justifiable, yet it saved the life of the woman, and it appears to be one which may be sometimes recommended when bad symptoms come on after tapping. Symptoms indicative of low cyst inflammation came on a few days after tapping, and it was determined to give the patient a chance of life by the prompt removal of the tumour.]

I confess that, on uncovering the abdomen after she was under chloroform, I almost hesitated to proceed, for the intestines were now very much distended. The cyst was exposed midway between the umbilicus and pubes, and as far as the finger reached was unattached. About half a gallon of fluid, which had accumulated in the cyst since the tapping, was withdrawn, and its gangrenous odour was at once perceived by all who were present. The aperture made in the cyst was then carefully closed, the incision was enlarged upwards and downwards, and very extensive intestinal adhesion came at once into view. There was no parietal adhesion except over a space the size of the hand, close to the false rib on the left side. This was very firm, and the bleeding from it very troublesome. The adhesions between the different folds of small intestine and to the tumour were of greater extent than I had ever met with before and embraced the whole of the posterior surface of this large cyst; while the transverse colon was attached by recent lymph to its upper margin. The intestines were of a dark cherry colour, and in a high state of inflammation; their swollen, spongy, friable condition was very

remarkable, and it required the utmost care and tenderness in effecting their separation from the posterior wall of the cyst, which was of a very dark colour. After an hour the whole mass of cysts was turned out, but of course the recent adhesion that existed in all directions between the different coils of bowel was not interfered with. The operation had been sometimes most embarrassing, for the bleeding was profuse, and, from the constant straining of the patient from the chloroform sickness, the swollen intestines were continually being forced downwards, obscuring the view, and pushing out of sight the newly separated bowel, often before I had satisfied myself that the bleeding had ceased. Numerous clots were then removed by passing several sponges of special softness in all directions amongst the viscera; but it was impossible to bring all the separated surfaces into view, from the amount of adhesion that existed between the different folds of bowel themselves. The blood was allowed to gravitate into the pelvis, and thence removed, though several small clots must have been left behind. The oozing from the separated parietal surface gave a good deal of trouble, and it was necessary to keep up pressure on it by the hand, and allow a large flat coagulum to form, which was left adhering to the wall. The pedicle was thick and of fair length; but, from the unusual size and depth of the pelvis, and the distended condition of the abdomen, there was a considerable strain upon the uterus when it was secured by the clamp. The wound was then closed by deep and superficial silk sutures, and it was remarked, as the last look was taken of the intestines—glued together as they were with shreds of recent lymph visible in all directions—that they were now, from the local bleeding they had received, of nearly their normal colour. The patient was under chloroform for nearly two hours.

The cyst-walls weighed 8½ lbs., and the large cyst had contained upwards of 49 lbs. of fluid; so that the weight of the whole tumour was 57½ lbs. On opening the large cyst, it was found in a complete state of decomposition. Its anterior wall, which was half an inch in thickness, was lined with a very thick layer of recent fetid lymph. The greater part of the posterior wall was in a state of complete slough, extending through to its cerous covering. The base of the tumour was about an inch and a half in thickness, and was of cartilaginous hardness.

[The improvement that followed was remarkable; she was out of bed in three weeks. This was Mr. Keith's twenty-fourth case of ovariotomy in two years, and the eighth successful case in succession. Of these 24, six died, eighteen recovered and are now in good health.]—*Lancet*, *July* 8, 1865, *p.* 36.

Art 99.—ON CHOLERA: ITS PATHOLOGY AND TREATMENT.

As this formidable disease is once more amongst us, we will again, as we did in 848, collect some of the most important information respecting it: especialy with regard to its Pathology and Treatment. The history of cholera may be very interesting and its cause mysterious, but it is our chief duty in this volume to give such information as may be especially available and useful at the bedside. We have learned much both as to its nature and treatment from our own observations in all the attacks of this disease since about 1832, and from the experience of others: we have learned how to avoid opium, stimulants, and other kinds of treatment which used to be the fashion; we have learned that from some mysterious poison acting at first apparently on the organic nerves, and then on many of the cerebro-spinal nerves, the arterial system becomes violently and spasmodically contracted—the capillaries at one end, and the pulmonary artery and its branches at the other end—so as to collect nearly all the blood in the great central veins, causing the most rapid collapse, and almost completely impeding the circulation through the lungs. We shall make extracts from the papers of Dr. Bell, Dr. G. Johnson, and others, so that our opinions may be coincided with or not. It appears most probable that the subtle poison acts, either through the blood primarily or through the nervous system, especially on the organic nerves, the sympathetic, which supply the contractile power to the arteries, the intestinal canal, the organs of respiration, including the muscles of voice—producing a kind of tetanic contraction, thus emptying all the capillaries into the great central veins. In fact it seems to us to be a kind of *tetanus*. We all know what a small portion of strychnine will act on the body, and why should not a similar effect be caused by the poison of cholera; but on rather different parts of the nervous and muscular systems from those acted upon by strychnine? If we take this view of the disease, we see at once the reason why some of the most successful modes of treatment went on the principle of relaxing spasm, such as bleeding, emetics, the injection of warm water and salines, and especially chloroform; we also see the bad effects of opium and stimulants given alone. The *combination* of such measures as relaxed spasm with careful stimulation was often indeed successful, but not stimulants alone. The virulence of the poison is indeed so great in some cases, or the patient is so feeble to begin with, that the first attack of spasm may be so short as not to be perceived, and may have entirely passed off before the physician has seen him, leaving the patient in that state of collapse which seems almost hopeless. We would therefore particularly point out to our readers that the successful treatment by chloroform and other antispasmodic remedies is only applicable in the first spasmodic stage, or when this spasm of the organic and voluntary muscles continues to come on. At the time Dr. Bell treated these cases by bleeding, chloroform was not in use. Bleeding, then, was the most rapid and powerful means of relaxing spasm of all muscles—chloroform has now almost entirely superseded it. The paper which seems to us to have thrown most light on this disease was published by Dr. C. W. Bell, Physician to the Manchester Infirmary, and late Physician to H. M. Embassy in Persia. When the reader has read this paper and the succeeding one by Dr. George Johnson, he will be struck with the similarity of views. Although Dr. Bell thinks that the spasm is chiefly in the capillaries, thus emptying all their blood into the great central veins, and Dr. Johnson thinks the contraction is in the branches of the pulmonary artery, yet both seem to think that the fault is in the muscular contraction of more or less of the arterial system. For our own part we think the spasm attacks all the organic muscles, and most of the voluntary, and seems to be tetanic. Dr. Bell's paper will be found more at large in *Retrospect*, vol. xvii., extracts from which we shall give as we proceed.

The views of Dr. GEORGE JOHNSON, of King's College, coincide in many

points with those of Dr. Bell, although we suspect that he never read Dr. Bell's paper, for in another lecture he says that Bell, Orton, Scot, &c., "none of these writers have any very definite pathological theory. They looked upon cholera as a disease chiefly of the nervous system." We were surprised to read this opinion of Dr. Johnson, and we think that he will alter it should he read this paper. Dr. Johnson says :—

"What is the pathological explanation of this remarkable train of symptoms? The one great central fact is this, that, *during the stage of collapse, the passage of blood through the lungs, from the right to the left side of the heart, is in a greater or less degree impeded.* Let us now consider the evidence of there being this impediment to the pulmonary circulation; and let us endeavour to ascertain the probable cause and consequences of this obstruction to the flow of blood. Very conclusive evidence as to the existence of impeded pulmonary circulation during life is afforded by the appearances observed in the heart, blood-vessels, and lungs after death. The *post mortem* appearances within the chest have been described, with more or less minuteness, by several authors; but by no one, I believe, with so much care and accuracy as by Dr. Parkes. (*Researches on the Pathology and Treatment of the Asiatic or Algide Cholera.*)

"In the great majority of cases in which death has occurred during the stage of collapse, the right side of the heart and the pulmonary arteries are filled, and sometimes distended, with blood; while the left cavities of the heart are generally empty, or contain only a small quantity of blood the auricle being partially and the ventricle completely and firmly contracted. The tissue of the lungs is, in most cases, of pale colour, dense in texture, and contains less than the usual amount of blood and air. With respect to the extremely anæmic condition of the lung, when death has occurred during the stage of collapse, there is an entire agreement amongst those authors who have most accurately described the *post mortem* appearances. There is something surprising, as Briquet and Mignot observe (*Traité Critique et Analytique du Choléra Morbus*), in the contrast between the almost constant occurrence of this extremely anæmic condition of the lung, from which scarcely even a few drops of blood flow when the tissue is cut, and the hyperæmia of most of the other viscera.

"There is a remarkable contrast, too, between this anæmia of the lung when death has occurred during collapse, and the great engorgement of the lungs which is almost invariably found when death occurs in the febrile stage which often follows reaction.

"Now, it is evident, from the appearances here described, that, during the stage of collapse, there is an arrest of blood in the branches of the pulmonary artery *before* it has reached the pulmonary capillaries. The arrest at this point explains the remarkable anæmia of the texture of the lungs, while the hyperæmia of the lungs after reaction is due to engorgement of the pulmonary capillaries. Before I attempt to explain this remarkable arrest of blood, it may be well to allude briefly to certain phenomena in the living but collapsed patient which afford confirmatory evidence that the pulmonary circulation is greatly impeded. The impeded flow of blood through the lungs, resulting, as it must, in a very scanty supply of blood to the arteries, accounts for the character of the pulse in cholera. It accounts, too, for the fact that the pulse has often been observed to increase in power and volume under the influence of venesection, which, by relieving the over-distension of the right cavities of the heart, increases their contractile power. (See Dr. Reid's essay *On the Effects of Venesection in Renewing and Increasing the Heart's Action under certain Circumstances.*)*

"I believe the true explanation of the arrest of blood in the lungs to be this. The blood contains a poison whose irritant action upon the muscular tissue is shown by the painful cramps which it occasions; the blood thus poisoned excites contraction of the muscular walls of the minute pulmonary arteries, the effect of which is to diminish, and in fatal cases entirely to arrest, the flow of blood through the lung

* British Med. Journal, Nov. 11, 1865, p. 491.

"We have seen that the condition of the lungs after death during collapse affords conclusive evidence that the arrest of the blood occurs, not in the capillaries, but in the branches of the pulmonary artery, before the capillaries are reached by the blood. We know that the walls of the arteries are muscular, and that they have the power of contracting upon their contents under the influence of a stimulus, such as cold, electricity, or mechanical irritation. I suppose that no physiologist at the present day would deny that spasm of the arteries is as real a fact as spasm of the muscles."*

"The most interesting and conclusive evidence, that arrest of blood in the lungs is the true key to the pathology of choleraic collapse, is to be found in the simple yet complete explanation which it affords of all the most striking chemical phenomena of the disease—the imperfect aëration of the blood, the fall of temperature, the dark and thickened appearance of the blood, and the suppression of bile and urine.

"*Chemical Consequences of the Obstructed Flow of Blood through the Pulmonary Arteries.*—It is obvious that the stream of blood from the pulmonary capillaries to the left side of the heart is the channel by which the supply of oxygen is introduced into the system. One necessary consequence, then, of a great diminution in the volume of blood transmitted to the left side of the heart must be, that the supply of oxygen is lessened in a corresponding degree. This position probably will not be disputed by any one who will give the subject a moment's consideration. Nor, again, can it be denied or doubted that certain results must of necessity follow this limited supply of oxygen.

"The combustion of those constituents of the blood which are normally subjected to the action of oxygen will be diminished in proportion to the deficiency of that gas; and thence follows simultaneously and of necessity a reduction of temperature, and so scanty a formation of urinary and biliary constituents, that, while the state of collapse continues, the functions of the kidneys and liver are virtually suspended. The blood at the same time has that black, thick, treacly appearance which is not peculiar to the collapse of cholera, but which is common to it with all diseases which are attended with a defective aëration of the blood. The blood has this character during the cold stage of a severe ague fit. Dr. Mackintosh and others, who have bled patients during the cold stage of ague, describe the blood as flowing from the arm at first in a slowly trickling stream, being of a dark colour, and not coagulable. (See Mackintosh's *Practice of Physic*, vol. i, p. 86 *et seq.*) Dr. Dundas Thompson, who published an elaborate paper on the Chemistry of the Blood in Cholera (*Med. Chir. Trans.*, vol. xxxii., p. 67), states that one specimen of blood from 'a patient who laboured under an affection of the mucous membrane of the air-tubes' contained a greater excess of solids, in proportion to water, than he had found even in cholera-blood.

"The blood in cholera is black and thick only during the stage of collapse; in other words, during the stage of pulmonary obstruction and defective aëration. This state of the blood bears no relation to the loss of water; it comes on when the loss of water has been very trifling; it passes off rapidly, while loss of water by purging continues unchecked. It is simply a result of defective aëration, just as the thick and smoky flame of a lamp is a result of defective aëration.

"The constituents of bile and urine and carbonic acid are all results of oxidation; none of them can be formed without a large supply of oxygen. Suppression of bile and urine during the stage of collapse is a necessary consequence of the limited supply of oxygen which results from the obstruction in the lungs. The amount of carbonic acid expired during collapse is also much diminished. (See, on this point particularly, Twining's *Clinical Illustrations*, &c., 2nd ed., p. 15.) One fact, confirmatory of the view that the very scanty formation of bile, urine, and carbonic acid during collapse is a result of the diminished supply of oxygen, is, that *the secretion of milk continues apparently undiminished.* I have myself observed this fact, and it has been mentioned by several authors. Thus Magendie states that, one of

* British Med. Journal, Nov. 11, 1865, p. 492.

his patients having been delivered of a child a few days before she was seized with cholera, the secretion of milk continued so abundant through a first and second collapse, which ended fatally, that it was necessary to empty the breasts, in order to relieve the pain which their distension occasioned. Now, if the other secretions are suppressed on account of the deficiency of water in the blood, why does the secretion of milk continue? Magendie says, because the blood reaches the breasts, and supplies the materials for their secretion, on account of those glands being nearer to the heart than the liver and kidneys, which, being at a greater distance, do not receive the supply of blood necessary for the discharge of their functions! My explanation of this remarkable and instructive fact is simply this. The chief constituents of milk—casein, sugar, oil, and water—may be obtained from the blood without the addition of oxygen. The secretion of milk, therefore, continues during the stage of collapse; while the highly oxygenised secretions are suspended, their essential constituents being formed only in very minute quantities during that stage on account of the defective supply of oxygen.

"*Explanation of the great temporary Relief resulting from the Injection of a Hot Saline Solution into the Veins.*—No theory of collapse can be considered complete which does not give a satisfactory explanation of the great temporary benefit which immediately follows the injection of a hot saline solution into the veins. I have before referred to this subject (see *ante*, p. 178); and I deferred my explanation until I had given some account of the phenomena of collapse.

"I have adduced many facts and arguments in proof of the position that the essential cause of collapse is an arrest of blood in the lungs, occasioned by a spasmodic contraction of the muscular walls of the pulmonary arteries. If this be the actual cause of collapse, we might, *à priori*, expect that for a time it would be removed by the injection of a hot fluid into the veins. The temperature of the fluid injected by Dr. Mackintosh varied from 106° to 120°; but he states that 'the good effects of the injection were rapid in proportion to the heat of the solution.' (*Op. cit.*, vol. i., p. 365.) The hot fluid, rapidly mixing with the blood in the right side of the heart and in the pulmonary artery, would as it were dilute the poisoned blood, and render it less irritating, just as diluents render the urine less irritating to an inflamed bladder or urethra. It is probable, however, that the chief action of the injection would be to relax the spasm of the minute arteries by its high temperature.* Thus the impediment to the circulation being overcome, the blood rapidly flows on to the left side of the heart and the arteries, and the phenomena of collapse pass away with marvellous rapidity. The benefit, however, is of but short duration; for the primary cause of the impeded circulation—namely, the poisoned condition of the blood—being still in operation, and the originally hot solution being cooled down by its diffusion through the entire mass of the circulating blood, the stream of blood through the lungs will soon again be obstructed; and the patient thus passes into a state of collapse as profound as and more hopeless than before. It appears, therefore, that the hot saline injection into the veins and the operation of venesection, when it rapidly relieves, as it often has done, the symptoms of collapse, have this effect in common, that they facilitate the passage of the blood through the lungs, and thus lessen that embarrassment of the pulmonary circulation which is the essential cause of choleraic collapse. But, whereas the hot injections act by removing the impediment which results from spasmodic contraction of the arteries, venesection acts by relieving over-distension of the right cavities of the heart, and thus increasing the contractile power of their walls.

* Dr. Parkes, who tried the injection in some cases, states that he did not see even the temporary vivifying effects which others describe. (*Op. cit.*, p. 219.) The probable reason of this is, that the temperature of his injection was too low. In one case, he says it did not exceed 98°; while in another it was "tepid." In the other cases, the temperature is not stated. I think, therefore, that Dr. Parkes's failure to do good by a tepid injection is confirmatory of my view, that the high temperature of the injection employed by Dr Mackintosh and others had more influence than the mere mixture of water with the blood. I believe that the hottest room of the Turkish bath would quickly relax the spasm of the pulmonary arteries; and so, if it did not cause faintness, it might be a real help to recovery.

"In concluding this communication, I would implore those pathologists who have hitherto thought that the temporary benefit following injections into the veins affords conclusive evidence that collapse results from loss of water, to consider whether the explanation here offered is not at least as probable, as complete, and as consistent with all the known facts of the problem, as that which is commonly received."*

These views of the pathology of cholera will be seen to agree very much with those of Dr. Bell, but we think that Dr. Johnson *localises* the arterial spasm too much to the pulmonary arteries. It is well known that Dr. George Johnson strongly advocates purgatives in these cases, to eliminate the poison. We think his success may be encouraging, but his explanation is not, to us, satisfactory. We would by no means discourage either vomiting or purging, but in most cases nature herself does quite enough in this way, without being assisted.

Dr. G. WALLIS, of Clifton, Bristol, believes that cholera is produced by the decomposition of animal and vegetable matter. He does not regard it as contagious. He says:—

"It is true that women who washed the soiled linen of those who have died of this epidemic sometimes were affected by this disease; but it must be remembered that heat and moisture conveyed the poison to the lungs. All those who are weak, irritable, and highly susceptible are often knocked down at once by the deleterious atmosphere of cholera; but stronger persons who have not stayed too long in the tainted air recover. This fact accounts for the uniformity of the mortality in the different countries over which it passed, and the different modes of treatment which have been adopted in the various places. So that medical treatment can do nothing in some cases; they either die or get well, according to the strength of the vital power or the force of the poison on weak habits. Medical aid can only be useful in such cases as are on the balance, and are struggling to continue the vital functions. Sanitary measures are, no doubt, useful, because all malaria add to the destructive power of the epidemic.

"The cholera never appeared in this country until the year 1832, although malaria of various kinds were extensively in existence; cesspools, stagnant ditches, and manure heaps were far more commonly distributed over the whole country then than they are now or have been since 1832; so that malaria has nothing whatever to do with the production of cholera. Poverty and overcrowded rooms may produce low fever, but they never produce cholera or typhus fever. This is an impossibility, for these diseases are produced by specific agents.

"The phenomena which are exhibited when the deleterious air has been drawn into the lungs are these: The great gastro-pulmonary nerve is either wholly or partially paralysed; the consequences are the cessation of all its functions, either wholly or partially. This great nerve is a nerve of function, and performs the functions of digestion and respiration, and influences all secretions. Air is drawn into the lungs, and is expelled in the same state as it was when taken in, except its having been in contact with a moist animal surface; that air remains cold, and there is no secretion of carbon (the essential principle of respiration), so that this process of the vital actions is, in effect, abolished, and the skin assumes a blue colour. The muscles which draw the air into the lungs remain unaffected, and continue their action. In most cases (when the victim is not at once destroyed) the bowels act once largely, and then all secretion is suspended. It will be seen at once that remedies can be of no use in stopping the progress of this disease; except when the vital powers are struggling to continue their functions, they may be assisted by helping them to overcome the difficulties that obstruct them.

"The most painful effect of this disease is the universal spasm or cramp which pervades the whole body, and this arises from the condition of the stomach. Although fighting symptoms is a very bad mode of contending

* British Med. Journal, Nov. 11, 1865, p. 491.

against a disease, yet relieving pain, and so far lessening the evils of the sufferer, is a legitimate object of humanity. As the air which produces this disease is limited, the sufferer should be at once removed to a purer atmosphere. The most effective remedy in relieving all spasms is the spirit of turpentine, one drachm of which is an ordinary dose, but its quantity must be suited to the object required. It is not intended to recommend remedies for the relief of cholera; it is merely intended to be a guide to younger men in the consideration of this formidable epidemic. No epidemic can be contagious, as the state of the air breathed is the exciting cause of this disease. Ignorance alone produces fright, and fright magnifies all horrors."*

We agree with Dr. Wallis that in almost all cases, *at the very commencement*, this spasm or cramp exists more or less, but this may pass off so rapidly, and be followed by such complete collapse, that it may even escape the notice of the physician. His success, however, depends on this knowledge of the nature of the attack.

Dr. W. G. MAXWELL, of Calcutta, corroborates many of the foregoing remarks:—

"The development of the stages of fever entirely depends on the changes the leaven has effected. If this change has been such that the blood has become too thick to flow through the lungs, then, as a matter of course, the collapse stage is developed in excess; in other words, cholera asphyxia is exhibited. The blood, unable to pass through the middle passage into the arteries, collects and swells out the veins, giving that deadly or blue colour to the skin. When the vomiting and spasms come on, this mass of blood in the veins is squeezed with great force, and hence the clammy moisture that is forced from every part during these fits. There is no pulse, because there is no blood in the arteries. There are also lethargy and languor, and oppression in breathing, caused by the blood being all collected in the veins. These make up the principal links of the chain of mechanical symptoms."†

Dr. Maxwell had a violent attack in his own person, and found the greatest relief from effervescing soda powders mixed up in large quantities of water. He took fifteen tumblers full of this effervescing draught with the greatest relief, although frequently vomiting them as soon as taken.

Another valuable paper is by Dr. MASSY. He says:—

"The symptoms increase if not checked by medicine; cramps of the most violent description come on, chiefly in the feet, calves of the legs, thighs, and hands, which cause such excruciating torture, that I have rarely met even the most resolute able to resist screeching out. The skin now becomes quite cold, assumes generally the blueish hue so remarkable in cholera, and in a short time you will find it corrugated in almost every part, reminding one exactly of a woman's hand after washing for several hours. You will also be struck by the very peculiar countenance of the patient; the features are all sharpened; the eyes sunk in the orbits, but starting wildly; the tongue, if you place your finger on it, will be found quite cold; the patient's breath, if he breathes on your hand, is also cold. I shall never forget the first time a man in the advanced stage of cholera breathed on my hand—it actually made me start, it was so much colder than the atmosphere; all this time the patient screams out for cold water to drink: if you ask him is he cold, he says no, he is burning hot, he wants something cold to drink. I never actually tested how much a man in this state would drink, but fancy it would be something enormous, for he generally throws it up as fast as he drinks it;—sometimes, one would fancy, by a spasm of the œsophagus, for it has hardly time to reach the stomach, and still while he is vomiting he stretches out his livid hands, crying more, more!

"The treatment of this case depends in the first instance on bleeding, and largely, if the patient's pulse is good, giving at the same time twenty grains of calomel with one of opium. This, I think, will be found the best practice, at least we found it so in the hospital of the 31st Regt. Often have I seen the purging and vomiting cease almost instantaneously. The calomel and

* Lancet, Nov. 11, 1865, p. 544. † Retrospect, vol. xviii., p. 389.

opium in the first place act as an immediate sedative, and the bleeding relieves the circulation more than purging, removing a mass of blood instead of depriving it of its fluid part. After twenty minutes give ten grains more of calomel and half a grain of opium; this will further prolong the cessation of the symptoms, and in about an hour you will find the calomel acting freely on the liver, when quantities of bile will be passed by stool. When it has acted in this way for some time, you may give opium, but not till then: of the various species of practice I have seen tried, I think a reliance on opium in this form of cholera the most faulty: you may exhibit any amount of opium, while a man is purging and vomiting white fluid, without producing a single sign that the medicine has been swallowed—the purging and vomiting still continue. If you happen not to see your patient (as is too often the case among soldiers) until the disease is farther advanced—until his voice is changed, pulse a good deal reduced in strength and frequency, features sunken, temperature reduced, skin blueish, tongue cool, &c.—I would recommend the bleeding to be smaller. You may be able to repeat it; but, as you draw blood, stimulate, give punch, brandy, or wine and water, or carbonate of ammonia. Apply friction, with stimulating and hot liniments to the extremities, warm sandbags to the feet, sinapisms to the calves of the legs and pit of the stomach; for, if once you can raise the pulse, the chances in favor of recovery will be vastly increased. I have seen more than one case where the pulse improved after this treatment and again fell away, when it was deemed advisable to take blood a second time with decided advantage, stimulating at the same time. Cases of this kind cannot endure the loss of blood, even in small quantities, without stimulants. This we learned by sad experience at Umballa; most of the cases we bled in this stage sunk without a rally. We were about to condemn bleeding in cholera, when once the symptoms of collapse set in, but we tried stimulants together with the bleeding, and found the practice the most successful of all we adopted." *

In this paper Dr. Massy points out the practice of bleeding and stimulating at the same time, that is, relaxing the cramps and spasm of the arteries, and judiciously keeping up the powers of nature while this depressing treatment was carried out. This can be done beautifully by substituting chloroform for bleeding, and even by combining the internal administration of compound spirit of ammonia and good large doses of chloric ether. The chloric ether being made from chloroform, acting as it were both as a stimulant and sedative, or anti-spasmodic,

We have placed before our readers some of the most valuable papers on this dreadful disease, and we will now endeavour to *pick out* from them and other papers some of the best modes of

Treatment.

We cannot but think that the most serious mistakes have been made in the treatment of cholera, chiefly owing to not knowing what cholera really is. Let us first point out some of what we consider the most sensible explanations of its pathology. Our opinion is, that it consists in a tonic rigidity, spasm, or tetanic contraction of nearly all the arteries of the body, caused by some violent poison acting on the organic nerves, the sympathetic, and all its branches and connections. The small terminal arteries and capillaries being thus spasmodically contracted, the blood is driven inward upon the great veins. Dr. Bell says:—

"When this has reached to such a point as to oppress the action of the heart, yawning first, and then shivering, or a sense of suffocation and pain in the præcordia, are the indications of oppressed circulation, and of the commencing effort of the heart to overcome the mass of blood which is stifling it. If, by the application of tourniquets to the limbs, or by bleeding, part of the blood which is rushing from the extremities to increase this congestion is prevented from reaching the great veins—the heart, excited to increased

* Retrospect, vol. xvii., p. 127

action, is enabled by this relief more quickly to overcome the obstruction and restore the balance of the circulation, and the paroxysm passes off. If not thus mechanically aided, the heart, after a severe struggle to maintain the circulation during the period of constriction, is at length relieved, by this nervous disturbance or spasm of the capillary circulation passing off of itself, and then the heart and arteries, so long excited by the struggle, maintain for a time their increased action after the obstruction in the capillaries is removed, and produce apparent febrile action. Presently this excitement subsides, the vessels become relaxed, and sweat succeeds. The vessels continue in this state for a longer or shorter period, according to circumstances, till they at length recover their ordinary tone and action in the intermission. This fever, however, is not fever properly so called, but reaction; and the sweating not critical or essential, but relaxation. The cold stage is alone essential, and is the physiological cause of the subsequent stages."*

"Collapse, if it mean anything, surely means a sinking from the cessation of the power which supported the vital actions, but the more the condition so called in cholera is examined, the more, I am convinced, it will be found that it arises from an active cause controlling these powers; that, if collapse from sinking or hemorrhage may be termed adynamic, we shall be forced to express that which occurs in cholera by the contradictory term dynamic; that it is an active, not a passive, condition of the capillary circulation. It is obvious that, if this distinction be not made by the practitioner, he will inevitably attempt to treat collapse in cholera on the same principles that he would adopt in an apparently similar condition of the system proceeding from a deficiency of action. It is true that both in adynamic collapse with sinking and syncope, and in that of cholera, which I have termed dynamic, there is diminished circulation and arterial action; but in the first the pulse is open, soft, and feeble, and, unless proceeding from hemorrhage, becoming generally slower, softer, and feebler, till it ceases; whereas in cholera it is contracted, small, and wiry, as long as it can be felt."†

"At the same time that the blood over-distends the cava and oppresses the heart, being still poured inwards from the extremities to which their well-valved veins permit no regurgitation, it is necessarily driven back from the gorged vessels upon all the internal venous branches unprovided with valves; the renal veins are distended, and the returning current of blood from the kidneys stopped; consequently a first effect or symptom of such congestion is the cessation of the secretion of urine, and the first urine passed on recovery from cholera is often mixed with serum and blood-globules. In like manner the hepatic circulation is oppressed, impeding the secretion of bile; these two constitute the second class of mechanical effects or symptoms. At the same time another effect is produced by the same cause: the portal circulation, at first impeded by meeting there flux from the vena cava, is presently reversed, and the blood is thrust back into the mesenteric veins, till it distends their extreme ramifications on the mucous membrane, where it finds a species of vent; for from this surface the repellant force squeezes out the fluid portion of the blood into the bowels, carrying with it the mucous epithelium, and constituting that serous or rice-water evacuation which is esteemed characteristic of cholera, but which has no title whatever to the name of secretion, or to be treated as such, being a mere exudation which is the very reverse of a vital action."‡

There is one important omission in the explanation of Dr. Bell which is well filled up by Dr. Johnson, of King's College. According to this view of Dr. Bell, the blood is forcibly sent into the great central veins, and Dr. G. Johnson beautifully explains *why it is kept there and cannot get any further*. If the road had been clear and uninterrupted through the lungs, the blood would easily have got round to the left ventricle, and have again gone its round, but it is stopped by the spasmodic contraction of the minute branches of the pulmonary artery which will not even allow the blood to enter the pulmonary capillaries, as shown by the remarkable anæmia of the texture of the lungs.

* Dr. Bell, Retrospect, vol. xvii., p. 109. † Ibid., p. 111. ‡ Ibid., p. 118.

"The one great central fact is this, that, *during the stage of collapse, the passage of blood through the lungs from the right to the left side of the heart is, in a greater or less degree, impeded,*"*

"The tissue of the lungs is, in most cases, of pale colour, dense in texture, and contains less than the usual amount of blood and air. With respect to the extremely anæmic condition of the lung, when death has occurred during the stage of collapse, there is an entire agreement amongst those authors who have most accurately described the *post mortem* appearances. There is something surprising as Briquet and Mignot observe (*Traité Critique et Analytique du Choléra Morbus*) in the contrast between the almost constant occurrence of this extremely anæmic condition of the lung, from which scarcely even a few drops of blood flow when the tissue is cut, and the hyperæmia of most of the other viscera."†

This anæmic state of the lungs, however, only continues during the stage of collapse. When reaction follows, the lungs become gorged with blood, while we suspect the other viscera are more or less relieved by the circulation going on once more. Thus the blood is driven inwards from the general capillary circulation, and prevented from getting further on account of the similar contraction of the pulmonary arteries. On the clear understanding of this one point we think depends the whole success of the treatment of the first stage of cholera; and with this view before us we can see the great success which has attended certain modes of treatment. Dr. Bell explains his success by bleeding, by supposing that he thus diminishes the quantity of blood in the great central veins, but this is not a correct or scientific explanation. The blood-letting relieves the spasm of the capillaries, emetics act in the same way, and even the encouraging of the vomiting by water relaxes muscular spasm in the same way as ipecacuanha, antimony, belladonna, and aconite, and in a similar manner we think that the injection of warm water or salines act. But it is to *chloroform* that we especially wish to direct the attention of our readers, to the cases which have been cured by this wonderful agent. We ought always remember that the period of spasm is not uninterrupted, it comes and goes like tetanus; although perhaps always, more or less, existing till nature fairly gives away, and then we have complete prostration and relaxation of all the muscular system, but (if the patient live long enough) with a consequent reaction in the circulation, and congestion of those parts which were previously anæmic. Hence, if we bleed or nauseate, or use chloroform, these ought to be done *only during the period of spasm.* If the spasm be carefully and early overcome, without producing violent reaction by stimulants and other means, the cure of the cases is accomplished. Let us examine some of the most important measures resorted to in this early stage of this disease, for in this consists the great success.

Bleeding.—With respect to this mode of treatment, the reader ought to notice particularly the following directions of Dr. Bell:—

"When the exudation from the bowels or skin is yet flowing freely—for in different forms of the epidemic either symptom may predominate—and when the heart is heard to be struggling in an extraordinary state of excitement, and in the active endeavour to overcome obstruction, it is *churning* as if it would burst—a sound difficult to describe, but, once heard, never to be forgotten—when, I would say, the sound of the heart conveys to the listener the impression that it is labouring with unabated vigour of reaction, the lancet cannot be used too promptly; but when the attack has continued for four or five hours—when the purging has become less copious—and when, instead of manifesting a continuous struggle, the heart appears only to be roused up to reaction at intervals, when the natural warmth of the skin is a little more extended on the neck and chest, and the patient, though still tossing uneasily, is not now in his previous agony, I would hold my hand and trust to medicine for some hours to come; I would then wait until I observed him rest a little more quietly, and bear the weight of the bed-clothes

* Dr. Johnson, British Med. Journal, Nov. 11, 1865, p. 491. † Ibid.

"I determined on giving chloroform, after giving hydr. chlorid. with opium, which was immediately rejected. I gave the following mixture:—Chloroform, ℥vj.; brandy, ʒiij.; water, ʒijss. I gave a third part, which was thrown up in half an hour; I gave him a second dose, which was retained; the vomiting and diarrhœa ceased; the spasm less severe. I gave him, in two hours, the remaining part, and during the next six hours I administered, in two doses, six minims more of the chloroform with the most decided benefit; and he is now, the 17th inst., convalescent. To the extreme tenderness over the region of the epigastrium I applied flannel soaked in rectified spirits of turpentine. I observed there was no urine secreted, and I am firmly of opinion that the usual remedies would not have met this case. I candidly confess I had no hope of success, from its severity, and, but for Mr. Brady's case, I believe I should have lost my patient."*

Mr. STEADMAN gives the following case of English cholera treated by the same remedy.

"About three o'clock on the following morning I was hastily aroused by her husband, as the patient had become much worse. All her symptoms had increased to an alarming degree: the spasm was universal and excessively violent, 'as if knots were being tied in her bowels;' vomiting incessant; countenance livid and cold; articulation feeble, praying to be released from her sufferings. As all the medicines had been rejected, I thought it fruitless to continue them, but at once decided upon administering chloroform. A mixture composed of the following was prescribed:—

℞. Chloroform. gr. xiv.; aquæ vitæ (cong.), ʒj.; aquæ destill., ad ʒvj. M.

"A fourth part was given immediately, which had a partial but most satisfactory effect: an abatement of all her symptoms was the immediate consequence. In two hours a disposition to a recurrence manifested itself, when a second dose of the mixture was administered, which entirely controlled all spasms, vomiting, and purging. She expressed herself 'very comfortable,' and fell into a quiet sleep. At nine o'clock I again saw her, and found her suffering only from some febrile symptoms, accompanied with much exhaustion. She was ordered cold rice and mucilaginous drinks, and had the chalk mixture with nitric ether prescribed. A dose of ox-gall (gr. x.) was given in the course of the day, which produced three bilious evacuations and some disposition to vomiting, which soon passed away. In two days she was declared convalescent. In 1832, when the cholera visited this place, my patient was attacked, but she declares her sufferings then were nothing in comparison with her late disorder. The two remaining doses of the chloroform mixture were ordered to be carefully preserved in case she had any return of her symptoms. A daughter, grown up, who had assiduously attended upon her mother, was on Wednesday evening seized in precisely a like manner, except that the dejections were more abundant and frequent; and the mother, without hesitation or appeal for advice, gave her the two remaining doses of the mixture. The same magic result followed; the first dose was only partial in its effect, but the second completely subdued the disease. When I called on Thursday, the gratifying announcement was made to me of the success of my medicine in a second case.

"Perhaps I am not justified in calling these decided cases of Asiatic cholera, but the disease in its latter stage, in the case of the mother, assumed a much more severe type than our English form usually bears.

"Without offering any remarks upon the *fons et origo* of the malady in its worst form, and with prospective fears for its soon visiting our shores, I am but too happy (in conjunction with Mr. Brady) in being able to report so favourably of a remedy which I believe only requires to be more extensively tested to be appreciated."†

In the "Reports on Epidemic Cholera, drawn up at the desire of the Cholera Committee of the Royal College of Physicians, by W. Baly, M. D., and William Gull, M. D.," the treatment by chloroform is copiously referred

* Retrospect, vol. xviii., p. 491. † Ibid, p. 492.

to, but, with all deference to these eminent physicians, we think that there is not sufficient emphasis placed on its effects as an antispasmodic. By many it was given more as a stimulant than an antispasmodic, moreover it was often given too late, when collapse had too much set in. Although the poison of cholera seems to act primarily on the ganglionic nerves, almost paralysing them, yet at the commencement of the attack the heart and other muscles are in a state of almost tetanic continued spasm; but this continues a very short time, and, if not relieved, is followed by the real collapse, in which stage the powers seem to be exhausted and death soon follows. Bleeding, chloroform, emetics, and all other antispasmodic and exhausting remedies ought only to be used when we want to effect relaxation of cramp and other spasmodic action of the arterial system, the intestinal organic muscles, the muscles of respiration, voice, &c. Hence we find in most of the reports of cases, that the *time* to use these remedies is not carefully pointed out. In the Cholera reports of the College of Physicians, Dr. DAVIES gives one case in which he used chloroform fifteen hours after the seizure, with relief but not success; we think that it was used too late. He further says:—

"In 22 cases, as severe symptoms came on, the chief remedy was chloroform administered internally in doses of from seven to ten minims every hour, half hour, or quarter of an hour, according to the severity of the symptoms, but of these 22, 8 terminated fatally, and 14 recovered."

Dr. Davies (second report):—

"Out of 9 cases of cholera, and 13 of the worst cases of diarrhœa, occurring in my own practice, and treated with chloroform, one died. All these were in the better ranks of life. In some of them, the warm salt-water bath was used as an auxiliary, and the diet consisted of nothing but cold milk and water, with some carbonate of soda ad libitum. The fatal case was that of a drunkard, who probably did not take the remedy.

"These cases varied in degree of severity, from sickness and diarrhœa, and some account of collapse, to sickness, diarrhœa, severe cramps, and great collapse, with almost clear watery evacuations passing away involuntarily.

"Of 14 cases of cholera treated by my friend, Mr. Towers, Medical Resident of the Infirmary, many of them under my observation, one died. The fatal case was that of a woman, aged 63, in a state of great destitution."

Dr. Davies (third report):—

"It will probably be remembered by the committee, that in my second report, I expressed a very favourable opinion of chloroform in this deadly malady. I considered I had strong grounds for so doing, after observing the large proportion of cases which recovered under its administration. From the history of this last visitation in the county prison, however, the fact turns out, that, under some uncertain circumstances, the use of chloroform will not prevent the proportion of deaths being considerable. I have reason to believe that it was, from over anxiety, given in too frequent doses in some cases, and that it thus rather added to the coma, which is one of the characteristics of the malady.

"At the commencement of the outbreak, the doses were repeated every hour, or every two hours, and it is to be noted, that the first 7 cases recovered. As the cases multiplied, the remedy was given every half hour, and, in some instances, every quarter of an hour; the result was, that the next 6 cases died. Whether these cases had any thing in them inherently more fatal, it is difficult to tell; the symptoms at first were about equal, and the differences did not show themselves until towards the end. There was next a recovery of 7 cases in succession; in these the remedy was administered less frequently, but subsequently 2 deaths occurred under the less frequent administration.

"The chloroform was administered also by inhalation in some of the more severe cases of cramps, with the effect of affording relief in every instance. The inhalation was not carried so far as to produce perfect insensibility.

"Although I am still of opinion that chloroform, properly regulated, is the remedy, of all others hitherto tried, to be depended upon, yet it cannot be considered as a 'specific' for cholera. In one of my fatal cases the chlo-

roform mixture had been taken for two or three days, about every four hours, during the stage of diarrhœa, yet the patient sank rapidly into collapse, and died in two days."

Dr. Davies' subsequent statement:—

"I found that no reliance could be placed on chloroform alone. It is true the cramps and pain were greatly relieved by it if the inhalation was carried so far as to produce insensibility, but the course of the disease did not seem to be at all impeded."*

Now, we think that Dr. Davies was more successful than he himself thought: and if chloroform be used very early so as to keep off all spasm, and at the same time stimulants be very carefully given if the pulse do not rally, we think that success will be great.

Hot Mustard Bath.—An interesting case relieved by this remedy is just related by

Dr. BULLAR, who says:—

"The patient was in the collapsed stage of cholera. His naturally red face was of purple blue: his surface livid, cold, and shrunken, with the washerwomen's hands. He had rice-water vomiting and purging, was doubled up with abdominal pain, and writhed with cramps in his calves, drawing the strands of his gastroonemii muscles into rigid cords, which the nurse and porter were rubbing to ease his torture. His pulse was barely perceptible. No urine had been passed since his attack.

"No one who had seen the cholera of 1832 and 1849 could mistake the nature of such a case.

"Had a slipper-bath been available, I should have used one; but as he had been placed in a house adjoining the Infirmary, and only used occasionally for infectious cases, where were only hip- and foot-baths, I had him seated in a hip-bath half filled with very hot water with three-fourths of a pound of powdered mustard in it, with his feet in a hot water foot-bath with a quarter of a pound of mustard mixed with it. The water was as hot as the nurse's hands could bear—almost scalding hot. That the water should not be merely warm, but very hot, and renewed by degrees to keep up the heat, is most important; and the medical attendant should superintend this himself. Gradually his face improved in colour; the cramps ceased; he did not vomit; and after half an hour, as he expressed a great wish to get to bed, and to pass a stool, he was moved to a close stool, dried, a clean shirt put on, and he was placed in bed, well covered with blankets, and with hot tins to his feet and hands. His pulse, though weak, was not easily felt; and his hands, though still cold, were warmer.

"The contrast was striking. When placed in the bath, he was writhing with pain and cramps; when placed in bed, he said he was comfortable, had no pain, and wanted to sleep. Whilst in the bath, he drank freely of milk and water with lumps of ice in it; and was urged to swallow in the intervals of drinking small lumps of ice. Shortly after being placed in bed, he suddenly ejected a large basinful of milk and water, and was not sick afterwards. An hour after he had been placed in the bath, reaction had so far advanced that there were good hopes, and four hours afterwards he was out of immediate danger. He was ordered to take iced milk in smaller quantities, to swallow ice, and to have broth and tea. He had but one stool of rather a yellower colour the same evening. No reaction-fever followed; and, except weakness, he was on the second day well.

"Having seen, with Dr. Lake, a case of much less severity, in which reaction went on from the time he applied Dr. Chapman's ice-bag to the spine, I had one prepared; and, on his being placed in bed, it was applied; but, as it made him chilly and prevented sleep, it was not persevered in. This coldness from an ice-bag is a proof that reaction has occurred, and that it is not needed."†

In this case the hot mustard bath was beneficial not only in restoring the capillary circulation to the skin, but in relaxing the spasm of the organic

* Cholera Reports, p. 192. † British Medical Journal, Oct. 21, 1865, p. 416.

muscles generally, but as Dr. Bullar truly remarks, neither this nor any other remedy will avail when the dose of the poison has been so virulent and so large that "the disease begins with death."

Calomel.—The treatment by calomel was at one time much praised, but we think that it did not deserve all the praise given to it. It was certainly a better treatment than that by opium and stimulants, which was then much practised. It left the cases more to nature.

Cold Water.—This is one of the best remedies. The patient will drink enormous quantities, and perhaps vomit it as rapidly as he drinks it. Nevertheless it ought to be taken copiously and without restraint. Effervescing draughts in large quantities are an agreeable form.

CONSECUTIVE FEVER.—It is too true that even if the first stage of cholera be got over, the patient frequently sinks from the consecutive fever, but we think that the more easily the spasm is overcome, the less stimulants, opium, and other violent treatment be adopted, so much the milder will be the reaction, and consequent fever.

We have not space left us in this volume to dwell at large on the treatment of this stage of the disease. A great number of interesting papers on cholera will be found in our previous volumes. See *Retrospect*, Vols. 17 and 18.

SYNOPSIS.

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

CONTAGIOUS FEVERS.—Fevers are due to the passage from one body to another of living particles. These living particles consist of germinal matter, and have descended by a process of degeneration from the germinal matter of the organism itself, and are therefore of animal origin. To those familiar with vital phenomena, the fact that a terrible and fatal malady may arise from the introduction into the body of a minute germ, weighing less than the $\frac{1}{10000}$ of a grain, will not appear incredible. (Dr. Lionel Beale, p. 28.)

FEVERS, TABES MESENTERICA, GASTRIC IRRITATION, &c.—Give the hypophosphite of lime, soda, or potash. The following formula for an infant is good: ℞. Hypophos. sodæ, gr. vi.; hypophos. calcis, hypophos. potass., ana gr. iv.; glycerinæ, ʒii.; aquæ ʒi. M.; forty drops three times a day in a little water. The dose to be increased according to age. (Dr. H. S. Purdon, p. 20.)

OZONE AS A DISINFECTANT.—Get a wide-necked bottle and put in half a pint of water, with a cork floating at the top, on this cork fix a bit of phosphorus; cover the bottle with another bit of cork very loosely. This apparatus may be moved from room to room, remaining till the characteristic smell of ozone is perceived. Do this night and morning. (Dr. T. Moffatt, p. 28.)

SCARLET FEVER.—*Inunction with Hot Fat or Lard.*—Once or twice in the twenty-four hours let the patient be rubbed with hot fat or lard. This checks and quells the violence of the disorder in an extraordinary manner. Death is a rare result, and dropsical symptoms are almost unknown. The pulse will sometimes fall thirty beats in three hours. (Mr. J. L. Milton, p. 34.)

Cold Affusion.—Cold affusion is a plan attended with such benefit in cases of scarlet fever, with sthenic febrile symptoms, that one is puzzled to know how it has come to be abandoned. (Mr. J. L. Milton, p. 34.)

ZYMOTIC DISEASES.—The fermentation of poisons in the system, generating zymotic diseases, may be arrested by the alkaline sulphites, the same as vinous fermentation is arrested by sulphurous acid. Therefore in typhus, typhoid, scarlatina, small-pox, &c., give scruple doses of sulphite of soda every three or four hours. This dose may be decreased or increased according to circumstances and age. (Dr. De 'Ricci, p. 17.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CEREBRAL PARALYSIS.—*Faradization.*—Faradization is useful in cases of hemiplegia after the complete formation of the apoplectic cyst, when there is no longer any irritation within the cranium. It is more suitable to cases where the arm or leg is nearly or entirely useless, than to those where there is much muscular rigidity. (Dr. J. Althaus, p. 48.)

CHOREA.—*Calabar Bean.*—In a case of chorea recorded, four drops of a tincture of calabar bean were given thrice daily, and gradually increased in quantity during a period of nine weeks. Complete recovery resulted. All means previously tried during the course of several months had proved useless. (Dr. J. W. Ogle, p. 62.)

DELIRIUM TREMENS.—*Subcutaneous Injection of Morphia.*—In cerebral

affections, medicines called anodynes, when introduced subcutaneously, frequently act so far more effectually, and so differently from the stomachic doses of the same, as almost to warrant us in considering them as different medicines. Thus in delirium tremens, half-grain doses of morphia may be given repeatedly without effect, whilst one such dose introduced subcutaneously will frequently produce sleep. The use of the drug in this manner, moreover, is not followed by dryness of the mouth, and sickness and constipation of the bowels. (Mr. C. Hunter, p. 72.)

EPILEPSY.—*Ice to the Spine.*—A most interesting case is related in which the patient was cured of severe epilepsy of long standing, by means of ice to the spine. He had many symptoms of spinal and cerebral irritation. Ice was applied along the whole spine during two hours, four times in each twenty-four hours, one bag being applied at bed-time, one being used during the night, and the other two during the day. He had no fit after this treatment was commenced, although previously he had three fits a day. The cure was permanent. (Dr. J. Chapman, p. 57.)

INCURABLE AND PAINFUL DISEASES.—*Subcutaneous Injection of Morphia.*—By means of subcutaneous injection, life in these cases is made bearable. It may for a long time prove a boon to patients suffering from incurable neuralgia, cancer, or the agony of diseased joints. One-third of a grain of morphia introduced subcutaneously every alternate day, will sometimes give all the relief that two grains given daily had previously done. (Mr. C. Hunter, p. 75.)

INSANITY.—In cases where there is a determination of blood to the brain, with sleeplessness, restlessness, and anxiety, the following plan of treatment, with the object of drawing the blood from the brain, may be pursued with benefit. Let the whole legs and lower part of the abdomen be wrapped up in cloths steeped in mustard and hot water. A wet towel should be applied round the head. The effects are most soothing. (Dr. S. Newington, p. 63.)

INSOMNIA.—In cases of insomnia, bromide of potassium can almost always be used with advantage, to diminish the amount of blood in the brain, and to allay any excitement of the nervous system. (Dr. W. A. Hammond, p. 64.)

NEURALGIA.—*Hypophosphites.*—Try hypophosphite of soda in doses of a drachm three times a day in beef-tea. (Dr. T. H. Jackson, p. 62.)

Hypodermic Injection.—When opiates or other anodynes are injected subcutaneously for the relief of a local neuralgia, it is quite unnecessary to eject the neuralgic site: the medicine will act just as well when inserted in any other part of the body. (Mr. C. Hunter, p. 69.)

PARALYSIS.—*Electricity.*—The interrupted or electro-magnetic current proves useful in local paralysis due to injury of the motor nerves and muscles by pressure, rheumatic effusions, poisoning by lead, &c. The continuous galvanic current, known as "galvanization," on the other hand, proves useful chiefly in forms of paralysis due to affections of the nervous centres. (Dr. J. Althaus, p. 46.)

Paralysis from Pressure on Nervous Plexuses.—Cases of loss of power from continued pressure, especially of the head on the arm during intoxication either by chloroform or alcohol, are generally curable by faradization. (Dr. J. Althaus, p. 49.)

Rheumatic Paralysis.—Cases of loss of power in the extremities, arising from exposure to cold and damp, even if of a severe kind and of long standing, are generally relieved by faradization, which also relieves the muscular pains, which are sometimes very severe. (Dr. J. Althaus, p. 54.)

SCIATICA.—*Croton Oil.*—Give a dose of two drops of croton oil; in many cases cessation of the pain will immediately ensue on the copious action of the medicine. (Dr. S. C. Sewell, p. 59.)

Subcutaneous Injection of Atropine.—A lady had suffered from sciatica for two years, for which she had been obliged to take morphia daily. The subcutaneous injection of $\frac{1}{10}$ of a grain of atropine produced its characteristic phenomema upon the system, and forthwith removed the pain permanently. (Mr. C. Hunter, p. 70.)

Danger of Subcutaneous Injections.—Be very careful not to inject into a

subcutaneous vein instead of into the cellular tissue. As it may be impossible to avoid this at all times, always inject very slowly—in order that at the first sign of danger the injection may be stopped. (Prof. Nassbaum, p. 75.)

SPINAL PAIN.—In all cases of chronic pain on either side of the spine, attended with even the *slightest* undue prominence of the muscles over the transverse processes, an anatomical lesion of the ligamentous and bony part of the spine exists, combined with deviation of the bodies of the vertebræ and rotation. The term hysterical pain is incorrect. For the relief and cure of these cases, mechanical means are requisite. (See description of instrument, page 57.) In cases where the general health is disturbed, appropriate means must be adopted. (Dr. H. Dick, p. 56.)

TIC-DOLOUREUX.—*Subcutaneous Injection of Atropine.*—Inject $\frac{1}{30}$ of a grain of atropine subcutaneously, and repeat occasionally at intervals of two or three days, if necessary. It is not necessary to inject it in the region of the pain. (Mr. C. Hunter, p. 70.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ARTERIAL HEMORRHAGE.—To *tie arteries*, use the *reef-knot* as figured and described at page 137. This method has the advantages of the common ligature and acupressure combined. You can tie the artery securely, and loose the knot at any moment without disturbing the parts, thus obviating the objection of allowing the ligature to remain till it separates in the usual way. (Mr. F. Churchill, p. 136.)

HEART AFFECTIONS.—*Action of Digitalis in.*—Nothing seems more certain than the action of digitalis on the heart. When there is hypertrophy, its administration is dangerous; but in a weak organ, acting, because of its weakness, with great rapidity, it will reduce the number of its contractions, and, as it were, strengthen or tone them down. In a case of valvular heart disease, with dropsy of the legs and abdomen, the use of digitalis in ten drop doses was followed by the disappearance of the dropsy and the reappearance of the pulse at the wrist. (Dr. W. Murray, p. 80.)

No fact is now better established in medicine than that digitalis is capable of diminishing the frequency of the heart's beat, and of increasing the power of the heart's impulse. Nevertheless, when digitalis is given in such cases, the patient ought to be watched day by day, and the medicine discontinued as soon as the heart begins to be more regular and the impulse stronger. There is no proof that sudden death ever results from the administration of digitalis. This is liable to occur in any patient with dilated and feeble left ventricle, whether digitalis is given or not. (Dr. Gull, p. 75.)

POPLITEAL ANEURISM.—*Digital pressure.*—A very successful cure of a popliteal aneurism is recorded by Mr. Porter, of Dublin, in which, by digital pressure only, the aneurism became perfectly solid in twenty-seven hours. No matter how carefully applied, instrumental pressure cannot be borne so long as can the finger-tip; and further, the artery cannot be isolated so completely from its accompanying vein by mechanical as by digital compression, and therefore, œdema of the limb is more likely to ensue. There is less likelihood of ulceration or sloughing of the skin occurring than when instrumental pressure is used, for a less amount of force suffices to control the current of blood through the artery, and the point of pressure can with such ease be changed to a higher or lower position. The pressure must be effected by placing the points of the middle and index fingers of the left hand over the femoral artery where it crosses the brim of the pelvis, then by fixing and increasing the pressure by placing the middle and ring fingers of the right hand on those already over the vessel. The pulsation of the tumour must be arrested as much as possible, and the pressure kept up by fresh assistants relieving each other every two hours. (Dr. G. H. Porter, p. 135.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

LARYNGOSCOPY.—There is generally an unpleasant glare in the eyes of the operator, owing to his facing the source of light. This may be avoided by the simple device of a black shade over the eyes, the shade being placed beneath the reflector on the forehead, and fastened by two hooks and eyes to the band which passes round the head. (Dr. G. Johnson, p. 85.)

DISEASES OF THE LARYNX AND PHARYNX.—*Inhalation of Atomized Fluids.*—By means of the little tubes, now seen in every druggist's window, for blowing out a fine cloud of scent, &c., various medicated fluids may be atomized and applied directly to the mucous membrane of the throat and larynx in a more effectual way than by the brush. In inflammatory states, solution of nitrate of silver may be thus applied, and nitrate of aluminium, if a milder agent is required. Solutions of tannin, alum, sesquichloride of iron, corrosive sublimate, sulphate of zinc, and even common salt, are useful in various cases. In nervous affections of the larynx, preparations of opium, and salts of iodine or bromine may be used. (Dr. Beigel, p. 82.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

AZOTURIA.—A patient suffering from azoturia appears at first to have no tangible disease, but his urine is found habitually of a high specific gravity, and containing a large amount of urea. The general symptoms are debility, dyspepsia, and a kind of indefinable malaise. Dr. Prout considered that these cases generally pass into diabetes, or some other formidable disease. The best medicinal remedies are mineral acids, opiates, and tonics of various kinds. (Dr. E. H. Sieveking, p. 94.)

CHRONIC ULCER OF THE TONGUE.—In a case of ulcer of the tongue of twenty years' standing, the following treatment rapidly procured the healing of the sore. Its syphilitic origin was questionable. Four minims of Fowler's solution of arsenic were taken three times a day, and a mercurial bath twice a week, the ulcer being touched daily with nitrate of copper. After twenty-five days, Donovan's solution of arsenic, iodine, and mercury was given in doses of twenty minims twice a day. The ulcer rapidly healed. (Dr. A. Leared, p. 185.)

FLATULENCE.—*Charcoal.*—Charcoal, when introduced into the stomach in a dry and fresh state, has the property of absorbing gases. It may be given freshly prepared and hermetically sealed in gelatine capsules. Three or four capsules are generally sufficient to give complete relief in cases of flatulence. The best charcoal for this purpose is made from vegetable ivory, and must not be finely powdered. It has been shown by direct experiment that twenty grains, or two capsules of the vegetable ivory charcoal, absorb two-and-a-half cubic inches of carbonic acid gas. (Dr. A. Leared, p. 86.)

HEMORRHOIDS.—An ointment of persulphate of iron (℥ss.—℥j. ad oz.) in hemorrhoids is an excellent application when they are ulcerated, or when the constitution is weakened by diarrhœa or excessive fatigue. (Assistant-Surgeon Cartwright, p. 148.)

Internal Hemorrhoids—Clamp versus Ligature.—The danger of pyæmia from the use of the ligature for the removal of hemorrhoids has been very much overrated, and other means, such as the clamp, proposed instead. There is, however, no plan more effectual or safer than the use of the ligature. Sir Benj. Brodie only saw two deaths out of 300 operations. Mr. Syme has never met with a case which terminated fatally, or threatened to do so, when the ligature simply was employed. The operation by the clamp is undoubtedly an effectual method of removing the disease, but it is in many respects inferior to that by the ligature. It is unnecessary to ligature the whole tumour, for the vessels which supply it always descend from above, just beneath the mucous membrane; the lower three-fourths may therefore be divided from the base by scissors, and the remaining

fourth ligatured. When the ligatures have been tied, the bulk of the tumours may be cut away. (Mr. J. R. Lane, p. 141.)

INGUINAL HERNIA.—*Radical Cure of.*—The success of an operation for the radical cure of inguinal hernia does not depend so much on retention of the invaginated scrotum within the canal, as on the formation there of a quantity of exudation and cicatrix tissue. Success frequently results when the invagination has completely descended, provided the patient has not by any premature over-exertion forced down the protrusion whilst the process of cicatrisation is yet incomplete. (Dr. J. Fayrer, p. 137.)

POLYPOID GROWTHS IN THE RECTUM, WITH FISSURE.—Occasionally polypus of the rectum is associated with anal fissure, the latter being a consequence of the former. A free incision must be made through the fissure, the polypus drawn down by a vulsellum, and a ligature applied at its point of attachment. It must then be cut off just beyond the ligature. (Mr. J. R. Lane, p. 146.)

AFFECTIONS OF THE URINARY ORGANS.

LITHOTOMY.—That free division of the prostate which implies a division of the reflected portion of the pelvic fascia. This should always be left untouched, for stones of great magnitude may be removed without such free use of the knife. This is one of the principal secrets of success. (Prof. Fergusson, p. 152.)

Infiltration of urine is the bugbear of modern lithotomists. I have never seen a genuine case of it. Diffuse inflammation of cellular tissue, arising from some unhealthy state of the constitution, is often confounded with infiltration. (Prof. Fergusson, p. 152.)

LITHOTRITY *versus* LITHOTOMY.—Lithotomy should be preferred in patients below the age of 15, as being more successful than lithotrity. Could blades be brought against small stones, so as effectually to crush them to sand, this operation might after all be found to be one more generally applicable in children than it is usually deemed. In the female, any other operation than lithotrity should be an exception to the rule. In adult males, lithotrity should be considered the rule, and lithotomy the exception. It is in many cases desirable to dilate the urethra with bougies before crushing, and their use is often advisable to relieve irritability. (Prof. Fergusson, p. 148.)

AFFECTIONS OF BONES, JOINTS, ETC.

ACUPRESSURE.—An effectual way of closing an artery, and one quicker than all other methods, is that by entering the point of the needle on one side of the artery, and pushing it behind the vessel until it emerges on the opposite side. The needle must then be twisted over the artery, and fixed on the parts beyond. In this method the artery is, to a certain degree, both twisted and compressed. (Prof. W. Pirrie, p. 119.)

Acupressure is not only easy of application, but its adaptation to situations and circumstances is as great or even greater than that of the ligature. It may be employed in the instance of vessels as large as the posterior tibials. There can be no question as to its favouring primary union. (Dr. P. H. Watson, p. 127.)

Acupressure in Amputation at the Knee.—The popliteal artery may be effectually closed in cases of amputation at the knee by Simpson's third method of acupressure, that is, inserting the needle under the vessel and then putting a loop of wire across it, and turning it under the needle. (Prof. Macnamara, p. 133).

AMPUTATION *of the Upper Arm.*—Amputation near the middle of the arm is often done, when a more conservative process would suffice. The covering of the end of the humerus may be taken from the forearm, where there is abundant material for a flap. (Prof. Fergusson, p. 108.)

Amputation of the Thigh.—It is better in amputation of the thigh to depend on compression of the main artery by the finger of an assistant than to trust to a tourniquet. Before commencing the operation, the limb should be raised for a short time so as to unload the vessels. It is a good plan to combine the advantages of the circular and of the flap operations, transfixing the limb as in the flap operation, but dividing the tissues adherent to the bone as in the circular, while retraction is made by an assistant. (Prof. Fergusson, p. 110).

Amputation of the Thigh and Leg.—It is unwise to leave a thick muscular flap. This is the general result of transfixing and cutting outwards. I am by no means convinced that this is invariably the best method of performing the flap operation, and I feel assured that cutting from without inwards is in many instances followed by the best results. (Professor Fergusson, p. 107.)

Amputation at the Knee-Joint.—An admirable stump may be made in some instances, by reserving the condyles and cartilages untouched, and covering them with a flap of skin merely from the front of the leg. (Prof. Fergusson, p. 109.)

Amputation at the Knee-Joint by Single Flap.—The operation consists in reflecting a rounded or semi-oval flap of skin and fat from the front of the joint; dividing every thing else straight down to the bone, and sawing the bone slightly above the plane of the muscles, thus forming a flat-faced stump with a bonnet of integument over it. There is less danger in this than in amputation at the lower third of the thigh, and the stump resulting is broader and more fitted to bear the weight of the body. The cicatrix is quite out of the reach of pressure. (Mr. H. D. Carden, p. 103.)

Cancerous Tumours of Long Bones.—Tumours of the lower jaw are often successfully removed by vertical section, both ends of the bone being left. The disease seldom or never returns, if the section is made in the healthy part. This plan is, however, totally lost sight of when a tibia, fibula, or other long bone is affected with tumour, and nothing short of total ablation will satisfy. (Prof. Fergusson, p. 113.)

Capital Operations.—*Subcutaneous Injection of Morphia.*—It is a good plan in the majority of cases of amputations, ovariotomy, herniotomy, and other capital operations, to inject from a third to a half a grain of morphia subcutaneously immediately after the operation. (Mr. H. W. Freeman, p. 68.)

Fractures.—Put the patient under chloroform, and ascertain, by careful examination of the parts, whether an attempt should be made to save the limb, or amputation at once be performed. If amputation is not performed, and secondary symptoms come on and progress as they usually do to a great extent, there is little or no chance of recovery. After the supervention of serious constitutional symptoms, an operation is generally useless. (Dr. W. Lyon, p. 111.)

Fracture of the Skull.—Fracture of the skull from a blow by a stick is generally situated in the left parietal, when the blow is given from before, owing to the weapon being held in the right hand of the assailant; and in the right parietal when given from behind. The bone usually gives way where struck; fracture by *contre-coup* does not take place except from a fall. (Mr. B. W. Switzer, p. 119.)

Hemorrhage during Operations.—*Effect of Chloroform on the Amount of.*—There is more venous, but less arterial bleeding, when chloroform is used. The venous bleeding occurs as a general oozing when the tension of the muscles wears off. (Dr. Kidd, *Medical Mirror, August*, 1865, p. 458.)

Operations on the Jaw Bones.—In the removal of even the largest tumours, it is unnecessary that the incisions should be so extensive as to divide the lip, even when a portion of the bone is removed by vertical incision. When a ligature is used in this kind of operation, both ends of the vessel should be tied. The great secret of success is to entirely remove the diseased portions, clipping and gouging, when the operation is on the upper

jaw, until healthy surfaces are reached. If it is necessary to divide the lip, the incision should be in the mesial line. (Prof. Fergusson, p. 112.)

Tumours of the Lower Jaw.—Horizontal sections of the lower jaw must always be preferred to vertical ones if practicable, in order that the shape of the lower side of the face may be preserved. (Prof. Fergusson, p. 113.)

STRUMOUS SYNOVITIS.—Cases of strumous synovitis in the torpid phase of the disease are as a rule ordered to remain at complete rest, with the limb on a splint for months or even years. This, however, destroys the health of the patient, and the disease notwithstanding terminates in the destruction of the joint. As soon as all inflammation has completely ceased, the skin being white and cool, passive and then active movements along with friction must be employed in order to restore form and flexibility. (Mr. R. Barwell, p. 100.)

TEST OF INFLAMMATION IN JOINTS.—In every important joint there is some one spot peculiar to each, tenderness on pressure in which indicates the presence of latent inflammation. These are, for the knee, a small space over the inner trochanter and inside the patella; for the elbow, the posterior part of the junction between the radius and humerus; for the wrist, the union between the scaphoid and ulna; for the ankle, a spot just outside the extensor longus tendon. For the shoulder, this choice spot of tenderness is situated at the back and external aspect, where the posterior fibres of the deltoid cross the joint; at the hip, the test place is just behind the trochanter. (Mr. R. Barwell, p. 102.)

AFFECTIONS OF THE SKIN.

ACNE.—There is only one medicine really necessary, and that is arsenic. All the others only clear the way for it. Sometimes it is necessary to give a mild saline aperient for a few days before beginning with the arsenic. In some cases it is well to combine liquor potassæ with the arsenic. Another remedy of great value is the biniodide of mercury in doses of the sixth of a grain or so daily. The best local applications are calomel ointment and the ointment of the hypochloride of sulphur. (Mr. J. L. Milton, p. 174.)

ALOPECIA.—Only two remedies can be relied upon, viz., the internal use of arsenic, and the external one of cantharides. A regular course of arsenic should be given. The cantharides must be so applied that a piece of skin the size of a shilling is blistered every second or third day until the whole of the surface is operated upon. (Mr. J. L. Milton, p. 160.)

Mr. Wilson uses an ointment made by macerating six drachms of the powdered cantharides, with a gentle heat for twenty-four hours, in three ounces of pure lard, it is then strained through blotting paper. (p. 159.)

CARBUNCLE.—*Subcutaneous Section.*—The following plan of treating carbuncle has been pursued with great success for many years by Mr. French, surgeon to the St. James's Infirmary. Let a tenotomy knife be introduced through the healthy skin a little beyond the edge of the induration, the forefinger of the left hand serving as a guide upon the surface of the tumour. Let it be divided subcutaneously with a crucial incision, the skin being left uninjured. Immediate relief is obtained, and the rapidity of the recovery which follows is very striking. The knife may be entered several times, if the carbuncle is very large. (Mr. C. Heath, p. 184.)

ECZEMA.—Eczema is not a vesicular disease at all. Let the surgeon examine carefully for himself, and he will fail at any period of the disease to find true vesicles. (Mr. J. L. Milton, p. 162.)

Antiphlogistic treatment must always be avoided in eczema, as the disease never occurs without a certain feebleness of health. Every chronic case is benefited by cod-liver oil, if given long enough, and in doses not larger than the stomach can bear comfortably. On arsenic alone no reliance can be placed at any stage except the last, and not always then. It is chiefly useful when the skin is red and tender with a quick reproduction of unhealthy cuticle or scabs, great itching, and a strong tendency in the disorder to stand

still. The most useful remedy against the eczema of middle or advanced age is steel. One or two drachms of steel wine, given two or three times daily will remove most cases of eczema in children. The saccharine carbonate is also a good preparation. In adults the tincture of the muriate of iron is the proper remedy. It is essential, however, that no free acid be present in the preparation used. It must be given with occasional intermissions for two or three months at least. (Mr. J. L. Milton, p. 164).

Few, if any, cases of eczema will in the long run resist the influence of iron. Cod-liver oil must be given at the same time. If after a full trial of these the case is not cured, cod-liver oil and arsenic should be given, and these failing, the biniodide of mercury should be given. (Mr. J. L. Milton, p. 164.)

Ill-Smelling Feet.—Shoes which have induced the excessive and ill-smelling secretion must be left off, and great cleanliness observed. If this simple means fail, completely envelope the feet in linen upon which an ointment composed of diachylon and linseed oil has been spread. Then cover the foot with a stocking and light shoe; every twelve hours the application is to be renewed, the foot being at the same time well rubbed with a dry towel. In about ten days, friction by means of some simple powder, such as common flour, must be employed and the ordinary shoes worn. After some days, brownish-yellow portions of epidermis will separate, leaving a white, clean, healthy epidermis behind. For some time afterwards the flour should still be rubbed into the foot. At the end of two or three weeks, the foot-sweating either disappears for ever, or, at least, for one or more years. (Prof. Hebra, p. 186.)

Itching of Skin, in various Complaints.—Apply a lotion of bichloride of mercury, ex. g., hydr. bichlor. gr. i., aquæ destill. ℥ v.; alcohol 3ij. ft. lotio.; apply this three times a-day. The quantity may be increased or diminished according to the effect. (Dr. Martin, p. 159.)

Prurigo.—In obstinate cases and in the prurigo of old people, the following treatment gives good results. Let strychnia be given in doses of the sixtieth of a grain in solution every three or four hours, till a decided effect is produced upon the disease, or till nervous symptoms show themselves, when it may be left off; a vigorous course of arsenic should follow it. To relieve the intolerable itching, the skin should be frequently rubbed with a damp sponge dipped in fine oatmeal, after this the tincture of croton (made by steeping an ounce of bruised croton seeds for a week in four ounces of spirit) should be applied, and after this has been done a few times, a lotion of bichloride of mercury in almond emulsion, fifteen or twenty grains to a pint, will often complete the cure. The application of iodine or of glycerine is sometimes of use. For the affection of the pudendum, Bateman recommends a lotion made of two grains of bichloride of mercury, and one ounce of lime water. Injections of very hot water, juniper-tar ointment, blisters to the thighs, and small bleedings, are upon the whole the most reliable remedies. (Mr. J. L. Milton, p. 157.)

Psoriasis.—Arsenic in the treatment of psoriasis would be found much more serviceable if its administration were continued steadily for months. Next in efficacy to arsenic in the internal treatment of psoriasis, comes cod-liver oil. Its use is especially indicated when there are any indications of struma. Alkalies should be given when there is a gouty or rheumatic taint, or, when there is a tendency to acidity of the stomach, and to the deposit of lithates in the urine; no alkaline preparation is more suitable than liquor potassæ, taken in a large quantity of water, in doses of twenty drops thrice daily. In chronic cases, local treatment is required, the scales must be removed by means of warm baths and thorough rubbing with towels after each. Preparations of tar are the most generally useful. To the head and face, however, these are inapplicable, and a solution of soft-soap and water is preferable. (Dr. T. M'Call Anderson, p. 170.)

Solution of Venice Turpentine as a Dressing for Wounds.—Apply over the whole surface of the wound a compress consisting of several folds of linen, well soaked in the following dressing: Venice turpentine 2 lb

8 oz., troy; bicarbonate of soda, 6½ drachms; water, eighteen pints imperial. This is to digest in a bath for six days, at a temperature not higher than 75° C., and then filter. The compress should be moistened by means of a sponge with some of the solution every four or five hours. Cicatrization of even very large wounds under this treatment is wonderfully rapid, the secretion of pus being very slight, while there is an entire absence of fœtidity. (M. Kerner, p. 186.)

TINEA.—*Diagnosis of.*—Tinea may be recognised without the use of the microscope. First we may eliminate all diseases which really begin with pustules, and refer them to impetigo, which is not a contagious disease at all. Vesicular diseases do not occur. True eczema is a weeping of the surface followed by crusts. Thus there only remains the true tinea, dry, scurfy, reddened patches, accompanied by clipping, shedding, twisting, and loosening of the hair, and a secretion of scales, which in favus may even assume the form of crusts. (Mr. J. L. Milton, p. 160.)

SYPHILITIC AFFECTIONS.

SYPHILIZATION.—Immunity from variola is obtained by one single inoculation, whereas immunity from the syphilitic virus is only obtained by a long series of inoculations. But when the cure is effected it is permanent, whereas nothing can be more melancholy than the results of the mercurial treatment. The employment of syphilization as a prophylactic would be immoral; but as a curative agent, where the disease is already constitutional, this accusation cannot be made. The virus must be taken from an indurated chancre and inoculated on both sides of the chest; after three days, pustules are developed, and from these fresh inoculations must be made at a distance from the first. At the end of every three days, the process must be repeated, so long as the matter continues to afford a positive result. When this is no longer the case, fresh matter must be procured and inoculation proceeded with as at the first. When this no longer takes, a third must be procured and inoculated on the upper arms, and then on the thighs. This treatment is radically curative of syphilis, being followed only by 12 or 13 per cent. of relapses, and these are so mild as only to require two or three days' treatment. Children should be treated precisely the same as adults; but the results have not been so successful as they have among adults. (Prof. Boeck, p. 212.)

Sometimes it is difficult to procure an inoculable secretion from an indurated sore. By constantly applying some savine powder to the surface of the sore, an inoculable secretion is produced. (Mr. Henry Lee, *Lancet, July* 8, 1865, *p.* 40.)

AFFECTIONS OF THE EYE.

CATARACT.—There is no notable difference between the results of the upward and downward section, if equally well performed; but the downward operation is generally carried out more perfectly, in consequence of the greater rapidity and facility with which it may be executed. The operation for cataract should be performed in bed, in order that the patient may have the advantage of being in a position of the most complete muscular repose, and of being able to remain in the same position after the operation. The operation being on the right eye, the surgeon must sit behind the head of the b-d. Immediately after the operation, the compress bandage must be applied. The orbit should be equally padded with picked lint, in little discs, over which a simple ascending turn of a flannel bandage is moderately tightly drawn across the eye. In a case which is running a favourable course, it is unnecessary to open the lids until four or five days after the operation. (Dr. Von Graefe, p. 195.)

Removal o Cataract by the Scoop Method, or by Traction.—The present mode of extracting a cataract by the flap operation is attended by a considerable percentage of failures. A safer operation is the following, the principle of which is to make a wound of such a size and shape as would permit the transit of the cataract, but not without the use of an instrument to draw it through the opening. Having given chloroform and opened the lids fully with a wire speculum, fix the eye by means of a strong pair of forceps. Then, by means of a spear-headed knife, curved at an angle on the flat, make an incision through the substance of the cornea, about half a line from its junction with the sclerotic. This may be either at the upper or outer part of the cornea. Next remove a small portion of iris by means of forceps bent at an angle, and with fine Asselini points. The object of this is to facilitate the exit of the cataract, to prevent the possibility of prolapse, and to diminish the liability to iritis and closed pupil. The capsule must now be opened freely in the same way, and with the same instrument as in an ordinary flap extraction. A thin, flat, and concave scoop must now be passed to the posterior surface of the lens, and then withdrawn, bringing with it the lens, or such nucleus portion of it as is of sufficient density to escape entire. Every particle of opaque lenticular matter must be carefully removed by means of a suitable spoon. The after treatment consists in the application of cotton wool soaked in cold water to the surface of the eyelid, and in the use of atropine in order to keep the pupil well dilated. (Mr. G. Critchett, p. 197.)

Catarrhal Ophthalmia.—More frequently than otherwise local applications, such as nitrate of silver and sulphate of zinc, prolong the complaint and cause granulation of the lids. All depleting remedies must be avoided: the treatment should be entirely restricted to constitutional remedies of a tonic character, merely directing the eyes to be frequently fomented with warm water, as much for the purpose of cleanliness as for comfort. If there is much intolerance of light or lachrymation, Dover's powder should be given in small doses. In ordinary cases bark and ammonia are the most suitable remedies. (Mr. H. Hancock, p. 187.)

Fundus Oculi.—*To Examine without the Ophthalmoscope.*—Place a lamp on a table in a darkened room, place yourself between the lamp and the patient, letting the lamp shine into the eye over your right shoulder (previously dilating the pupil with atropine). With a lens of about two inches focus you may see the interior of the eye very well. (Dr. Roseburgh, p. 195)

Improved Eye Speculum.—The ordinary wire forceps sometimes allow the lids to overlap, and so interfere seriously with the progress of any operation. The parts of the instrument directly employed in separating the lids should be grooves of metal, troughs in fact. These should be curved, and the length of the ordinary eyelid. This speculum may be obtained from Messrs. Weiss, London. (Mr. J. Z. Lawrence, p. 194.)

Paralysis of the Ocular Muscles.—*Electricity.*—When electricity is applied for the cure of paralysis of the ocular muscles, a beneficial action is only produced when the excitation is relatively weak. All improvement ceases if the current is applied too strong, or for too long a time. Half a minute at a time is quite sufficient. The cure is dependent, generally speaking, upon reflex excitation through the fifth pair of nerves, and not upon direct excitation of the nerves supplying the muscles themselves. Hence, the proper measure for the strength of the current is always measured by the sensitiveness of the fifth pair. When improvement occurs it is generally instantaneous, so that if none takes place in a fortnight it is not worth continuing the trial longer. (Dr. Benedikt, p. 210.)

Phlyctenular Corneitis.—*Yellow Oxide of Mercury.*—Ointments containing red oxide of mercury are favourite remedies, especially in various forms of blepharitis. But they have one great disadvantage, they are liable to irritate too much, owing to the crystalline form of the red oxide which cannot readily be sufficiently pulverised. The amorphous or yellow oxide, prepared by precipitation, is indubitably preferable to the red. One drachm of the yellow oxide in each ounce of ointment is about the proper strength. The good effect of this ointment is most seen in the more chronic cases,

where congestion is only passive. It should be applied only once a day, with a small brush, which is dipped in the ointment and applied between the eyelids; these, by their closure, rub off the ointment from the brush. It should not be left long in the eye, but, when it has become well diffused over the eye, wipe off as much as possible with a piece of linen. (Dr. Pagenstecher, p. 189.)

VASCULAR CORNEA AND GRANULAR LIDS.—*Purulent Inoculation.*—The more vascular the cornea the greater the probability that no sloughing will follow the treatment by purulent inoculation, and *vice versa*. In several cases, it is a good plan to perform circumcision of the conjunctiva, and then to inoculate a few weeks afterwards. A broad belt of conjunctiva should be removed around each cornea, care being taken to clear away every trace of tissue down to the sclerotic. (Mr. J. Hutchinson, p. 194.)

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

ABORTION.—When the general symptoms, such as a shrunk condition of the papillæ of the breasts, a watery uterine discharge, and extreme lassitude, indicate the death of the early fœtus, it is better to give ergot at once; if this fail, the introduction of sponge-tents will be sure to produce expulsion. (Dr. C. Bell, p. 228.)

DEFORMED PELVIS.—*Induction of Premature Labour.*—An interesting case of induction of premature labour is related at p. 227. The os was gradually dilated by means of the india-rubber bags, and the attempt to extract by means of the forceps proving a failure, the child was turned. The pelvis however was so contracted that it was found impossible to extract the head: so as the funis had ceased to beat the occipital bone was perforated, and the head (occiput looking directly backwards) extracted by the craniotomy forceps. (Dr. G. Hewitt, p. 227.)

DYSMENORRHŒA.—In bad cases of dysmenorrhœa there is frequently retention of the fluid of the uterus, owing to mechanical obstruction at the junction of the cervix with the body of the uterus. Where the cervix is hard and dense, cutting operations are most indicated, but under other circumstances the use of tents is preferable. (Dr. Graily Hewitt, p. 250.)

A good instrument for incising the contracted os is like a scissors with the cutting edges directed outwards. When closed, the blades overlap each other, and thus the cutting edge of each is protected by the back of the other. The incision of course is made as the instrument is withdrawn. To prevent union, roll a thin piece of sheet lead round the points of an ordinary dressing forceps, and pass it into the wound, when by opening the blades of the forceps the lead is expanded and it is thus left in the wound, the forceps being withdrawn. A piece of cotton wool must be placed in the upper part of the vagina, so as to keep the lead in its place and to control bleeding. (Dr. Kidd, p. 247.)

ERGOT.—*Active Constituents.*—Two vegetable nitrogenised alkaloids may be isolated from ergot of rye. Half a grain of "ecbolina" produces the same effect as half a drachm of the ergot; it induces a specific action on the spinal column and muscular system, which leaves behind a great amount of debility. The other alkaloid "ergotina" is of no medicinal value. (Mr. Wenzell, p. 225.)

FACILITATION OF THE FIRST STAGE OF LABOUR.—Separation of the membranes for some distance round the os favors dilatation much more efficiently than any means hitherto employed, Relaxation of the os will very generally result from this procedure in cases of prolonged first stage from rigidity. This separation may generally be effected by the finger; if this is not practicable, however, Dr. Hamilton's uterine bolt may be used. (Dr. A. Inglis, p. 221.)

FLACCID UNCONTRACTED UTERUS.—*Electro-Magnetism.*—Apply one pole of an electro-magnetic machine inside the uterine cavity, whilst the other is

applied externally over the abdominal walls. By this plan contraction was produced in a case which had resisted all other means. (Mr. D. W. Parsons, p. 233.)

HYSTEROTOMY.—All the good that can be gained by hysterotomy in the cure of uterine flexions, and of obstructive dysmenorrhœa, and the sterility which is so often associated with these conditions, is certainly gained by an incision which only passes through the mucous membrane and the innermost layer of muscular fibres. It is dangerous to cut into the thicker middle layer of muscular fibres, containing as it does large vessels, especially veins, and *à fortiori* into the outer layer which is so intimately connected with the peritoneum. (Mr. T. S. Wells, p. 234.)

INCISION OF THE OS UTERI.—There are many cases of conical cervix uteri, the os externum being so minute as scarcely to admit the uterine sound. The os internum in these cases is usually of normal calibre. Division of the os externum relieves these cases, which are characterised by dysmenorrhœa, metrorrhagia, and sterility. (Dr. Barnes, p. 248.)

LACERATED PERINEUM.—The surface of the wound must be clean and regular in the operation for the cure of lacerated perineum, otherwise primary union of the two surfaces will not take place. It is well that the wire sutures should be inserted at least half an inch, or, better still, an inch from the margin of the wound, and brought out at its posterior border close to the vaginal mucous membrane; it should then be reintroduced at a corresponding point on the opposite side, and brought out at a spot similarly situated into the one at which it was introduced. When the recto-vaginal septum is introduced or sphincter ani has been lacerated, the posterior suture should be made to pass through the thickness of the recto-vaginal septum. (Mr. T. Bryant, p. 253.)

Complete Rupture of the Perineum.—When the plastic operation is performed for the cure of complete rupture of the perineum, very great care and attention are required to retain the parts in accurate contact, because the action of the sphincter muscle is continually tending towards the separation of the edges of the wound at the lower angle. For this reason it is very desirable to paralyse the action of the sphincter for the time, by an incision. It is, however, sufficient to divide it on one instead of both sides as is usually done. (Mr. J. R. Lane, p. 254.)

PAINFUL MENSTRUATION.—The severe operative measures which have been recently recommended in the cases of painful menstruation are in most cases unnecessary. The cavity of the cervix is scarcely ever contracted without severe disease, and any contraction, if such really exists, is generally at the sphincter of the os internum, and may be remedied by a division of the tissue to the depth of a line on either side. It must be remembered, however, that this sphincter muscle, even in the healthiest females, is rarely passed by a bougie without some little resistance. Menstruation may be painful from its dawn to its close without any mischief or impediment existing of any kind whatever. Gradual dilatation by means of sponge-tents is far more permanent in its results than division with cutting instruments. The tents, however, must be very short, so as not to reach the uterine cavity, and must never be introduced without the absence of any inflammatory state is first ascertained by means of the speculum. Metal dilators are injurious, they do not dilate but crush the tissues. (Dr. J. H. Bennet, p. 251.)

PLACENTA PRÆVIA.—When the os is undilated and rigid, separation of a zone of placenta by the finger will not only check hemorrhage, but produce relaxation. (Dr. Trask, p. 224.)

PROCIDENTIA UTERI.—Remove the mucous membrane from the base of the bladder in two broad bands, extending from a point a little behind the urethra to each side of the cervix. The scarified surfaces must then be connected just in front of the cervix uteri for the purpose of closing the pouch, and thus forming a firm, unyielding support. This is to be effected by means of silver sutures. In order to keep the prolapsed uterus in the proper mesial line, and so obviate the difficulty of keeping the proper line for scarification, bend the end of a flexible plated copper sound in a circle sufficiently large to

slip over the neck as a collar, and let an assistant with this firmly depress the prolapsed base. The mucous membrane may be removed by means of curved scissors. (Dr. T. A. Emmet, p. 257.)

PUERPERAL FEVER.—The principal object to be sought, in order to prevent that form of puerperal fever not dependent on contagion, by the medical attendant or the nurse, must be firm contraction of the uterus after labour. Pressure by the hand, and the passage of lumps of ice up to the uterus per vaginam, are the only means which can be relied upon for this purpose. The administration of ergot is, however, a useful addition, and may be given as a rule in almost every case of labour, after the birth of the child, with the object of ensuring the firm contraction of the uterus. It may be combined, in powder, with tincture of Indian hemp and borax, and some stimulant and aromatic. If noxious fluids are present in the uterus, at any period subsequent to the labour, and are threatening by their absorption to produce fever, the uterine cavity should be washed out with a weak solution of the sulphites or hyposulphites in tepid water. (Dr. Snow Beck, p. 235.)

Puerperal Convulsions.—Chloroform.—In an interesting case of severe puerperal convulsions attended with opisthotonos and symptoms resembling those produced by strychnine, the use of chloroform produced marked alleviations of the symptoms. The inhalation was kept up for three hours, and the patient ultimately recovered. (Dr. J. Cuthill, p. 241.)

REMOVAL OF THE PLACENTA.—When the child is born, the hand must follow the descending womb, and grasp it, so gently as not to cause pain, and yet with a moderate firmness. This pressure ought to be relaxed as little as possible during the separation of the child. The placenta will now usually be found partially extruded, and a short continuance of the pressure will effect complete expulsion. (Dr. Paterson, p. 231.)

TEDIOUS LABOUR.—*Ergot.*—When we desire to produce pains exactly like the natural labour pains, that is, not so severe as to be continuous, we ought not to employ the large doses of ergot usually given. Five grains given every quarter of an hour will produce all effects desired. (Dr. D. Dyce Brown, p. 225.)

VESICO-VAGINAL AND RECTO-VAGINAL FISTULÆ.—In the operation for the cure of vesico-vaginal fistula, the principal element of success is the absence of irregularity or raggedness of the wound's surface. If any such irregularity exists, primary adhesion of the edges cannot take place. To render the paring of the edges more certain and satisfactory, Mr. Bryant, of Guy's Hospital, has invented an instrument to transfix and steady the part, whilst at the same time it acts as a guide to the knife. The instrument consists of a handle, to which are adaptable six guides, three for transverse and three for vertical fistulæ. In each of these two sets there is one with three prongs, one with two, and one with only one prong. In the set for transverse fistulæ, these prongs are in the same line as the handle, whilst in that for vertical fistulæ, they are at a right angle. In both sets the prongs are set in a bar, in the same manner as the prongs of a rake. These prongs should be inserted at the edge of, but not through, the mucous membrane of the bladder, and passed *between* the tissues, and then brought out through the mucous membrane of the vagina. It is now easy with one sweep of the knife to remove the transfixed portion of tissue. The lower margin must then be treated in the same manner as the upper, and the raw surfaces be brought together by wire sutures. (Mr. T. Bryant, p. 256.)

CHOLERA.

PATHOLOGY AND TREATMENT.—The poison of cholera acts through the blood chiefly on the ganglionic nervous system, at first probably rousing it up to violent action, but followed very rapidly by its paralysis. Thus, at first, we have spasm of the organic muscles of circulation, respiration, voice, of the intestinal canal, and many of the voluntary muscles. The consequence

is, that the blood is sent into all the great central veins, and cannot get round to the left side of the heart, on account of the same spasm or contraction of the branches of the pulmonary artery. In short, the circulation is stopped or nearly so. Then follows the real collapse, when spasm relaxes, but it is the relaxation of death, unless the vital power has energy enough to begin again. On the first attack of real Asiatic cholera, therefore, do not stimulate—do not interfere with either vomiting or purging*—but use the utmost diligence in giving *chloroform*, both by inhalation and internally. Teach the nurse how to do it, as you cannot be everywhere, and cannot be both doctor and nurse. Relax the cramps and spasm by chloroform; and, if necessary, carefully stimulate by ammonia or champagne, but *not until the spasm is overcome*. The circulation will then be resumed, and not be followed by the dangerous reaction, which formerly killed so many patients. But avoid all stimulants if you can, and let the reaction come slowly, and if you feel compelled to stimulate let it be by spt. am. co., champagne, or other mild wine. Give copious draughts of cold water, and even encourage vomiting. Give copious saline effervescing draughts, if agreeable.

Do not give opium at all, let the purging alone. It is supposed that it does more good than harm by *eliminating* the poison, and some even increase the purging by giving castor oil. Encourage vomiting by large quantities of cold water, with a little mustard in it if necessary. The muscles of vomiting are so energetic that the water will often be ejected with violence, but keep repeating the cold water, which is always greedily swallowed. There seems to be no specific power in calomel or any other medicine. Apply external cold or warmth as may be most agreeable. The best way to apply warmth is to wrap the patient up in a blanket wrung out of hot water, and over this hot-wet blanket place three or four other blankets. The hot blanket can at any time be renewed, and if you like you can, during its renewal, wipe the body over with a towel or sponge wrung out of *cold* water. The skin will be powerfully stimulated by this treatment to draw the blood back from the central veins, as soon as the contraction of the pulmonary capillaries will allow the blood to get round to the left side of the heart. Another method is to place the patient in a hot hip-bath with his feet in a foot-bath, the hot water to have about a pound of flour of mustard in it, and to be kept hot by adding hot water as required till reaction sets in, and the cramps and spasm relax. We have said nothing about bleeding, because, although we think that the practice was correct as laid down by the several writers mentioned, yet we have in chloroform and other antispasmodics all the good effects of bleeding without the danger. And as to stimulants they are generally injurious: a combination of compound spirit of ammonia with chloric ether is one of the safest and best stimulants we possess, and champagne and other sparkling wines are now so cheap and good that we should recommend them before all others. (Dr. Braithwaite, p. 265.)

* The premonitory purging of cholera may be cautiously treated in the way we treat English cholera, but, if it prove to be the real Asiatic cholera, we think that all astringents ought to be at once given up.

INDEX TO VOL. LII.

PAGE

Abortion in the early months of pregnancy, Dr. Bell on 228
Acne, Mr. Milton on the treatment of 174
Acupressure, Prof. Pirrie on 119
" after amputation, Dr. Keith on cases of 121
" as an hæmostatic agent, Dr. Watson on 127
" for controlling hemorrhage, Prof. Macnamara on . . 133
Acute rheumatism, Dr. Davies on the blister treatment of . . . 42
Addison, Dr. W., on scarlet fever 29
Ague, Mr. Hunter on the hypodermic treatment of 74
Alkalies, Dr. M'Call Anderson on their use in psoriasis . . . 171
Alkaline sulphites in the treatment of zymotic diseases . . . 17
Allingham, Mr. W., on the treatment of internal hemorrhoids . . 140
Alopecia, Mr. Milton on the treatment of 159
Althaus, Dr. J., on galvanization and faradisation in paralysis . . 46
Amputation, Prof. Fergusson on 106
" Dr. Keith's cases of acupressure after . . . 121
" at the thigh, Prof. Fergusson on 110
" by single flap, Mr. Carden on 103
Anal fissure, Mr. Lane on polypus of the rectum with . . . 146
Anderson, Dr. T. M'Call, on the treatment of psoriasis and lepra . . 170
Aneurism of the popliteal, Dr. Porter's case of 135
Arsenic, Dr. M'Call Anderson on its use in psoriasis 170
" Mr. Milton on its use in acne 174
" Mr. Milton on its use in eczema 164
Arteries, Mr. Churchill on a new mode of tying 136
Ataxy, progressive locomotor, Dr. Radcliffe on 51
Atomized fluids, Dr. Beigel on the inhalation of 82
Azoturia, Dr. Sieveking on 94
Barnes, Dr. R., on puerperal fever 237
" on the treatment of dysmenorrhœa 248
Barwell, Mr. R., on certain errors in the treatment of joint-diseases . . 101
Beale, Dr. L. S., on contagion 22
Beck, Dr. T. S., on puerperal fever 235
Beigel, Dr., on the inhalation of atomized fluids 82
Bell, Dr. C., on abortion in the early months of pregnancy . . . 228
Bell, Dr. C. W., on the pathology of cholera 271
Benedikt, Dr. M., on the treatment of ocular paralysis by electricity . . 210
Bennet, Dr. J. H., on the surgical treatment of painful menstruation . . 251
Blackshaw, Mr. J., his case of puerperal tetanus 242
Blister treatment of acute rheumatism 42
Boeck, Prof., on syphilization 212
Bowman, Mr. W., on extraction of cataract by a traction instrument . . 204
Brady, Mr. P., his case of cholera treated by chloroform . . . 275
Braithwaite, Dr. W., on the pathology and treatment of cholera . . 265
Brown, Dr. D., on the use of ergot in tedious labour 225
Bryant, Mr. T., on the operation for lacerated perineum . . . 253
" on vesico-vaginal and recto-vaginal fistula . . . 256
Bullar, Dr., on the treatment of cholera by the hot mustard bath . . 278

PAGE
Calabar bean, Dr. Ogle on its effect in obstinate chorea . . . 62
Cancer, Mr. Hunter on the hypodermic treatment of 75
Carbuncle, Mr. French's successful treatment by subcutaneous section . 184
Carden, Mr. H. D., on amputation by single flap 103
Cataract, Dr. Graefe on the operation for 195
" Mr. Bowman on extraction of, by a traction-instrument . . 204
" Mr. Critchett on its removal by the scoop method . . . 197
" form of spoon employed by Mr. Critchett for . . . 199
Catarrhal ophthalmia, Mr. Hancock on the treatment of . . . 187
Cerebral diseases, Mr. Hunter on the hypodermic treatment of . . 71
Cervix uteri, Dr. Barnes on division of, in dysmenorrhœa . . . 249
" Dr. Kidd on division of, in dysmenorrhœa . . . 246
Chapman, Dr. J., his case of epilepsy with insanity . . . 57
Chloroform, Dr. Braithwaite on its use in the treatment of cholera . . 274
" Dr. Cuthill on its use in puerperal convulsions . . . 241
Cholera, Dr. Bell on its treatment by bleeding 273
" Dr. Bell on the pathology of 271
" Dr. Braithwaite on its pathology and treatment . . . 265
" Dr. Bullar's case treated by the hot mustard bath . . . 278
" Dr. Hill on the treatment of 274
" Dr. Johnson on the pathology of 265
" Dr. Massy on the symptoms and treatment of . . . 270
" Dr. Maxwell on its effects on the system 270
" Dr. Wallis on the cause of cholera and its effects on the system . 269
" Mr. Brady's case treated by chloroform 275
" Mr. Plimmer's case treated by chloroform 275
" Mr. Steadman's case treated by chloroform 276
" consecutive fever of 279
" its treatment by calomel 279
" its treatment by chloroform 274
" its treatment by cold water 279
Chorea, obstinate, Dr. Ogle on the effect of Calabar bean in . . . 62
Churchill, Mr. F., on a new mode of tying arteries, &c. . . . 136
Cod-liver oil, Dr. M'Call Anderson on its use in psoriasis . . . 171
" Mr. Milton on its efficacy in eczema 164
Conjunctivitis, Dr. Pagenstecher on the treatment of 189
Constable, Dr. J., his case of tape-worm successfully treated by oil of male fern 93
Contagion, Dr. Lionel Beale on 22
Convulsions, puerperal, Dr. Cuthill on the use of chloroform in . . 241
Cornea, vascular, Mr. Hutchinson's treatment by purulent inoculation . 194
Corneitis phlyctenulosa, Dr. Pagenstecher on the treatment of . . 189
Critchett, Mr. G., on removal of cataract by the scoop method . . 197
Croton oil in neuralgia and rheumatism, Dr. Sewell on . . . 59
Cuthill, Dr. J., on puerperal convulsions successfully treated by chloroform . 241
Davies, Dr., on the treatment of cholera by chloroform . . . 277
Davies, Dr. H., on the blister treatment of acute rheumatism . . 42
Deformed pelvis, induction of premature labour in a case of . . . 227
Deformity, Mr. Paget on a remarkable case of 233
Delirium tremens, Mr. Hunter on the hypodermic treatment of . . 72
De 'Ricci, Dr., on treatment of zymotic diseases by alkaline sulphites . . 17
Diabetes, Dr. Harley on the pathology of 97
Dick, Dr. H., his cases of pain of the spine 56
Digitalis, Dr. Gull on its action on enfeebled hearts 75
" Dr. Henry on its action as a cardiac tonic 80
" Dr. Murray on its action in heart diseases 80
Digital pressure, aneurism of the popliteal cured by 135
Disinfectant, Dr. Moffatt on phosphorus as a 28
Dobell, Dr. H., on the germs of rheumatic fever 40
Dyer, Dr. S. S., his case of rupture of the uterus; abdominal section . . 229
Dysmenorrhœa, Dr. Barnes on the treatment of 248
" Dr. Bennet's treatment of 251
" Dr. Greenhalgh's treatment of 249
" Dr. Hewitt's treatment of 250
" Dr. Kidd on 243
" Dr. Routh's treatment of 250
" Mr. Baker Brown's treatment of 249

PAGE
Dysmenorrhœa, Mr. Hunter on the hypodermic treatment of . . . 75
Dyspepsia, Dr. Leared on charcoal in 86
Eczema, Mr. Milton on external applications in 168
" Mr. Milton on the treatment of 164
" Mr. Startin's treatment of 167
" Mr. Wilson's treatment of 167
" not a vesicular disease 162
Electro-magnetism in post-partum hemorrhage, Mr. Parsons on . . 232
Emmet, Dr. T. A., on a radical operation for procidentia uteri . . 257
Epilepsy with insanity, Dr. Chapman's case of 57
Ergot, Dr. Brown on its use in inefficient action of the uterus . . 225
" Dr. Simpson on its use in inducing uterine action . . . 226
" Mr. Wenzell on the active constituents of 225
Eye, Mr. Lawrence on an improved speculum for the 194
Faradisation in certain forms of paralysis, Dr. Althaus on . . . 48
Fayrer, Dr. J., on the radical cure of inguinal hernia 137
Feet, ill-smelling, M. Hebra on the treatment of 186
Fergusson, Prof., on amputation 106
" on amputation at the thigh 110
" on lithotrity 148
" on lithotomy in the adult 151
" on operations on the jaws 112
Fever, Mr. Hunter on the hypodermic treatment of 74
" contagious, Dr. Beale on the nature of the poison of . . 23
" puerperal, Dr. Barnes on 237
" puerperal, Dr. Snow Beck on 235
" remittent, Dr. Purdon on the hypophosphites in . . . 20
" rheumatic, Dr. Dobell on the germs of 40
" scarlet, Dr. Addison on 29
" scarlet, Mr. Milton on the treatment of 34
First stage of labour, Dr. Inglis on the facilitation of the . . . 221
Fistula, vesico-vaginal and recto-vaginal, Mr. Bryant on . . . 256
Flatulence, Dr. Leared on a novel use of charcoal on 86
Fractures, Dr. Lyon on 111
Freeman, Mr. H. W., on the hypodermic injection of morphia . . 65
Fundus oculi, Dr. Roseburgh on a simple mode of examining . . 195
Galvanization in certain forms of paralysis, Dr. Althaus on . . . 50
Gastrotomy in a case of rupture of uterus; subsequent pregnancies . . 229
Gout, Dr. Bence Jones on the treatment of 35
Graefe, Dr. A. Von, on the operation for cataract 195
Granular lids and vascular corneæ treated by purulent inoculation . . 194
Gudden, M., his recent researches on scabies 178
Gull, Dr., on the action of digitalis on enfeebled hearts . . . 75
Hæmostatic agent, on the employment of acupressure as a . . . 127
Hammond, Dr. W. A., on the treatment of insomnia 64
Hancock, Mr. H., on catarrhal ophthalmia 187
Harley, Dr. G., on the pathology of diabetes 97
Heart diseases, Dr. Gull on the action of digitalis in 75
" Dr. Henry on the value of digitalis in . . . 80
" Dr. Murray on the action of digitalis in . . . 80
Heath, Mr. C., his successful treatment of carbuncle by subcutaneous section . 184
Hebra, M., on ill-smelling feet 186
Hemorrhage, Prof. Macnamara on acupressure for controlling . . 133
" Mr. Churchill's new application of the ligature for . . 136
" post-partum, Mr. Parsons on electro-magnetism in . . 232
Hemorrhoids, Mr. Allingham on the treatment of 140
" Mr. Cartwright on persulphate of iron in . . . 148
" Mr. Lane on the advantages of the ligature in . . . 141
" Mr. Smith on the clamp operation for 147
Henry, Dr. W. J., on digitalis as a cardiac tonic 80
Hernia, inguinal, Dr. Fayrer on the radical cure of 137
Hewitt, Dr. G., on the induction of premature labour 227
Hill, Dr. J., on the use of chloroform in cholera 274
Hunter, Mr. C., on the progress of the hypodermic treatment of disease . 69
Hutchinson, Mr. J., his treatment of vascular corneæ and granular lids . 194
Hypodermic injections, Prof. Nassbaum on the danger of . . . 75

PAGE
Hypodermic injection of morphia, Mr. Freeman on the . . . 65
" treatment of disease, Mr. Hunter on the progress of . . 69
Hypophosphites in neuralgia, Dr. Jackson on 62
" in remittent fever, &c., Dr. Purdon on 20
Hysterotomy, Mr. Spencer Wells on 234
Ice to the spine, case of epilepsy with insanity cured by . . . 57
Infantile strophulus, Dr. Martin's treatment of 159
Inglis, Dr. A., on facilitation of the first stage of labour . . . 221
Inguinal hernia, Dr. Fayrer on the radical cure of 137
Inhalation of atomised fluids, Dr. Beigel on the 82
Insanity, Dr. Newington on a new remedy in 63
Insomnia, Dr. Hammond on the treatment of 64
Itch, M. Gudden's recent researches on 178
" M. Hardy's rapid cure of 183
Jackson, Dr. T. H., on neuralgia and hypophosphites 62
Jaws, Prof. Fergusson on operations on the 112
Johnson, Dr. G., his suggestion in aid of laryngoscopy . . . 85
" on the pathology of cholera 265
Joint diseases, Mr. Barwell on certain errors in the treatment of . . 100
Jones, Dr. H. B., on the treatment of gout 35
Keith, Mr. T., his case of ovariotomy 263
Keith, Dr. W., on acupressure after amputation 121
Kerner, M., on solution of Venice turpentine as a dressing for wounds . 186
Kidd, Dr., on dysmenorrhœa 243
Labour, Dr. Inglis on the facilitation of the first stage of . . . 221
" natural, Dr. Paterson on the management of the placenta in . . 231
" premature, induction of, in a case of deformed pelvis . . . 227
" tedious, Dr. Brown on the use of ergot in 225
Lacerated perineum, Mr. Bryant on the operation for 253
" perineum, Mr. Lane on the operation for 254
Lane, Mr. J. R., on polypoid growths in the rectum 146
" on the operation for complete laceration of the perineum. . 254
" on the treatment of hemorrhoids 141
Laryngoscopy, Dr. Johnson's suggestion in aid of 85
Laurence, Mr. J. Z., on an improved eye-speculum 194
Leared, Dr. A., his case of ulcer of the tongue of twenty years' standing . 185
" on a novel use of charcoal in flatulence . . . 86
Lepra, Dr. M'Call Anderson on the treatment of 170
Ligature, Mr. Churchill's new mode of applying the 136
" Mr. Lane on its advantages in internal hemorrhoids . . . 141
Lithotomy in the adult, Prof. Fergusson on 151
Lithotrity, Prof. Fergusson on 148
Liver, Dr. Pavy on the assimilation of sugar by the 90
Lyon, Dr. W., on fractures 111
Macnamara, Prof., on acupressure for controlling hemorrhage . . 133
Male-fern oil, its efficacy in tapeworm 93
Martin, Dr., on the treatment of infantile strophulus, prurigo, &c. . . 159
Massy, Dr., on the symptoms and treatment of cholera . . . 270
Maxwell, Dr. W. G., on the effects of cholera on the system . . . 270
Melancholia, Mr. Hunter on the hypodermic treatment of . . . 72
Menstruation, painful, Dr. Henry Bennet on the treatment of . . . 251
Mercurial ointments in the treatment of psoriasis 173
Milton, Mr. J. L., on eczema and its treatment 162
" on the treatment of acne 174
" on the treatment of alopecia 159
" on the treatment of prurigo 157
" on the treatment of scarlet fever 34
" on the treatment of tinea 160
Moffatt, Dr. T., on ozone 28
Morphia, Mr. Freeman on the hypodermic injection of 65
Murray, Dr. W., on the action of digitalis in heart diseases . . . 80
Nassbaum, Prof., on the danger of subcutaneous injections . . . 75
Neuralgia and hypophosphites, Dr. Jackson on 62
Neuralgic affections, Dr. Sewell on croton oil in 59
Newington, Dr. S., on a new remedy in insanity and other diseases . . 63
Ocular paralysis, Dr. Benedikt on its treatment by electricity . . . 210

PAGE
Ogle, Dr. J. W., on the use of the Calabar bean in obstinate chorea . . 62
Ophthalmia, catarrhal, Mr. Hancock on the treatment of . . . 187
Ovariotomy, Mr. Keith's case of 263
Ozone, Dr. T. Moffatt on 28
Pagenstecher, Dr., on conjunctivitis and corneitis phlyctenulosa . . 189
Paget, Mr. J., on a remarkable case of deformity 233
Pain of the spine, Dr. Dick's cases of 56
Painful spasmodic affections, Mr. Hunter on the hypodermic treatment of . 73
Paralysis, Dr. Althaus on galvanization and faradisation in . . . 46
" of the ocular muscles, Dr. Benedikt's treatment of . . 210
" reflex, Dr. Robertson on 43
Parsons, Dr. D. W., on electro-magnetism in post-partum hemorrhage . 232
Paterson, Dr. J., on the management of the placenta in natural labour . 231
Pavy, Dr. F. W., on the assimilation of sugar by the liver . . . 90
Perineum, lacerated, Mr. Bryant on the treatment of . . . 253
" lacerated, Mr. Lane on the operation for 254
Peritonitis, Mr. Hunter on the hypodermic treatment of . . . 74
Persulphate of iron in hemorrhoids 148
Philipson, Dr. G. H., on puerperal tetanus 239
Phlegmasia dolens, Dr. Barnes on 237
Phosphorus as a disinfectant 28
Pirrie, Prof., on acupressure 119
Placenta, Dr. Paterson on the management of, in natural labour . . 231
" prævia, Dr. Trask on 224
Plimmer, Mr. G., his case of cholera treated by chloroform . . . 275
Polypus of the rectum, with anal fissure, Mr. Lane on . . . 146
Popliteal artery, digital pressure in aneurism of the . . . 135
Porter, Dr. G. H., his case of aneurism of the popliteal . . . 135
Pregnancy, Dr. Bell on abortion in the early months of . . . 228
Premature labour, Dr. Graily Hewitt on the induction of . . . 227
Procidentia uteri, Dr. Emmet on a radical operation for . . . 257
Progressive locomotor ataxy, Dr. Radcliffe on 51
Prolapsus uteri, Dr. Emmet on a radical operation for 257
Prurigo, Mr. Hardy's treatment of 159
" Mr. Milton on the treatment of 157
Psoriasis, Dr. M'Call Anderson on the treatment of 170
Purdon, Dr. H. S., on the hypophosphites in remittent fever, &c. . . 20
Puerperal convulsions successfully treated by chloroform, Dr. Cuthill on . 241
" fever, Dr. Barnes on 237
" fever, Dr. Snow Beck on 235
" tetanus, Dr. Philipson on 239
" tetanus following abortion, Dr. Blackshaw's case of . . 242
" thrombosis and embolia, Dr. Barnes on 238
Radcliffe, Dr. C. B., on progressive locomotor ataxy 51
Rectum, Mr. Lane on polypoid growths in the 146
Reflex paralysis, Dr. Robertson's cases of 43
Remittent fever, Dr. Purdon on the hypophosphites in . . . 20
Rheumatic affections, Dr. Sewell on croton oil in 59
" fever, Dr. Dobell on the germs of 40
Rheumatism, acute, Dr. Davies on the blister treatment of . . . 42
Robertson, Dr. A., his cases of reflex paralysis 43
Roseburgh, Dr. A. M., on a simple mode of examining the fundus oculi . 195
Rupture of the uterus, Dr. Dyer's case of abdominal section in . . 229
Rutherford, Dr. W., on the treatment of stricture 155
Scabies, M. Gudden's recent researches on 178
" M. Hebra's opinions on 181
Scarlet fever, Dr. Addison on 29
" Mr. Milton on the treatment of 34
Sewell, Dr. S. C., on croton oil in neuralgic and rheumatic affections . 59
Sieveking, Dr. E. H., on azoturia 94
Sleeplessness, Dr. Hammond on the treatment of 64
Speculum for the eye, Mr. Laurence on an improved 194
Spinal affections, Mr. Hunter on the hypodermic treatment of . . 73
Spine, Dr. Dick's cases of pain of the 56
Steadman, Mr., his case of cholera treated by chloroform . . . 276
Steel, Mr. Milton on its use in eczema 164

PAGE

Stricture of the urethra, Dr. Rutherford on Holt's treatment of . . 155
Subcutaneous injections, Prof. Nassbaum on the danger of . . . 75
Sugar, Dr. Pavy on its assimilation by the liver 90
Sulphites, alkaline, Dr. De 'Ricci on the treatment of zymotic diseases by . 17
Sweating feet, M. Martin on prevention of the smell of . . . 187
Switzer, Mr. B. W., on trephining for compression of the brain . . 119
Syphilis, Prof. Boeck on its treatment by syphilization . . . 212
Syphilization, Prof. Boeck's method of practising 214
Tapeworm, Dr. Constable's case treated successfully by oil of male-fern . 93
Tar, Dr. M'Call Anderson on its use in psoriasis 172
Tetanus, puerperal, Dr. Philipson on 239
" puerperal, Mr. Blackshaw's case of 242
Tinea, Mr. Milton on the treatment of 160
Tongue, ulcer of, Dr. Leared's treatment of a case of . . . 185
Trephining for compression of the brain, Mr. Switzer on . . . 119
Tumours of the jaws, Prof. Fergusson on the operations for . . . 112
Ulcer of the tongue of twenty years' standing, Dr. Leared's cure of . . 185
Urethra, stricture of, Mr. Rutherford on the treatment of . . . 155
Uterine action in labour, Dr. Brown on the use of ergot in inducing . . 225
Uterus, Dr. Emmet on a radical operation for prolapsus of . . . 257
" Dr. Dyer's case of rupture of the 229
Venice turpentine, solution of, as a dressing for wounds . . . 186
Vesico-vaginal fistula, Mr. Bryant on the treatment of . . . 266
Wallis, Dr. G., on the causes of cholera and its effects on the system . . 269
Watson, Dr. P. H., on acupressure as an hæmostatic agent . . . 127
Wells, Mr. T. S., on hysterotomy 234
Wenzell, Mr., on the active constituents of ergot 225
Wilks, Dr., on the action of digitalis on the heart 76
Wounds, M. Kerner on solution of Venice turpentine as a dressing for . 186
Zymotic diseases treated by alkaline sulphites, Dr. De 'Ricci on . . 17

GENERAL INDEX

TO

PARTS XLIX., L., LI., AND LII.,

OF

THE RETROSPECT OF PRACTICAL MEDICINE.

POINTING OUT

THE PARTICULAR PART AND PAGE IN WHICH INFORMATION ON ANY GIVEN SUBJECT MAY BE ACQUIRED.

	PART	PAGE
ABDOMINAL swellings, Dr. Kennedy on the diagnosis of	50	56
Abortion in the early months of pregnancy, Dr. Bell on	52	228
Acid and sugar in spirituous liquors, Dr. Bence Jones on	49	272
Acidity or alkilinity of the urine, Dr. Donovan on estimating the	49	69
" consequences and treatment of excess of	51	93
" on the means of detecting in the urine	51	91
" on its removal from the body	51	90
" on the acid diathesis, Dr. Bence Jones	51	88
" symptoms of excess of	51	92
Acid gravel, Dr. Bence Jones on	51	97
" means of detecting	51	98
" symptoms produced by	51	99
" treatment of	51	100
Acne, Mr. Milton on the treatment of	52	174
Acne Rosacea, Mr. Ker on the use of armoracea in	50	144
Aconite, Dr. Garrod on its external use in neuralgia	49	60
Acupressure, Professor Simpson on	49	84
" " on its application in ovariotomy	49	214
" its employment in cases of amputation of the thigh	49	91
" Mr. Dix on the substitution of wire for the needles in	49	90
" Prof. Pirrie on	52	119
" after amputation, Dr. Keith on cases of	52	121
" as an hæmostatic agent, Dr. Watson on	52	127
" for controlling hemorrhage, Prof. Macnamara on	52	133
Acupuncture, its use in the cure of hydrocele	51	175
Acute Rheumatism, Dr. Davies on its treatment by blisters	50	272
" " on the blister treatment of	52	42
Adams, Dr. A. L., his case of hydatid disease affecting the kidneys	50	77
" Mr., his case of tetanus treated by ice to the spine	50	39
" Mr. J., on incontinence of urine in boys	50	271
" Mr. W., on restoration of motion in cases of partial anchylosis	51	135
Addison, Dr. W., on scarlet fever	52	29
Adenoceles, or innocent tumours of the breast	50	261
Ague, Mr. Hunter on the hypodermic treatment of	52	74
Albumen in the urine, Dr. Clark on the best mode of detecting small quantities of	50	127
Albuminuria in children, Dr. Dickinson on the treatment of	50	268
Alkalies, Dr. M'Call Anderson on their use in psoriasis	52	171

	PART	PAGE
Alkaline sulphites in the treatment of zymotic diseases	52	17
Alkaline and earthy sulphites in the treatment of catylictic diseases	50	31
Alkaloids of opium, M. Claude Bernard on the	51	289
Allingham, Mr. W., on the treatment of internal hemorrhoids	52	140
" on communicability of secondary syphilis by intercourse	51	217
Alopecia areata, treatment of	49	129
" Dr. Watson's case treated by carbolic acid	50	139
Alopecia, or falling off of the hair, Dr. Frodsham on	50	138
" Mr. Milton on the treatment of	52	159
Althaus, Dr. J., on galvanization and faradisation in paralysis	52	46
Amaurosis, cerebral, Mr. Hutchinson's clinical data respecting	50	195
" intra-cranial, Mr. Hutchinson on	51	207
Amenorrhœa, Dr. Corlieu on the use of apiol in	50	258
Ammonæmia, Dr. Harley on	49	276
Amputation, Dr. Porter on a new method of arresting venous hemorrhage after	49	97
" by single flap, Dr. Carden on	49	285
" of thigh by Teale's operation, Mr. Porter on	49	81
" Prof. Fergusson on	52	106
" Dr. Keith's cases of acupressure after	52	121
" at the thigh, Prof. Fergusson on	52	110
" by single flap, Mr. Carden on	52	103
Anal fissure, Mr. Lane on polypus of the rectum with	52	146
Anchylosis, Mr. Adams on restoration of motion in cases of partial	51	135
Anderson, Dr. M'C., on the treatment of eczema	50	144
" Dr. T. M'Call, on obstruction of the eustachian tubes	49	173
" " on the treatment of psoriasis and lepra	52	170
Aneurismal clamps, Mr. Hart on the application of indices to	49	99
Aneurism of ascending aorta, Mr. Moore's case treated by insertion of wire	49	100
" Dr. O'Ferrall, on combined distal and proximal compression in cases of	51	139
" of the popliteal, Dr. Porter's case of	52	135
Aniline, sulphate of, its efficacy in hysterical chorea	51	50
Anthrax, Mr. Collins, on its treatment by pressure	49	134
Antiseptic, Dr. Watson on carbolic acid gas as an	49	260
Antilactescent, Dr. Morris on iodide of potassium as an	50	231
Antispasmodics in typhoid fever, Dr. Osborn on	50	35
Apiol, Dr. Corlieu on its use in amenorrhœa and dysmenorrhœa	50	258
Armoracia (root of horse-radish), its use in acne rosacea	50	143
Arsenic, Dr. M'Call Anderson on its use in psoriasis	52	170
" Mr. Milton on its use in acne	52	174
" Mr. Milton on its use in eczema	52	164
Arsenical preparations, on the treatment of neuralgia by	49	59
Arteries, Mr. Churchill on a new mode of tying	52	136
Arthritic posterior internal ophthalmia, or acute glaucoma	50	191
Articular cartilages, Mr. Holmes on exposure of, in wounds	51	125
Artificial pupil and extraction of cataract, Mr. Solomon's case of	51	205
Ascites and ovarian dropsy, Mr. Spencer Wells on the diagnosis between	50	253
Ashburner, Mr. J., his formula for preparing chlorodyne	50	287
Ashurst, Dr. J., on injuries of the head	50	100
Asthma, Dr. Salter on its treatment by iodide of potassium	49	62
" Dr. Hyde Salter on the value of chloroform in	51	69
Ataxy, progressive locomotor, Dr. Radcliffe on	52	51
Atomized fluids, Dr. Beigel on the inhalation of	52	82
Atropised and calabarised gelatine for dilating or contracting the iris	49	148
Atrophy, muscular, Dr. Radcliffe on	51	44
Auscultatory signs which precede crepitation in pneumonia	51	63
Azoturia, Dr. Sieveking on	52	94
Backache, Dr. Gervis on a variety of	49	241
Bader, Mr. C., on the treatment of granular conjunctivitis by inoculation	50	160
Banon, Dr. A. P., on the surgical treatment of vesico-vaginal fistula	51	268
Banks, Dr. A. J. H., on paralysis of the hand and forearm from smoking	50	45
Barnes, Dr. R., on a means of arresting flooding	51	238
" on the constitutional and local causes of puerperal fever	51	239
" on puerperal fever	52	237
" on the treatment of dysmenorrhœa	52	248

	PART	PAGE
Barton, Dr. J. K., on excision of the knee-joint	51	127
Barwell, Mr. R., on certain errors in the treatment of joint-diseases	52	101
Beale, Dr. L. S., on contagion	52	22
Beck, Dr. T. S., on puerperal fever	52	235
Behrend, Dr. H., on the action of bromide of potassium in inducing sleep	50	287
Beigel, Dr., on the inhalation of atomized fluids	52	82
Bell, Dr. C., on abortion in the early months of pregnancy	52	228
" Dr. C. W., on the pathology of cholera	52	271
Belladonna, Dr. Harty on its use in distension of the breasts	50	257
" Dr. Thorowgood on its use in epilepsy	50	40
" Mr. Hogg on its value in iritis	50	169
" its injurious action in glaucoma	50	189
" poisoning, Dr. Frazer on the treatment of	50	288
" in cases of severe burn, Mr. Hutchinson on	49	133
Benedikt, Dr. M., on the treatment of ocular paralysis by electricity	52	210
Bennet, Dr. J. H., on the surgical treatment of painful menstruation	52	251
" on the treatment of pneumonia by restoratives	51	64
Benzine, Dr. Mosler on its efficacy in trichiniasis	51	193
Bickersteth, Mr. E. R., on a new operation for union of an ununited fracture	49	77
Blackshaw, Mr. J., his case of puerperal tetanus	52	242
Bladder, Mr. Porter on the extraction of a hair-pin from the	49	232
" Mr. Smith's case of a portion of tobacco-pipe broken into the	49	125
" displacement of, as a cause of tedious labour	49	243
" Dr. Humphrey's case of bone, teeth, hair, &c., extracted from the	50	130
Blake, Dr. J., on influence of living in the open air in phthisis	50	53
Bleeding in injuries of the head, Dr. Ashurst on	50	100
Blister treatment of acute rheumatism	52	42
Blyth, Mr. E. J., on the chlorine treatment of scarlet fever	49	21
Boeck, Prof., on syphilization	52	212
Borax as a substitute of ergot, Mr. Moore on	50	227
Bowel, intussusception of, Dr. Greig on inflation as a remedy in	50	61
Bowman, Mr. W., on iridectomy	49	160
" on extraction of cataract by a traction instrument	52	204
Brady, Mr. P., his case of cholera treated by chloroform	52	275
Braithwaite, Dr. W., on the pathology and treatment of cholera	52	265
Breast, Dr. Hardy on the use of cere cloths in painful affections of the	50	257
" Mr. Bryant on the diagnosis of tumours of the	50	261
" " on excision of the	50	266
Bright's disease, Dr. Hassall on the treatment of	51	110
" Dr. Jenner on	51	107
Brisbane, Dr. J., on tapping through the vaginal wall for enlarged ovary	49	213
Broadbent, Dr. W. H., on displacement of the bladder as a cause of tedious labour	49	243
Broderick, Dr. H C., on a symptom of constitutional syphilis	49	179
Bromide of ammonium, Dr. Harley on its use in hooping-cough	49	66
" " its use in irritable uterus	49	231
" " Dr. Ritchie on its use in hooping-cough	50	54
" potassium, Dr. Garrod on	49	262
" " Dr. Behrend on its action in producing sleep	50	287
" " Dr. Rogers on its use in epilepsy	51	46
Bronchitis, chronic, Dr. Greenhow on tincture of larch in	49	65
" and fever, as early signs of phthisis	51	59
" rheumatic, Dr. Jones's treatment of	51	67
Browne, Mr. S., on iridectomy	49	169
Brown, Mr. J. B., on prolapsus of the uterus, bladder, and rectum	49	226
" on the treatment of fibrous tumours of the uterus	49	222
" " " of ovarian dropsy	49	208
" " " of retroflexion of the uterus	50	246
" on uterine hemorrhage from fibrous tumours	50	222
" his new method of securing the pedicle in ovariotomy	51	251
" his treatment of fibrous uterine tumours	51	247
" Dr. D., on the use of ergot in tedious labour	52	225
Brunton, Dr. J., his successful treatment of common tape-worm	51	83
Bryant, Mr. T., on the operation for lacerated perineum	52	253
" on vesico-vaginal and recto-vaginal fistula	52	256
" on the operation of ovariotomy	51	252

	PART	PAGE
Bryant, Mr. T., on the diagnosis of tumours of the breast	50	261
Buchanan, Dr. A. B., on malignant fibro-cellular tumour	49	137
Bullar, Dr., on the treatment of cholera by the hot mustard bath	52	278
Bullet, the travels of a	50	101
Bulley, Mr. F. A., on tumour of the tongue treated by galium aperinum	50	108
Burns, Mr. Hutchinson on internal administration of belladonna in severe	49	133
" Mr. Skey on the treatment of	49	132
Bursæ, enlarged, Mr. Skey on the treatment of	49	138
Cahen, Dr., on the arsenical treatment of neuralgia	49	60
Calabar bean, Mr. Nunneley on the action, preparation, and use of the	49	149
" paper, Mr. Hart on the effect of	49	152
" Dr. Ogle on its effect in obstinate chorea	52	62
Calculi, uric acid, Dr. Harley on the treatment of	50	70
" Dr. Roberts on their solution in the kidney and bladder	51	175
" passage of, from the kidney or bladder, treatment of	51	101
" phosphatic, Dr. Harley on	51	103
Calomel vapour bath, cases of syphilis treated by	50	201
Cancer, Mr. Hunter on the hypodermic treatment of	52	75
Cancerous tumours of the breast	50	261
" stricture of the rectum, Mr. Curling's case of	51	158
Carbolic acid, Dr. Watson on	49	260
" " on its use in alopecia areata	50	139
" " " in parasitic skin diseases	51	282
" " " for carious teeth	51	276
Carbuncle, Mr. French's successful treatment by subcutaneous section	52	184
Carbuncle and sloughing sores, use of carbolic acid in	49	260
" on its treatment by pressure	49	134
Carden, Mr. H. D., on amputation by single flap	52	103
Cardiac disease, Dr. Sutton on digitalis in	49	60
" Dr. Murray "	51	274
Caries of single bones of the tarsus and metatarsus, Mr. Holmes on excision in	51	132
Carious teeth, Dr. Watson on the use of carbolic acid for	51	276
Cartwright, Dr. G. S., on persulphate of iron in hemorrhoids	50	118
Cassells, Dr. J. P., on the sickness of pregnancy	50	229
Catarrhal inflammation of the auditory meatus and tympanum	49	178
Cataract, Mr. Critchett's vectis spoon for the extraction of	50	175
" Mr. Teale on the extraction of, by suction	50	171
" congenital, Mr. Critchett's treatment of, by iridesis	50	177
" Dr. Mooren's method of extraction of	51	198
" Dr. Taylor on the improved methods of extraction of	51	197
" Dr. Jacobson's method of extraction of	51	202
" Schufte's " "	51	204
" Mr. Solomon's case of extraction of, and formation of artificial pupil	51	205
" Dr. Graefe on the operation for	52	195
" Mr. Bowman on extraction of, by a traction-instrument	52	204
" Mr. Critchett on its removal by the scoop method	52	197
" form of spoon employed by Mr. Critchett for	52	199
Catarrhal ophthalmia, Mr. Hancock on the treatment of	52	187
Cauliflower excrescence of the uterus, Drs. Routh and Hicks on thee craseur in	51	249
Caustics, Dr. Morton on certain abuses of	49	256
Cazin, M., on the fabrifuge properties of the common white willow	50	36
Cerebral amaurosis, Mr. Hutchinson's clinical data respecting	50	195
Cerebral diseases, Mr. Hunter on the hypodermic treatment of	52	71
Cere cloths, use of, in painful affections of the female breasts	50	257
Cervix uteri, Dr. Priestley on an instrument for dilating	49	215
" Mr. Ellis on an instrument for dilating	49	217
" Dr. Gream on dilation versus division of, in dysmenorrhœa	51	265
" Dr. Barnes on division of, in dysmenorrhœa	52	249
" Dr. Kidd on division of, in dysmenorrhœa	52	246
Chancre and syphilitic lichen with intense pruritus, Dr. Sisson on	51	223
Chapman, Dr. J., his case of epilepsy with insanity	52	57
Charcot and Cornil, on the condition of the kidney in the gouty	51	85

	PART	PAGE
Child-crowing, Mr. J. Robertson on exposure to dry cold winds in	51	269
Children, Mr. Dickinson on the treatment of albuminaria in	50	208
" M. Guersant on polypus of the rectum in	50	207
Chlorodyne, Mr. Ashburner's formula for preparing	50	287
Chloroform, Dr. Clay on its use in ovariotomy	49	203
" Dr. Kidd on its use in puerperal convulsions	49	246
" and its safe administration, Dr. Kidd on	49	264
" on the administration of	50	287
" its local application in neuralgia and muscular rheumatism	50	44
" syrup of, Mr. Grove's formula for	50	288
" Dr. Parsons on its administration in ovariotomy	51	254
" Dr. Salter on its use in asthma	51	69
" Dr. Braithwaite on its use in the treatment of cholera	52	274
" Dr. Cuthill on its use in puerperal convulsions	52	241
Cholera, Dr. Bell on its treatment by bleeding	52	273
" Dr. Bell on the pathology of	52	271
" Dr. Braithwaite on its pathology and treatment	52	265
" Dr. Bullar's case treated by the hot mustard bath	52	278
" Dr. Hill on the treatment of	52	274
" Dr. Johnson on the pathology of	52	265
" Dr. Massy on the symptoms and treatment of	52	270
" Dr. Maxwell on its effects on the system	52	270
" Dr. Wallis on the cause of cholera and its effects on the system	52	269
" Mr. Brady's case treated by chloroform	52	275
" Mr. Plimmer's case treated by chloroform	52	275
" Mr. Steadman's case treated by chloroform	52	276
" consecutive fever of	52	279
" its treatment by calomel	52	279
" its treatment by chloroform	52	274
" its treatment by cold water	52	279
" and its prevention, Dr. Mapother on	50	26
Chorea, Dr. Trumble on the general treatment of	51	52
" hysterical, its successful treatment by sulphate of aniline	51	50
" obstinate, Dr. Ogle on the effect of Calabar bean in	52	62
Christie, Dr. J., on shoulder presentations of the fœtus	50	211
Christianson, Dr. R., on the prevalence of fevers in large towns	49	17
Chronic ulcers, Mr. Skey on the treatment of	49	139
Churchill, Mr. F., on a new mode of tying arteries, &c.	52	136
Ciliary blepharitis, Dr. Fano on the treatment of	49	149
" muscle, Dr. Hancock on the division of the	49	172
Clay, Dr. C., on ovariotomy	49	202
" " on wire loops as substitutes for pessaries, &c.	49	181
" " on ovariotomy and ovariotomists	51	250
" " on ovarian extirpations	50	249
Clark, Dr. A., on detecting small quantities of albumen in urine	50	127
Clarke, Mr. J., on the pathology of tetanus	50	37
Cleft palate, Prof. Fergusson on the operation for	50	104
" Mr. Collis on	51	152
" congenital, Dr. Kingsley on the treatment of	51	147
Codeia, Dr. Garrod on	49	253
Cod-liver oil, Dr. M'Call Anderson on its use in psoriasis	52	171
" Mr. Milton on its efficacy in eczema	52	164
Collis, Mr. M. H., on the treatment of anthrax by pressure	49	134
" on cleft palate	51	152
Colon, Dr. Kennedy on dilatation of the	50	56
Compress, wire, as a substitute for the ligature	50	90
Compression in certain cases of aneurism, Dr. O'Ferral on	51	139
Condylomata, Dr. Finco on mercurialised collodion as an application to	51	224
Conjunctivitis, granular, its treatment by inoculation with pus	50	160
" Dr. Pagenstecher on the treatment of	52	189
Constable, Dr. J., his case of tape-worm successfully treated by oil of male fern	52	93
Consumption, Dr. Cotton on the therapeutics of	50	49
" Dr. Mapother on the prevention of	50	27
" influence of living in the open air in	50	53
Contagion, Dr. Lionel Beale on	52	22

	PART	PAGE
Convulsions, puerperal. Dr. Cuthill on the use of chloroform in	52	241
" " Dr. Kidd on the use of chloroform in	49	246
Coote, Mr. H., on lithotomy and lithotrity	51	162
Corlieu, Dr., on the use of apiol in amenorrhœa and dysmenorrhœa	50	258
Cornea, Mr. F. Jordan on eczema of the	51	188
" Mr. Watson on the use of the seton in vascular ulcers of the	51	195
" vascular, Mr. Hutchinson's treatment by purulent inoculation	52	194
Corneitis phlyctenulosa, Dr. Pagenstecher on the treatment of	52	189
Cotton, Dr. R. P., on the therapeutics of consumption	50	49
Courty, Prof., on local injections in neuralgia, paralysis, &c.	50	44
Craig, Mr. W., on acupuncture in facial neuralgia	50	41
Crainiotomy, Dr. Inglis on the advantages of turning preliminary to	51	231
Creasote, its action in the treatment of sycosis	51	190
Crighton, Dr. R. W., on gastrotomy in rupture of the uterus	50	215
Critchett, Mr. G., on diseases of the lachrymal apparatus	49	139
" on iridectomy	49	153
" his case of congenital cataract treated by iridesis	50	177
" his vectis spoon for the extraction of cataract	50	175
" on removal of cataract by the scoop method	52	197
Croton oil in neuralgia and rheumatism, Dr. Sewell on	52	59
Cruise, Dr. F. R., on the endoscope in diagnosis and treatment of disease	51	178
Cummins, Dr. W. J., on the treatment of scarlatina by sulphites	51	38
Curling, Mr. T. B., his treatment of painful cancer of the rectum	51	157
Cuthill, Dr. J., on puerperal convulsions successfully treated by chloroform	52	241
Cysts, development of, in tumours of the breast	50	262
Davies, Mr. R., on the treatment of prolapsus uteri	49	229
" Dr., on the treatment of acute rheumatism	50	272
" " on the blister treatment of rheumatic fever	51	39
" " on the treatment of cholera by chloroform	52	277
" Dr. H., on the blister treatment of acute rheumatism	52	42
Death from chloroform, Dr. Kidd on the cause of	49	264
Deformed pelvis, induction of premature labor in a case of	52	227
Deformity, Mr. Paget on a remarkable case of	52	233
Delirium tremens, Mr. Hunter on the hypodermic treatment of	52	72
" " Dr. Seller on	49	38
" " Dr. Sloane on its treatment by digitalis	49	46
Decaisne, M., on the effect of tobacco on the heart	50	49
" Dr., his instantaneous cure of scabies	51	191
Delivery, Mr. Rigden's case of inversion of the uterus after	51	235
Deposites of urates, Dr. Harley on the pathology of	50	69
De Ricci, Dr., on sulphites and hyposulphites in zymotic diseases	49	22
" Dr. H. R., on the value of sulphites in catalytic diseases	50	31
" Dr., on treatment of zymotic diseases by alkaline sulphites	52	17
Diabetes, Dr. Harley on the pathology of	52	97
" Dr. Smith on the proper diet in	49	76
" Dr. Bence Jones on the treatment of	51	113
Dick, Dr. H., his cases of pain of the spine	52	56
Dickinson, Dr. W. H., on the treatment of albuminuria in children	50	268
Diet of lying-in women, Dr. Graily Hewitt on	50	288
" of children, Mr. Erasmus Wilson on the	51	289
" of children, Baron Liebig's soup	51	289
Digitalis, Dr. Robertson on its use in insanity	49	46
" Dr. Sloane on its use in delirium tremens	49	46
" Dr. Sutton on its employment in cardiac diseases	49	60
" Dr. Gull on its action on enfeebled hearts	52	75
" Dr. Henry on its action as a cardiac tonic	52	80
" Dr. Murray on its action in heart diseases	52	80
Digital pressure, aneurism of the popliteal cured by	52	135
Dilatation of the cervix uteri, Dr. Priestly on an instrument for	49	215
" of the colon, Dr. Kennedy on	50	56
" versus division of the cervix uteri in dysmenorrhœa, Dr. Gream on	51	265
Diptheria, Dr. Gibson on the treatment of	49	66
" Dr. Morton on the abuse of caustics in	49	257
" spurious, Dr. G. S. Smith on its nature and treatment	49	68
Disinfectant, Dr. Moffat on phosphorus as a	52	28
" Dr. Watson on carbolic acid as a	49	260

	PART	PAGE
Disinfectant, Mr. Druitt on iodine as a	49	262
Dislocation of shoulder, Mr. Salmon on reduction of, by slow manœuvres	49	80
Displacement of the bladder as a cause of tedious labour, Dr. Broadbent on	49	243
Dix, Mr. J., on the wire compress as a substitute for the ligature	50	90
Dobell, Dr. H., on the germs of rheumatic fever	52	49
Donkin, Dr. A. S., on the physiological action of the uterus in parturition	49	236
Donovan, Mr. M., on a simple method of estimating the acidity or alkalinity of urine	49	69
Dropsy, Dr. Johnson on the pathology, prognosis, and treatment of	49	29
" Dr. Smith on the use of erodium cicutarium in certain forms of	50	79
Druit, Mr., on a cheap disinfectant	49	262
" Dr. R., on the use of hot water for profuse perspiration	51	62
Duncan, Dr. J. M., on source of hemorrhage in fibrous tumours of the uterus	49	224
Duchesne-Duparc, Dr., on the treatment of prurigo	50	141
Dunn, Mr. R. W., on the non-mercurial treatment of syphilis	51	224
Dupuy de Frenelle, Dr., on chloroform in neuralgia and muscular rheumatism	50	44
Dyer, Dr. S. S., his case of rupture of the uterus; abdominal section	52	229
Dysmenorrhœa, Dr. Corlieu on the use of apiol in	50	258
" Dr. Gream on dilatation versus division of the cervix uteri in	51	265
" Dr. Marion Sims on excision of the os and cervix uteri in	51	261
" Dr. Barnes on the treatment of	52	248
" Dr. Bennet's treatment of	52	251
" Dr. Greenhalgh's treatment of	52	249
" Dr. Hewitt's treatment of	52	250
" Dr. Kidd on	52	243
" Dr. Routh's treatment of	52	250
" Mr. Baker Brown's treatment of	52	249
" Mr. Hunter on the hypodermic treatment of	52	75
Dyspepsia, Dr. Leared on charcoal in	52	86
Earle, Dr. J. L., on the treatment of prolapse of the funis	49	197
" his uterine sound with movable head	50	226
" on the scientific treatment of post-partum hemorrhage	50	219
Early pregnancy, Dr. Routh on some symptoms of	51	227
Echinococcus hominis, Dr. Leith Adams's case of	50	77
Ecraseur, Dr. Marion Sims on its use for removal of uterine polypi	51	256
" Drs. Routh and Hicks on the merits of the wire-rope	51	249
Eczema, Mr. Milton on external applications in	52	168
" Mr. Milton on the treatment of	52	164
" Mr. Startin's treatment of	52	167
" Dr. Anderson on the treatment of	50	144
" Mr. Meade on the treatment of	51	187
" Mr. E. Wilson on the nature, varieties, and treatment of	51	181
" of the eyelids, conjunctiva, and cornea, Mr. F. Jordan on	51	188
" Mr. Wilson's treatment of	52	167
" not a vesicular disease	52	162
Edmunds, Dr. J., his treatment of emprosthotonos by ice to the spine	49	56
Electro-magnetism in post-partum hemorrhage, Mr. Parsons on	52	232
Ellis, Mr. R., on an instrument for dilating the cervix uteri	49	217
Emprosthotonos, Dr. Edmunds' treatment by ice to the spine	49	56
Endoscope, Dr. Cruise's description and use of the	51	178
Enlarged ovary, Dr. Brisbane on tapping through the vaginal wall for	49	213
" tonsils, Dr. Mackenzie on their treatment without cutting	50	109
" tonsils, Dr. Smyly on the treatment of	51	156
Emmet, Dr. T. A., on a radical operation for procidentia uteri	52	257
Enteric fever, Dr. Christianson on	49	20
Epilepsy, Dr. McDonnell on bromide of potassium in certain forms of	49	49
" Dr. Thorowgood on the use of belladonna in	50	40
" Dr. Rogers on the use of bromide of potassium in	51	46
" with insanity, Dr. Chapman's case of	52	57
Epiphora, Mr. Critchett on	49	141
Epiphyses of long bones, Mr. Hutchinson on separation of the	50	102
Epitheliophyta, Bazin's treatment of	49	129
Ergot, Mr. Lumley Earle on the quality and mode of administration of	50	220
" Mr. Moore on borax as a substitute for	50	227
" Mr. Wenzell on the active constituents of	50	227

	PART	PAGE
Ergot, Dr. Brown on its use in inefficient action of the uterus	52	226
" Dr. Simpson on its use in inducing uterine action	52	226
" Mr. Wenzell on the active constituents of	52	225
Erodium cicutarium (stork's bill) as a remedy in certain forms of dropsy	50	79
Eustachian tubes, Dr. Anderson on obstruction of the	49	173
Excision of the knee-joint, Mr. Le Gros Clark on	49	79
" of the knee-joint, Professor Fergusson on	50	81
" of the knee-joint, Dr. Barton on	51	127
" of single bones of the tarsus and metatarsus for caries	51	132
" of the tongue, Prof. Syme on	51	140
Extraction of cataract, Mr. Chrichett's vectis spoon for	50	175
" of cataract, Mr. Teale's suction-curette for	50	173
" of cataract, Dr. Mooren on	51	197
Extra-peritoneal operation for strangulated hernia, Mr. Jordan on the	50	116
" operation for strangulated hernia, Mr. Jordan's simplification of the	50	112
Extreme divergent strabismus, Mr. Solomon on	51	208
Everett, Mr. B. G., on the use of alkaline baths in rheumatic fever	51	42
Eye, Mr. Vose Solomon on reflex disease of traumatic origin	50	196
" Mr. Lawrence on an improved speculum for the	52	194
Eyelashes. Flarer's operation for extirpation of the	51	193
Eyelids, Mr. F. Jordan on eczema of the	51	188
Facial neuralgia, Mr. Craig on acupuncture in	50	41
Fæcal vomiting in case of ileus, treated by galvanism	51	81
Fano, Dr., his treatment of ciliary blepharitis	49	149
Faradisation, Dr. Skinner on the galactogogue properties of	49	198
Faradisation in certain forms of paralysis, Dr. Althaus on	52	48
Fatty degeneration, Dr. Mapother on	50	28
" disease of the heart, Dr. Kennedy on	50	45
Favus, Dr. Watson on the efficacy of carbolic acid in	49	261
" Mr. Wilson on the treatment of	49	128
Fayrer, Dr. J., on the radical cure of inguinal hernia	52	137
Febrifuge properties of the common white willow, Mr. Cazin on the	50	36
Feet, ill-smelling, M. Hebra on the treatment of	52	186
Fenwick, Dr. S., on the condition of the stomach and intestines in scarlet fever	50	22
Fergusson, Prof. W., on excision of the knee-joint	50	81
" " on lithotomy in children, and on lithotrity	50	119
" " on the operation for cleft palate	50	104
" " on the operation for hare-lip	50	102
" " on amputation	52	106
" " on amputation at the thigh	52	110
" " on lithotrity	52	148
" " on lithotomy in the adult	52	151
" " on operations on the jaws	52	112
Fevers, Dr. Christianson on the prevalence of, in large towns	49	17
" enteric, Dr. Christianson on	49	20
" gastric, Dr. Christianson on	49	19
" relapsing, Dr. Christianson on	49	17
Fever, typhus, Dr. Christianson on	49	17
" typhus and typhoid, Dr. Gairdner on the treatment of	51	21
" typhus and typhoid, Dr. Wilkes on the treatment of	51	17
" typhus and typhoid, Editor of *Lancet* on the use of stimulants in	51	19
" typhoid, in Penicuik, Dr. Thin on	51	24
" scarlet, Mr. Blyth on the chlorine treatment of	49	21
" scarlet, Dr. Fenwick on conditions of the stomach and intestines in	50	22
" scarlet, Dr. Murchison on the etiology of	50	17
" scarlet, influence of age on liability to	50	21
" Dr. Gull on the treatment of	50	24
" Mr. Leach on the use of stimulants in	51	31
" intermittent, Dr. Ward on	50	28
" malarious, Mr. Hare on the treatment of	51	31
" rheumatic, Dr. Davies on the blister treatment of	51	39
" rheumatic, Dr. Everett on the use of alkaline baths in	51	42
" rheumatic, Mr. Robertson on the treatment of	50	30
" puerperal, Dr. Barnes on the constitutional and local causes of	51	239

	PART	PAGE
Fever, puerperal, Mr. Grisar on its propagation by accoucheurs	51	245
" Mr. Hunter on the hypodermic treatment of	52	74
" contagious, Dr. Beale on the nature of the poison of	52	23
" puerperal, Dr. Barnes on	52	237
" puerperal, Dr. Snow Beck on	52	235
" remittent, Dr. Purdon on the hypophosphites in	52	20
" rheumatic, Dr. Dobell on the germs of	52	40
" scarlet, Dr. Addison on	52	29
" scarlet, Mr. Milton on the treatment of	52	34
Fibro-cellular tumour, malignant, Dr. Buchanan on	49	137
Fibrous tumour of the uterus, Dr. Duncan on source of hemorrhage in	49	224
" tumour of the uterus, Dr. Sadler's case of	49	223
" tumour of the uterus, Dr. Rouse on the diagnosis of	50	232
" tumour of the uterus, Dr. Rouse on the treatment of	50	236
" tumour of the uterus, Mr. Brown's operation for	51	247
" tumour of the uterus, Mr. J. B. Brown on	49	222
Fisco, Dr., on application of mercurialised collodion to condylomata	51	224
First stage of labour, Dr. Inglis on the facilitation of the	52	221
Fistula, vesico-vaginal and recto-vaginal, Mr. Bryant on	52	256
Fistula lachrymalis, Mr. Critchett's treatment of	49	146
" vesico-uterine, Mr. Lane on a case of	49	217
" vesico-vaginal, Mr. Pollock on the operation for	49	221
" vesico-vaginal, Dr. Banon on the surgical treatment of	51	268
Flatulence, Dr. Leared on a novel use of charcoal on	52	86
Flooding, Dr. Barnes on perchloride of iron to arrest	51	238
Fœtal head, Dr. Hicks on the best mode of delivering after perforation	51	236
Forceps, Mr. Thompson on the use of the	49	183
Forcible extension in cases of partial anchylosis, Mr. Adams on	51	135
Fox, Dr. D. T., on syphilitic eruptions	50	206
" on the treatment of impetigo contagiosa	50	140
Fractures, Mr. Skey on the treatment of	49	78
" ununited, Mr. Bickersteith's new operation for union of	49	77
" Dr. Lyon on	52	111
" Mr. Tufnell on its immediate treatment by fixed apparatus	51	120
" principal points in its treatment by fixed apparatus	51	126
Fraser, Dr., his treatment of poisoning by belladonna	50	288
Freeman, Mr. H. W., on the hypodermic injection of morphia	52	65
Frodsham, Dr. J. M., on alopecia, or falling off of the hair	50	138
Fuller, Dr. H. W., on sciatica	49	266
Fundus oculi, Dr. Roseburgh on a simple mode of examining	52	195
Funis, prolapsion of, Dr. Earle on treatment of	49	197
Gairdner, Dr. W. T., his cases of uterine tumour simulating ovarine disease	50	240
" on the treatment of typhus and typhoid fevers	51	21
Galactagogue properties of faradisation	49	198
Galium aperinum, Mr. Bulley on its use in hard nodulated tumour of the tongue	50	108
Galvanization in certain forms of paralysis, Dr. Althaus on	52	50
Ganglions, Mr. Skey on the treatment of	49	138
Gangrene, hospital, Mr. Maunder on	50	153
Garrod, Dr. A. B., on external employment of aconitene in neuralgic pains	49	60
" on bromide of potassium	49	262
" on lettuce and its dried juice, lactucarium	49	255
" on the preparation of opium, and the salts of its alkaloids	49	253
Gascoyen, Mr. G. G., on the unity of the syphilitic virus	51	212
Gastrodynia, Dr. Leard on oxide of manganese in	51	80
Gastrotomy in a case of rupture of uterus; subsequent pregnancies	52	229
Gelatine atropine paper, Mr. Streatfield's	49	147
" atropised and calabarised, for dilating or contracting the iris, Mr. Hart on	49	148
Gervis, Dr. H., on a variety of chronic pains in the back	49	241
Gibson, Dr. W., on the treatment of diptheria	49	66
Glands, inflamed, Dr. Prieur on the use of iodine in	51	192
Glaucoma, Mr. Bowman on iridectomy in	49	160
" Mr. Browne on iridectomy in	49	169

	PART	PAGE
Glaucoma, Mr. Crichett on iridectomy in	49	153
" Mr. Hildige on iridectomy in a case of	49	171
" Mr. Hogg on iridectomy in	49	170
" Mr. H. Walton on iridectomy in	49	155
" Mr. Wharton Jones on iridectomy as a means of treating	50	191
" Mr. Wharton Jones on the pathology of	50	187
Glycerine as a vehicle for applications to the larynx, Dr. Johnson on	50	112
Goitres par enchymatous, Mr. Luton's use of local injections in	50	44
Gonorrhœal rheumatism, Dr. Popham on tincture of larch in	49	26
" orchitis, Mr. Smith's treatment of	50	136
Gonorrhœa, Dr. Henderson on two new specific remedies for	51	225
Gouty deposites, Dr. Harley on	50	72
Gout, Dr. Bence Jones on	51	86
" on the means of detecting	51	87
" chronic, Dr. Duncan on dilute hydrochloric acid in	51	272
" Dr. Bence Jones on the treatment of	52	35
Gouty, MM. Charcot and Cornil on the condition of the kidney in the	51	85
Graefe, Dr. A. Von, on the operation for cataract	52	195
Granular conjunctivitis, Mr. Bader on its treatment by inoculation	50	160
" ophthalmia, Mr. Lawson on its treatment by inoculation	50	165
" lids and vascular corneæ treated by purulent inoculation	52	194
Granville, Dr. J. M., on extreme cold as an anodyne in parturition	51	233
Grant, D. W. H., on saracenia purpurea in small-pox	49	36
Gravel, Dr. Bence Jones on	51	97
" and calculi, phosphatic, Dr. Harley on	51	103
Gream, Dr. G. T., on dilatation versus division of the cervix uteri in dysmenorrhœa	51	265
Greenhow, Dr. H., on tincture of larch in chronic bronchial affections	49	65
Greig, Dr. D., on inflation as a remedy in intussusception	50	61
Griffith, Dr. G. de G., on bromide of ammonium in irritable uterus	49	231
" " on cases of vascular tumour of the meatus urinarius	49	249
Grisar, M., on the propagation of puerperal fever by accoucheurs	51	246
Groves, Mr. T. B., his formula for syrup of chloroform	50	288
Gudden, M., his recent researches on scabies	52	178
Guérard, M., on gas fumes in pertussis	50	55
Guersant, M., on polypi of the rectum in children	50	267
Guide-hook, Mr. Newham's, a new obstetric instrument	51	234
Gull, Dr., his case of carcinoma about the gall-ducts, causing jaundice	50	68
" on the treatment of fever	50	24
" on the action of digitalis on enfeebled hearts	52	75
Gurgina balsam, or wood-oil, in gonorrhœa	51	225
Hæmostatic agent, on the employment of acupressure as a	52	127
Hair, Dr. Frodsham on the treatment of alopecia, or falling off of the	50	138
Hair-pin, extraction of, from the urinary bladder	49	232
Hamilton, Dr. F. H., on Nelaton's probe; with case	49	83
Hammond, Dr. W. A., on the treatment of insomnia	52	64
Hancock, Mr. H., on division of the ciliary muscle	49	172
" on catarrhal ophthalmia	52	187
Hardy, Dr., on the treatment of itch at the St. Louis Hospital	50	142
" on the use of cere cloths in painful affections of the breasts	50	257
Hare, Mr. E., on the treatment of malarious fever	51	31
Hare-lip operation, Mr. Smith's instrument for controlling hemorrhage in	51	151
" Prof. Fergusson on the operation for	50	102
Harley, Dr. G., on bromide of ammonium in hooping-cough	49	66
" on the urine	49	71
" on the urea	49	72
" on uræmia	49	275
" on the colouring matters of the urine	50	74
" on the pathology of deposites of urates	50	69
" on uric acid	50	70
" on phosphatic gravel and calculi	51	103
" on the pathology of diabetes	52	97
Hart, Mr. E., on atropised and calabarised gelatine for dilating and contracting the iris	49	148
" on the calabar bean paper	49	152
" on the application of indices to aneurismal clamps, &c.	49	99

	PART	PAGE
Hart, Mr. E., his case of extreme squint cured by the use of prisms	50	158
Hassall, Dr. A. H., on the action of perchloride of iron in renal affections	51	112
" on the treatment of Bright's disease	51	110
Head, Dr. Ashhurst on injuries of the	50	100
Headaches of nervous women, employment of essence of turpentine in	50	260
Heart, Dr. Kennedy on fatty diseases of the	50	45
" M. Decaisne on the effect of tobacco on the	50	49
Heart diseases, Dr. Gull on the action of digitalis in	52	75
" Dr. Henry on the value of digitalis in	52	80
" Dr. Murray on the action of digitalis in	52	80
Heath, Mr. C., his successful treatment of carbuncle by subcutaneous section	52	184
Hebra, M., on ill-smelling feet	52	184
Hemorrhage, Mr. Porter on a new mode of arresting, after amputation	49	97
" post-partum, Dr. Earle on the scientific treatment of	50	219
" uterine, from fibrous tumours or polypi, Mr. Brown on	50	223
" Dr. Smith's instrument for controlling, in hare-lip operations	51	151
" Prof. Macnamara on acupressure for controlling	52	133
" Mr. Churchill's new application of the ligature for	52	136
" post-partum, Mr. Parsons on electro-magnetism in	52	232
Hemorrhoids, Dr. Cartwright on efficacy of persulphate of iron in	50	118
" Mr. Allingham on the treatment of	52	140
" Mr. Cartwright on persulphate of iron in	52	143
" Mr. Lane on the advantages of the ligature in	52	141
" Mr. Smith on the clamp operation for	52	147
Henderson, Dr. T. B., on two new specific remedies for gonorrhœa	51	225
Henry, Dr. W. J., on digitalis as a cardiac tonic	52	80
Hepatic diseases in the East Indies, Sir Ranald Martin on	50	65
Hernia, Mr. Porter on the advantage of Syme's operation for the cure of	49	110
" Mr. Salt's new form of truss for	49	116
" Mr. Wood's operations for the cure of	49	114
" Mr. Wood's operations for radical cure of	49	272
" Mr. Wood's new truss-pads for	49	111
" Mr. Jordan on the extra-peritoneal operation for	50	116
" Mr. Jordan on a simplification of the extra-peritoneal operation in	50	113
" Mr. Kingdon on the causes of	50	118
" inguinal, Dr. Fayrer on the radical cure of	52	137
Hewitt, Dr. G., on the advantages of turning in certain cases of labour	50	209
" on the diet of child-bed	50	228
" his apparatus for performance of transfusion	51	237
" on the induction of premature labour	52	227
Hicks, Dr. J. B., on the best mode of delivering the fœtal head after perforation	51	236
" his wire-rope ecraseur	51	249
Hildige, Mr. J. G., on iridectomy in glaucoma	49	171
Hill, Mr. B., his two cases of syphilis, and communication by secondary contagion	50	159
" Dr. J., on the use of chloroform in cholera	52	274
Hinton, Mr., on a simple method of inflating the tympanum	49	179
" on chronic catarrh of auditory meatus and tympanum	49	178
Hogg, Mr. J., on the value of belladonna in iritis	50	169
" on iridectomy	49	170
Holmes, Mr. T., on the exposure of articular cartilages in wounds	51	129
" on treatment of caries of single bones of the tarsus and metatarsus, by excision	51	132
Holt, Mr. B., on the treatment of stricture of the urethra by splitting	50	132
" on the wire ligature and its retention in wounds	50	98
Hooping-cough, Dr. Harley on the efficacy of bromide of ammonium in	49	66
" Dr. Ritchie on bromide of ammonium in	50	54
" Mr. Guerard on gas fumes in	50	55
Hospital gangrene, Mr. Maunder on	50	153
Hulke, Mr. J. W., on the treatment of squint by operation	50	156
Humphrey, Dr. G. M., on lithotomy	49	117
" on stone in the female	50	128
Hunter, Mr. C., on the progress of the hypodermic treatment of disease	52	69
Hutchinson, Mr. J., on cerebral amaurosis connected with the use of tobacco	50	195
" on separation of the epiphyses of long bones	50	102
" his new tube for use after lithotomy	50	123

	PART	PAGE
Hutchinson, Mr. J., his report on cases of intra-cranial amaurosis	51	207
" " his treatment of vascular cornea and granular lids	52	194
" Mr., on internal administration of belladonna in severe burn	49	133
Hydatid disease affecting the kidneys, Dr. Adams's case of	50	77
Hydrocele, Dr. Jollie on the treatment of	51	176
" Dr. Thompson on acupuncture in	51	175
Hydrochloric acid, its use in consumption	50	50
Hydrochlorate of morphia, Dr. Garrod on	49	253
Hypodermic injections, Prof. Nassbaum on the danger of	52	75
" injection of morphia, Mr. Freeman on the	52	65
" treatment of disease, Mr. Hunter on the progress of	52	69
Hypophosphites in neuralgia, Dr. Jackson on	52	62
" in remittent fever, &c., Dr. Purdon on	52	20
Hysteria, Mr. Skey on	49	234
Hysterical chorea, its treatment by sulphate of aniline	51	50
" paralysis, Dr. Savage on its treatment by galvanism	51	48
Hysterotomy, Mr. Spencer Wells on	52	234
Ice to the spine, case of traumatic tetanus treated by	50	39
" case of epilepsy with insanity cured by	52	57
Ileus, with fæcal vomiting, Dr. Stokes's case treated by galvanism	51	81
Impetigo contagiosa, or porrigo, Dr. Fox on the treatment of	50	140
Incontinence of urine, in boys, Mr. Adams on	50	271
Infantine syphilis, Mr. Solomon's case of, from secondary contagion	50	205
Infantile strophulus, Dr. Martin's treatment of	52	159
Inflammation as an exciting cause of tuberculosis, Dr. Jones on	51	53
Inglis, Dr. A., on the advantages of turning in cases of disproportion	51	231
" on facilitation of the first stage of labour	52	221
Inguinal hernia, Dr. Fayrer on the radical cure of	52	137
Inhalation of atomized fluids, Dr. Beigel on the	52	82
Injections, vaginal, Dr. Rasch's instrument for	51	267
Injuries of the head, Dr. Ashhurst on	50	100
Inoculation, Mr. Lawson on the treatment of granular eyelids by	50	165
Intermittent fever, Dr. Ward on the treatment of	50	28
Intra-cranial amaurosis, Mr. Hutchinson on	51	207
Insanity, Dr. Robertson on the use of digitalis in	49	46
" Dr. Newington on a new remedy in	52	63
Insomnia, Dr. Hammond on the treatment of	52	64
Intussusception, Dr. Greig on inflation as a remedy in	50	61
Inversion of the uterus, Dr. Kelley on	49	188
" Mr. Sheppard on	49	191
Iodide of potassium, Mr. Salter on its use in asthma	49	62
" of iron, its use in consumption	50	51
" of potassium as an antilactescent, Dr. Morris on	50	231
Iodine as a disinfectant, Mr. Druitt on	49	262
" Dr. Prieur on its use in inflamed glands	51	192
Iridectomy, Mr. Bowman on	49	160
" Mr. Browne on	49	169
" Mr. Critchett on	49	153
" Mr. Hildige on	49	171
" Mr. Hogg on	49	170
" Mr. Walton on	49	155
" Mr. Wharton Jones on the operation of	50	184
" and section of the ciliary muscle, Journal of Brit. Ophthal. on	50	180
" as a means of treating glaucoma, Mr. W. Jones on	50	191
Iridesis, Mr. Critchett's case of congenital cataract, treated by	50	177
Iris, prolapsed, Mr. Hart's case of	49	152
" Mr. Lawson on the treatment of	51	196
Iritis, Mr. Hogg on the value of belladonna in	50	169
Irrigation in penetrating wounds of the knee joint	51	129
Irritable uterus, Dr. Griffith on the use of bromide of ammonium in	49	231
Itch, Dr. Gale's treatment of	50	143
" Dr. Harley on the treatment of	50	142
" on the treatment of	51	191
" M. Gudden's recent researches on	52	178
" M. Hardy's rapid cure of	52	183
Jackson, Dr. T. H., on neuralgia and hypophosphites	52	62

	PART	PAGE
Jaundice, Dr. Gull's case of carcinoma about the gall ducts causing	50	68
Jaws, Prof. Fergusson on operations on the	52	112
Jenner, Dr. W., on Bright's disease	51	107
Johnson, Dr. G., on the pathology, prognosis, and treatment of dropsy	49	29
" on glycerine as a vehicle for applications to the larynx	50	112
" his suggestion in aid of laryngoscopy	52	85
" on the pathology of cholera	52	265
Joints, Dr. Monro on severe wounds penetrating into	50	88
Joint diseases, Mr. Barwell on certain errors in the treatment of	52	100
Jollie, Dr. W., on the treatment of hydrocele	51	176
Jones, Dr. H. B., on acid and sugar in spirituous liquors	49	272
" " on acid gravel	51	97
" " on acidity, or the acid diathesis	51	88
" " on diabetes and its treatment	51	113
" " on gout	51	86
" " on the treatment of the oxalic acid diathesis	51	96
" " on the treatment of gout	52	35
" Dr. J., on inflammation as an exciting cause of tuberculosis	51	53
" " on the treatment of rheumatic bronchitis	51	67
" Mr T. W., on the operation of iridectomy	50	184
" " on the pathology of glaucoma	50	187
" " on iridectomy as a means of treating glaucoma	50	191
Jordan, Mr. F., on the extra-peritoneal operation for strangulated hernia	50	116
" " on a simplification of the extra-peritoneal operation in hernia	50	113
" " on eczema of the eyelids, conjunctiva, and cornea	51	188
Kamela and male-fern combined, efficacy of in tape-worm	51	83
Keith, Mr. T., his case of ovariotomy	52	263
" Dr. W., on acupressure after amputation	52	121
Kelly, Dr. A. L., his two cases of inversio uteri	49	188
Kendall, Mr. T. M., his treatment of scabies	51	192
Kennedy, Dr. H., on fatty disease of the heart	50	45
" " on the diagnosis of abdominal swellings	50	56
Ker, Mr. T. F., on the treatment of acne rosacea by armoracia	50	144
Kerner, M., on solution of Venice turpentine as a dressing for wounds	52	186
Keratitis, phlyctenular, Mr. Watson on the use of the seton in	51	195
Kidd, Dr. C., on chloroform and its safe administration	49	264
" " on the use of chloroform in puerperal convulsions	49	246
" Dr., on dysmenorrhœa	52	243
Kidney, Dr. Hassall on Bright's disease of the	51	110
" Dr. Jenner on Bright's disease of the	51	107
" on its condition in the gouty	51	85
Kingdon, Mr. J. A., on the causes of hernia	50	118
Kingsley, Dr. N. W., on the treatment of congenital cleft palate	50	147
Knee-joint, Mr. Le Gros Clark on resection of the	49	79
" Prof. Fergusson on excision of the	50	81
" Dr. Barton on excision of the	51	127
" Dr. Newman on irrigation in penetrating wounds of the	51	129
Labour, Dr. Monroe on a case of	49	196
" Dr. Palfrey's case in which uterine action progressed during sleep	49	195
" Dr. Grailly Hewitt on the advantages of turning in	50	209
" premature, Dr. Porter's case of induction by Dr. Barnes's method	50	208
" Dr. Barnes on arresting flooding after	51	238
" Dr. Granville on extreme cold as an anodyne to pain in	51	233
" Mr. Paterson on supporting the perineum during	51	230
" Mr. Rigden's case of inversion of the uterus after	51	235
" Dr. Inglis on the facilitation of the first stage of	52	221
" natural. Dr. Paterson on the management of the placenta in	52	231
" premature, induction of, in a case of deformed pelvis	52	227
" tedious, Dr. Brown on the use of ergot in	52	225
Lacerated perineum, Mr. Bryant on the operation for	52	253
" perineum, Mr. Lane on the operation for	52	254
Lachrymal apparatus, Mr. Critchett on diseases of the	49	139
" obstructions, Mr. Bowman's treatment of	49	142
Lactucarium, Dr. Garrod on	49	255
Lane, Mr. J. R., on vesico-uterine fistula	49	217
" " his observations on lithotomy	51	171

	PART	PAGE
Lane, Mr. J. R., on polypoid growths in the rectum	52	146
" on the operation for complete laceration of the perineum	52	254
" on the treatment of hemorrhoids	52	141
Laryngismus stridulus, Dr. Ramskill's treatment of	49	57
Larynx, glycerine as a vehicle for local applications to the	50	112
Laryngoscope, Dr. Sieveking on the use of the	51	74
Laryngoscopy, Dr. Johnson's suggestion in aid of	52	85
Laurence, Mr. J. Z., on an improved eye-speculum	52	194
Lawson, Mr. G., on inoculation and syndectomy	50	165
" on the treatment of prolapse of the iris	51	196
Laycock, Prof., his case of sycosis menti	51	188
Lead poisoning from spring water, Dr. Osborn on	50	287
Leach, Mr. H., on the use of stimulants in fever	51	31
Leared, Dr. A., on manganese as a remedy for pain caused by food	51	79
" his case of ulcer of the tongue of twenty years' standing	52	185
" on a novel use of charcoal in flatulence	52	86
Lee, Mr. H., on the modes of communicability of constitutional syphilis	51	218
Le Gros Clark, Mr. F., on resection of the knee-joint	49	79
Leishman, Dr. W., on rectification of the occipito-posterior position	49	191
Lente, Dr. F. D., on a new uterine porte-caustique	49	241
Lepra, Dr. McCall Anderson on the treatment of	52	170
Liebig's soup for children	51	289
Ligature, Mr. Dix on the wire compress as a substitute for the	50	90
" wire, Mr. Holt's cases in which it was left in the wound	50	98
" Mr. Churchill's new mode of applying the	52	136
" Mr. Lane on its advantages in internal hemorrhoids	52	141
Lithotomy, Mr. Humphrey's method of performing	49	117
" Mr. Thompson on the median operation of	49	124
" Mr. Hutchinson's tube for use after	50	126
" in children, Prof. Fergusson on	50	119
" in the female, Dr. Humphrey on	50	128
" Mr. Holmes Coote on the operation of	51	162
" Mr. Lane on the operation of	51	171
" advantages of the straight over the curved staff in	51	172
" in the adult, Prof. Fergusson on	52	151
Lithotrity, Prof. Fergusson on	52	148
" Prof. Fergusson on	50	119
" without injections, Mr. Pollock on	49	121
" without injections, Mr. Teale on	49	123
" without injections, Mr. Thompson on	49	119
" Mr. Holmes Coote on	51	163
" Mr. H. Thompson on its peculiar applicability in paralysed bladder	51	170
" Mr. H. Thompson on the eminently successful results of	51	164
" Mr. H. Thompson on the instruments employed in	51	168
" Mr. H. Thompson on the successful performance of	51	166
Liver, Dr. Pavy on the assimilation of sugar by the	52	90
" inflammation of, as it prevails in the East Indies	50	65
London Paste, as an escharitic, Dr. Mackenzie on	50	109
Long bones, Mr. Hutchinson on separation of the epiphyses of	50	101
Lowe, Dr. J., on the treatment of scabies	51	191
Lying-in women, Dr. Grailly Hewitt on the diet of	50	228
Lyon, Dr. W., on fractures	52	111
Mackenzie, Dr. M., on the treatment of enlarged tonsils without cutting	50	109
Macnamara, Prof., on acupressure for controlling hemorrhage	52	133
Malarious fever, Mr. Hare on the treatment of	51	31
Male-fern oil, its efficacy in tapeworm	52	93
Mammary tumours, Mr. Bryant on	50	261
Manganese, as a remedy for pain caused by food, Dr. Leared on	51	79
Mapother, Dr. E. D., on the prevention of zymotic and constitutional disease	50	25
Martin, Sir J. R., on hepatic diseases in the East Indies	50	65
" Dr. on the treatment of infantile strophulus, prurigo, &c.	52	159
Masse, Dr., on the action of creosote in sycosis	51	190
Massy, Dr., on the symptoms and treatment of cholera	52	270
Maunder, Mr. C. F., on hospital gangrene	50	163
Maxwell, Dr. W. G., on the effects of cholera on the system	52	270
Meade, Mr. R. H., on the treatment of eczema	51	187

	PART	PAGE
Meatus and tympanum, Mr. Hinton's treatment of catarrh of the	49	179
" urinarius, Dr. Griffin on cases of vascular tumour of the	49	249
McDonnell, Dr. R., on bromide of potassium in certain forms of epilepsy	49	49
Melancholia, Mr. Hunter on the hypodermic treatment of	52	72
Menstruation, painful, Dr. Henry Bennet on the treatment of	52	251
Mentagra, M. Bazin on the treatment of	49	129
Mercurial vapour, Mr. Parker on the treatment of syphilis by	50	204
" vapour, Mr. Pollard on the treatment of syphilis by	50	201
" ointments in the treatment of psoriasis	52	173
Mercurialized collodion as an application to condylomata	51	224
Mercury, Mr. Dunn's case of syphilis treated without	51	224
" in the treatment of syphilis	51	215
Milk, Dr. Skinner on the use of faradisation to promote the flow of	49	198
" Dr. Morris on iodide of potassium to arrest the secretion of	50	231
" fever, Dr. Graily Hewitt on	50	229
Milliken, Mr. J., on the travels of a bullet	50	101
Milton, Mr. J. L., on eczema and its treatment	52	162
" on the treatment of acne	52	174
" on the treatment of alopecia	52	159
" on the treatment of prurigo	52	157
" on the treatment of scarlet fever	52	34
" on the treatment of tinea	52	160
Moffatt, Dr. T., on ozone	52	28
Monro, Dr. W. S., on wounds into large joints	50	88
Moore, Mr. C. H., his case of aneurism treated by insertion of wire	49	100
" Mr. E. H., on borax as a substitute for ergot	50	227
Mooren, Dr. A., on the extraction of cataract	51	197
Morphia, hydrochlorate of, Dr. Garrod on the	49	253
" Mr. Freeman on the hypodermic injection of	52	65
Murray, Dr. W., on the action of digitalis in heart diseases	52	80
Morris, Dr. F. H., on iodide of potassium as an antilactescent	50	231
Morton, Dr. J., on certain abuses of caustics	49	256
Mosler, Dr., on the use of benzine in trichiniasis	51	193
Muller, Dr. W., on trichina spiralis	49	34
Munro, Dr. H., on a rare case of preternatural labour	49	196
Murchison, Dr. C., on the etiology of scarlet fever	50	17
" on the influence of age on the liability to scarlet fever	50	21
Murray, Dr. J. J., on the removal of large subcutaneous nævi by ligature	49	94
Muscular pain, formula of a local anæsthetic for relief of	50	45
" rheumatism treated by local application of chloroform	50	44
" atrophy, Dr. Radcliff on	51	44
Nævi, large subcutaneous, Mr. Murray on removal by ligatures without loss of skin	49	94
Nasal cavity, Dr. Skinner on a new mode of treating diseases of the	51	146
" cavity, Dr. Thudichum on a new mode of treating diseases of the	51	141
" duct, operation of catheterising the	49	143
Nassbaum, Prof., on the danger of subcutaneous injections	52	75
Nayler, Mr. G., on the treatment of pityriasis	49	131
Nealton's probe, Dr. Hamilton on, with case	49	83
Nervous headache, Prof. Teissier on internal use of essence of turpentine in	50	260
Neuralgia, Dr. Cahen on its treatment by arsenical preparations	49	59
" Dr. Garrod on the external employment of aconitine	49	60
" Prof. Courty on the use of local injections in	50	44
" Dr. Dupuy on the local application of chloroform in	50	44
" Mr. Thompson on	50	43
" facial, Mr. Craig on acupuncture in	50	41
" and hypophosphites, Dr. Jackson on	52	62
Neuralgic affections, Dr. Sewell on croton oil in	52	59
" affections, Dr. Braithwaite on the efficacy of gelseminum in	49	60
Newham, Mr. S., his "guide-hook," a new obstetric instrument	51	234
Newington, Dr. S., on a new remedy in insanity and other diseases	52	63
Newman, Dr. W., on penetrating wounds of the knee-joint—irrigation	51	129
Nicholls, Dr. J. F., on the treatment of scabies	51	191
Nicotine, Dr. Ogle's case of traumatic tetanus treated by	49	51
Nipples, sore, treatment of	51	246
Nitro-benzine and anoline, their different effects on the system	51	51

	PART	PAGE
Nose, on the medicinal solutions which may be applied to the cavity of the	51	144
Nunneley, Mr. T., on the action, preparation, and uses of the Calabar bean	49	149
Obscure disease, importance of urinary analysis in cases of	50	76
Obstetric guide-hook, Mr. Newham's	51	234
Obstruction of the eustachian tubes, treatment of	49	173
Occipito-posterior position, Dr. Leishman on rectification of	49	191
Ocular paralysis, Dr. Benedikt on its treatment by electricity	52	210
O'Ferral, Dr. J. M., on combination of distal with proximal compression in aneurism	51	139
Ogle, Dr. J. W., his case of traumatic tetanus treated by nicotine	49	51
" on the use of the Calabar bean in obstinate chorea	52	62
Oil of yellow sandal wood, its use in gonorrhœa	51	225
Ophthalmia, strumous, Dr. Price on the use of tartarized antimony in	50	165
" catarrhal, Mr. Hancock on the treatment of	52	187
Opium, Dr. Garrod on the preparations of	49	253
" M. Claude Bernard and Dr. Ozanane on the alkaloids of	51	289
Orchitis, acute, Mr. H. Smith on puncturing the testicle in	50	136
Osborn, Dr. H., on action of some spring water on lead	50	287
" on sulphuric ether and ammonia in typhoid fever	50	35
Ovarian dropsy and ascites, Mr. Spencer Wells on the diagnosis between	50	253
" tumours, Dr. Clay's cases of extirpation of	50	249
" disease, Dr. Gairdner's cases of uterine tumours simulating	50	240
Ovariotomy, Mr. Spencer Wells on practical details in	50	256
" Mr. J. B. Brown on the operation of	49	208
" Dr. Clay on the critical days in	49	204
" " on the large incision in	49	203
" " on the ligatures used in	49	203
" " on the operation in respect to age	49	205
" " on the results of his cases of	49	202
" " on the use of chloroform in	49	203
" " on the use of purgatives in the after treatment	49	204
" " results of his operations	51	250
" Mr. Keith's case of	52	263
" Prof. Simpson on the application of acupressure in	49	214
" Dr. Parson on the administration of chloroform in	51	254
" Dr. Scott's case of	51	257
" Mr. Brown's new method of securing the pedicle in	51	251
" Drs. Smith, Wells, and Hicks on securing the pedicle in	51	250
" Mr. Bryant on the operation of	51	252
Oxalic acid diathesis, Dr. Bence Jones on the treatment of	51	96
Ozæna, use of permanganate of potash for removing the fetor of	51	142
" chronic, Dr. Skinner's treatment of	51	146
Ozone, Dr. T. Moffatt on	52	28
Pagenstecher, Dr., on conjunctivitis and corneitis phlyctenulosa	52	189
Paget, Mr. J., on some cases of local paralysis	49	58
" " on a remarkable case of deformity	52	233
Pain in the back, chronic, Dr. Gervis on a variety of	49	241
" caused by food, Dr. Leared on its treatment by manganese	51	79
" of the spine, Dr. Dick's cases of	52	56
Painful spasmodic affections, Mr. Hunter on the hypodermic treatment of	52	73
Palfrey, Dr. J., his case of twin labour	49	195
Palsy, scriveners', or the paralysis of writers, Mr. Solly on	51	42
Pannus, Mr. Bader on its treatment by inoculation with pus	50	161
Paralysis, local, Mr. Paget on the treatment of cases of	49	58
" Prof. Courty on the use of local injections in	50	44
" of the hand and fore-arm from smoking, Dr. Banks' case of	50	45
" general hysterical, treated by the continuous galvanic current	51	48
" Dr. Althaus on galvanization and faradisation in	52	46
" of the ocular muscles, Dr. Benedikt's treatment of	52	210
" reflex, Dr. Robertson on	52	43
Paralysed bladder, Mr. H. Thompson on the applicability of lithotrity in	51	170
Paracentesis of the cornea, Mr. Bowman on	49	162
" " Mr. Walton on	49	156
Parasitic affections of the skin, Mr. Wilson on the treatment of	49	126
Parker, Mr. L., on the treatment of syphilis by the mecurial vapour bath	50	204
Parson, Dr., on the administration of chloroform in ovariotomy	51	254

	PART	PAGE
Parsons, D. D. W., on electro-magnetism in post-partum hemorrhage	52	232
Parturition, Dr. Donkin on the physiological action of the uterus in	49	236
" Dr. Granville on extreme cold as an anodyne in the pain of	51	233
Paterson, Mr. G. K. H., on supporting the perineum in labour	51	230
Paterson, Dr. J., on the management of the placenta in natural labour	52	231
Pavy, Dr. F. W., on the assimilation of sugar by the liver	52	90
Perchloride of iron as a means of arresting flooding	51	238
" " in renal and urinary affections	51	112
Perforation, Dr. Hicks on the best mode of delivering the fœtal head after	51	236
Perineum, ruptured, Mr. Brown on the operation for	49	227
" Mr. Paterson on supporting, during labour	51	230
" lacerated, Mr. Bryant on the treatment of	52	253
" lacerated, Mr. Lane on the operation for	52	254
Peritonitis, Mr. Hunter on the hypodermic treatment of	52	74
Persulphate of iron in hemorrhoids, Dr. Cartwright on	50	118
Petsulphate of iron in hemorrhoids	52	148
Pessaries, Dr. Clay on wire loops as substitutes for	49	181
Philipson, Dr. G.H., on puerperal tetanus	52	239
Phlegmasia dolens, Dr. Grailly Hewitt on	50	229
" " Dr. Barnes on	52	237
Phlyctenular keratitis, Mr. Watson on use of seton in	51	195
Phytodermatous furfuræ, treatment of	49	129
Phosphatic gravel and calculi, Dr. Harley on	51	103
Phosphorus, its use in consumption	50	50
" as a disinfectant	52	28
Phthisis, Dr. Blake on the influence of living in the open air in	50	53
" Dr. Cotton on the value of different therapeutic agents in	50	49
" Dr. Russell on fever and bronchitis as early signs of	51	59
Pirrie, Prof., on acupressure	52	119
Pityriasis, Mr. Naylor on the treatment of	49	131
Plasters, ready made, Dr. Tilt on	49	246
Placenta, Dr. Paterson on the management of, in natural labour	52	231
" prævia, Dr. Trask on	52	224
Plimmer, Mr. G., his case of cholera treated by chloroform	52	275
Pneumonia, Dr. Bennett on its treatment by restoratives	51	64
" Dr. Waters on auscultatory signs which precede crepitation in	51	63
Poisoning from belladonna, Dr. Fraser's treatment of	50	288
Politzer's eustachian catheter	49	177
Pollard, Mr. E. W., on the treatment of syphilis by mercurial vapour	50	201
Pollock, Mr. G., on lithotrity without injections	49	121
" on the operation for vesico-vaginal fistula	49	221
Polypus of the rectum, with anal fissure, Mr. Lane on	52	146
" in children, M. Guersant on	50	267
" uteri, Dr. Marion Sims on the ecraseur for removal of	51	259
Popham, Dr. J., on tincture of larch in gonorrhœal rheumatism	49	26
Popliteal artery, digital pressure in aneurism of the	52	135
Porrigo, Dr. Fox on the treatment of	50	140
Porte-castique, uterine, Dr. Lente's	49	241
Porter, Mr. W. E., his case of induction of labour by Dr. Barnes's dilating bags	50	208
" Dr. G. H., his case of aneurism of the popliteal	52	135
" Mr. G. H., on amputation of thigh by Teale's operation	49	81
" " on extraction of a hair-pin from the urinary bladder	49	232
" " on a new mode of arresting venous hemorrhage after amputation	49	97
" " on Syme's operation for reducible hernia	49	110
" " on the importance of excising a small portion of the windpipe, in tracheotomy	49	107
Potassium, bromide, chloride, and iodide of, physiological and therapeutical powers of	49	263
Pregnancy, Dr. Bell on abortion in the early months of	52	228
" Dr. Cassels, on the sickness of	51	229
" early, Dr. Routh on some of the symptoms of	51	227
Premature labour, Mr. Porter's case of induction by Dr. Barnes's method	50	208
" Dr. Grailly Hewitt on the induction of	52	227
Pre-taxoid operation for strangulated hernia, Mr. Jordan's	50	114

	PART	PAGE
Priestley, Dr. W. O., on an instrument for dilating the cervix uteri	49	215
Prieur, Dr., his treatment of inflamed glands	51	192
Price, Dr. W., on tartarised antimony as a remedy in strumous ophthalmia	50	165
Procidentia uteri, Dr. Emmet on a radical operation for	52	257
Profuse perspiration, Dr. Smith on hot water as a remedy for	51	62
Progressive locomotor ataxy, Dr. Radcliffe on	52	51
Prolapse of the funis, Dr. Earle on the treatment of	49	197
" of the iris, Mr. Hart's case of	49	152
" of the iris, Mr. Lawson's treatment of	51	196
Prolapsus of the bladder, with distention, in the first stage of labour	49	243
" of the uterus, bladder, and rectum, Mr. Brown on	49	226
" uteri, Mr. Davies on the treatment of	49	229
" uteri, Dr. Emmet on a radical operation for	52	257
Prurigo, Dr. Duchesne Dupare on the treatment of	50	141
" Mr. Hardy's treatment of	52	159
" Mr. Milton on the treatment of	52	157
Pruritus pudendi, M. Dieudonne's treatment of	51	288
Psoriasis, Dr. Vingtrinier's treatment of	50	142
" Dr. M'Call Anderson on the treatment of	52	170
Puerperal convulsions, Dr. Kidd on the use of chloroform in	49	246
" convulsions successfully treated by chloroform, Dr. Cuthill on	52	241
" diseases, Dr. Graily Hewitt on	50	229
" fever, Dr. Barnes on the constitutional and local causes of	51	239
" fever, M. Grisar on its propagation by accoucheurs	51	246
" fever, Dr. Barnes on	52	237
" fever, Dr. Snow Beck on	52	235
" tetanus, Dr. Philipson on	52	239
" tetanus following abortion, Dr. Blackshaw's case of	52	242
" thrombosis and embolia, Dr. Barnes on	52	238
Puncta and canaliculi, Mr. Critchett on the abnormal conditions of the	49	139
Pupil, artificial, Mr. Solomon's case of	51	205
Purdon, Dr. H. S., on the hypophosphites in remittent fever, &c.	52	20
Purulent infection, Prof. Polli on the sulphites and hyposulphites in	49	22
Quinine as an antidote in malarious fever	51	31
Radcliffe, Dr. C. B., on muscular atrophy or wasting palsy	51	44
" on progressive locomotor ataxy	52	51
Ranula, Mr. Skey on the treatment of	49	138
Rasch, Dr. A., his instrument for vaginal injections	51	267
Ready-made plasters, Dr. Tilt on the preparation of	49	246
Rectum, polypus of, in children, M. Guersant on	50	267
" Mr. Curling's case of painful cancer of the	51	158
" Mr. Lane on polypoid growths in the	52	146
Reflex diseases of the eye, of traumatic origin	50	196
" paralysis, Dr. Robertson's cases of	52	43
Remittent fever, Dr. Purdon on the hypophosphites in	52	20
Renal affections, Dr. Hassall on the action of perchloride of iron in	51	112
Renard, Dr., on the treatment of tendinous rheumatism	49	28
Resection of the knee-joint, Mr. Le Gros Clark on	49	79
Retroflexion of the uterus, Mr. Baker Brown's treatment of	50	246
Rheumatism, gonorrhœal, Dr. Popham's case of	49	26
" tendinous, Dr. Renard on the use of sulphur in	49	28
" acute, Dr. Davies on its treatment by blisters	50	272
" acute, Dr. Davies on the blister treatment of	52	42
" muscular, Dr. Dupuy on the local use of chloroform in	50	44
Rheumatic fever, Mr. Roberton on the treatment of	50	30
" fever, Dr. Davies on the blister treatment of	51	39
" fever, Mr. Everett on the use of alkaline baths in	51	42
" fever, Dr. Dobell on the germs of	52	40
" bronchitis, Dr. Jones's treatment of	51	67
" affections, Dr. Sewell on croton oil in	52	59
Rigden, Mr. G., on a rare case of midwifery practice	51	235
Ritchie, Dr. R. P., on the use of bromide of ammonium in hooping-cough	51	54
Roberton, Mr. J., on the treatment of rheumatic fever	50	30
" his treatment of child crowing	51	269
Robertson, Dr. C. L., on the use of digitalis in cases of insanity	49	46
" Dr. A., his cases of reflex paralysis	52	43

	PART	PAGE
Roberts, Dr. W., on the solution of calculi in the kidney and bladder	51	175
Rogers, Dr. G. G, on the use of bromides in epilepsy	51	46
Roseburgh, Dr. A. M., on a simple mode of examining the fundis oculi	52	195
Routh, Dr. C. H. F., on the diagnosis of fibrous tumours of the uterus	50	232
" on the treatment of fibrous tumours of the uterus	50	236
" on some of the symptoms of pregnancy	51	227
Rupture, Mr. Wood's operation for the cure of	49	114
" Mr. Wood on the radical cure of	49	272
" of the uretus, Dr. Crighton on gastrotomy in	50	215
" of the uretus, Dr. Dyer's case of abdominal section in	52	229
Ruptured perineum, Mr. J. B. Brown on	49	227
Russell, Dr. J., on fever and bronchitis as early signs of phthisis	51	59
Rutherford, Dr. W., on the treatment of stricture	52	155
Sadler, Dr. M. T., his case of removal of fibrous tumour of the uterus	49	233
Salmon, Mr., on reduction of dislocation of shoulder by slow manœuvres	49	80
Sall, Mr. T. P., on a new form of truss	49	116
Salter, Dr. H., on the treatment of asthma by iodide of potassium	49	62
" on the value of chloroform in asthma	51	69
Sarracenia purpurea, Dr. Grant on its efficacy in small-pox	49	38
" purpurea, Dr. Taylor on its efficacy in small-pox	49	37
" purpurea, Dr. Watson on	49	38
Savage, Dr., his case of general hysterical paralysis	51	48
Scabies, Dr. Decaisne's new and speedy method of treating	51	191
" Dr. Lowe on the treatment of	51	191
" Dr. Nicholls on the treatment of	51	191
" Mr. Kendall on the treatment of	51	192
" Mr. Strutt on the treatment of	51	192
" M. Gudden's recent researches on	52	178
" M. Hebra's opinions on	52	181
Scarlatina, Dr. Cummins's remarks on the use of sulphites in	51	38
Scarlet fever, Mr. Blyth on the chlorine treatment of	49	21
" Dr. Fenwick on condition of stomach and intestines in	50	22
" Dr. Murchison on the etiology of	50	17
" influence of age on the liability to	50	21
" Dr. Addison on	52	29
" Mr. Milton on the treatment of	52	34
Sciatica, Dr. Fuller on the treatment of	49	269
Scott, Dr. G., his case of ovariotomy	51	257
Scrivener's palsy, Mr. Solly on	51	42
Sea-tangle tents, Prof. Simpson on	50	247
Secondary contagion, Mr. Hill's case of syphilis communicated by	50	199
" Mr. Solomon's case of infantile syphilis communicated by	50	205
Section of ciliary muscle versus iridectomy	50	180
Seller, Dr. W., on delirium tremens	49	38
Separation of the epiphyses of long bones, Mr. Hutchinson on	50	102
Sesquichloride of iron, its efficacy in consumption	50	52
Sewell, Dr. S. C., on croton oil in neuralgic and rheumatic affections	52	59
Sheppard, Mr., on chronic inversion of the uterus	49	191
Shoulder, dislocation of, Mr. Salmon on reduction of	49	80
" presentations, Dr. Christie on	50	211
Sickness of pregnancy, Dr. Cassells on	51	229
Sieveking, Dr. E. H., on the use of the laryngoscope	51	74
" on azoturia	52	94
Simpson, Dr. J. Y., on acupressure	49	84
" " on the application of acupressure in ovariotomy	49	214
" Prof., on sea-tangle tents	50	247
" Dr. D. J., on syphilisation	51	215
Sims, Dr. J. M., on the removal of uterine polypi	51	259
" on the treatment of dysmenorrhœa	51	261
Sisson, Dr., his case of chancre and syphilitic lichen with pruritus	51	223
Skey, Mr. F. C., on hysteria	49	234
" on the treatment of burns	49	132
" on the treatment of chronic ulcers	49	139
" on the treatment of enlarged bursæ and ganglions	49	138
" on the treatment of varicose veins	49	98
" on the treatment of fractures	49	78

	PART	PAGE
Skey, Mr. F. C., on the treatment of ranula	49	138
Skin, Mr. Wilson on the parasitic affections of the	49	126
" disease, syphilitic, Dr. Squire on the diagnosis of	51	221
Skinner, Dr. T., on the galactagogue properties of faradisation	49	198
" on a new mode of treating diseases of the nasal cavity	51	146
Sleep, Dr. Behrend on the action of bromide of potassium in inducing	50	287
Sleeplessness, Dr. Hammond on the treatment of	52	64
Sloan, Dr. M. H., his case of delirium tremens treated by digitalis	49	46
Small-pox, Dr. Grant on saracenia purpurea in	49	36
" Dr. Taylor on saracenia purpurea in	49	37
" Dr. Watson on saracenia purpurea in	49	38
Smith, Dr. E., on the proper diet of diabetes	49	76
" Mr. G. S., on spurious diptheria, its nature and treatment	49	68
" Mr. H., his case of tobacco-pipe broken in the bladder	49	125
" Dr. W. A., on erodium cicutarium as a remedy in dropsy	50	79
" Mr. H., on puncturing the testicle in acute orchitis	50	136
" Dr. T., on ovariotomy	51	250
" Mr. H., on a case of death following the use of Holt's dilator	51	176
" Mr. T., his instrument for controlling hemorrhage in operation for hare-lip	51	151
Smoking, Dr. Banks's case of paralysis of hand and fore-arm from	50	45
" M. Decaisne on its effects on the heart	50	49
" as a predisposing cause of fatty heart, Dr. Kennedy on	50	47
Smyly, Dr. P. C., on the treatment of enlarged tonsils	51	153
Solly, Mr. S., on scrivener's palsy	51	42
Solomon, Mr. J. V., on infantile syphilis	50	205
" on reflex diseases of the eye of traumatic origin	50	196
" his case of extraction of cataract, and formation of artificial pupil	51	205
" on radical cure of extreme divergent strabismus	51	280
Solution of calculi in the kidney and bladder, Dr. Roberts on	51	175
Sore nipples, Dr. McCormac's treatment of	51	246
" Dr. Eastlake's treatment of	51	246
" Mr. Storer's treatment of	51	247
Sound, uterine, with movable head, Dr. Earle's	50	226
Speculum for the eye, Mr. Lawrence on an improved	52	194
Spine, Dr. Dick's cases of pain of the	52	56
Spinal affections, Mr. Hunter on the hypodermic treatment of	52	73
Spirituous liquors, Dr. Jones on the acid and sugar in	49	172
Squire, Dr. B., on syphilitic and non-syphilitic diseases of the skin	51	221
Staplyloraphy, Mr. Collis on	51	152
Steadman, Mr., his case of cholera treated by chloroform	52	270
Steel, Mr. Milton on its use in eczema	52	164
Sterility, Dr. Gream on dilatation versus division of the cervix uteri for	51	265
Stillicidiam, Mr. Bowman's treatment of	49	140
Stimulants in fever, Dr. Gairdner on the use of	51	21
" Dr. Wilks on the use of	51	17
" Mr. Leach on the use of	51	31
" Editor of *Lancet* on	51	19
Stokes, Dr., his case of ileus, with fæcal vomiting, treated by galvanism	51	81
Strabismus, Mr. Hart's case cured, withaut operation, by the use of prisms	50	158
" Mr. Halke on the operations for	50	156
" Mr. Critchett's operation for	50	157
" Mr. Graefe's operation for	50	157
" the Moorfields operations for	50	156
" extreme divergent, Mr. Solomon on the radical cure of	51	208
Stomach and intestines, condition of, in scarlatina	50	22
Stone in the female, Dr. Humphrey on	50	128
Streatfield, Mr. J. F., on a new mode of applying some external agents to the eye	49	147
Stricture, Mr. Smith on a case of death following the use of Holt's dilator	51	176
" of the rectum, cancerous, Mr. Curling on	51	157
" of the urethra, Mr. Holt's cases treated by splitting	50	132
" of the urethra, Dr. Rutherford on Holt's treatment of	52	156

	PART	PAGE
Strictures, Dr. Cruise on the use of the endoscope in	51	181
Strumous ophthalmia, Dr. Price on the use of tartarised antimony in	50	165
Strutt, Dr. G., his treatment of scabies	51	192
Subcutaneous injections, Prof. Nassbaum on the danger of	52	75
Substernal tenderness as a sign of acquired syphilitic taint	49	179
Suction-curette, Mr. Teale's, for the extraction of soft cataract	50	171
Sugar in spirituous liquors	49	272
Sugar, Dr. Pavy on its assimilation by the liver	52	90
Sulphites, Dr. Cummins on the treatment of scarlatina by	51	38
" Dr. De Ricci on their efficacy in catalytic diseases	50	31
" alkaline, Dr. De Ricci on the treatment of zymotic diseases by	52	17
Sulphuric ether and ammonia, efficacy of, as an anti-spasmodic in typhoid fever	50	35
Sweating feet, M. Martin on prevention of the smell of	52	187
Switzer, Mr. B. W., on trephining for compression of the brain	52	119
Sutton, Dr., on digitalis in cardiac diseases	49	60
Sycosis, Dr. Masse on the action of creasote in	51	190
" menti, vel mentagra, Dr. Lacock's treatment of	51	188
Syme, Prof., on excision of the tongue	51	140
Syndectomy and inoculation, Mr. Lawson on	50	165
Syphilis, constitutional, Dr. Brodrick on substernal tenderness as a sign of	49	179
" Editor of *Med. Times & Gazette* on therapeutics of	51	209
" Mr. Dunn's cases treated without mercury	51	224
" secondary, Mr. Allingham on communicability of	51	217
" secondary, Mr. H. Lee on the modes of communication of	51	218
" Mr. Hill on prolonged incubation period of, and secondary contagion	50	199
" Mr. L. Parker on its treatment by the mercurial vapour bath	50	204
" Mr. Pollard on its treatment by mercurial vapour	50	201
" and its treatment, Dr. Veale on	50	197
" infantile, Mr. V. Solomon's case from secondary contagion	50	205
" Prof. Boeck on its treatment by syphilisation	52	212
Syphilisation, Prof. Boeck's method of practising	52	214
Syphilitic and non-syphilitic skin diseases, Dr. Squire on the diagnosis of	51	221
" lichen and chancre, attended with intense pruritus	51	223
" virus, Mr. Gascoyen on the unity of the	51	212
" eruptions, Dr. Tilbury Fox on	50	206
Syphon-trocar, and hooked canular for ovariotomy, Mr. Wells's	49	212
Tape-worm, Dr. Rendle on the use of oil of male-fern in	49	284
" Dr. Burnton's treatment by kamela and male-fern combined	51	83
" Dr. Constable's case treated successfully by oil of male-fern	52	93
Tapping of the cornea in glaucoma	49	157
Tar, Dr. M'Call Anderson on its use in psoriasis	52	172
Tartarised antimony as a remedy in strumous ophthalmia	50	165
Taylor, Dr. J., on the efficacy of sarracenia purpurea in small-pox	49	37
Taylor, Dr. C., on recently improved methods of extracting cataract	51	197
Teale, Mr. T. P., on lithotrity without injections	49	123
" Mr. T. P., Jr., on extraction of soft cataract by suction	50	171
Teissier, Prof., on the internal use of turpentine in nervous headaches	50	260
Tendinous rheumatism, Dr. Renard on the use of sulphur in	49	28
Tents, Prof. Simpson on the dried stems of sea-tangle for	50	247
Tetanus from burn, Mr. Tyrell's case treated by the application of tobacco	49	54
" traumatic, Mr. Holmes and Dr. Ogle's case treated by nicotine	49	51
" Mr. Adam's case treated by ice to the spine	50	39
" Mr. Clarke on the pathology of	50	37
" Mr. Tyrrell on the local application of tobacco in	50	37
" puerperal, Dr. Philipson on	52	239
" puerperal, Mr. Blackshaw's case of	52	242
Thigh, amputation of, by Teale's operation, Mr. Porter on	49	81
Thin, Dr. G., on typhoid fever in Penicuik	51	24
Thompson, Mr. H., on lithotrity without injections	49	119
" on the median operation of lithotomy.	49	124

	PART	PAGE
Thompson, Mr. H., on the applicability of lithotrity in so-called paralysed bladder	51	170
" " on the instruments employed in lithotrity	51	168
" " on the modus operandi in successful lithotrity	51	168
" " on the successful results of lithotrity	51	164
" Mr. J., on the use of the short forceps	49	183
" Dr. J., on acupuncture in hydrocele	51	175
Thorowgood, Dr. J. C., on belladonna in epilepsy	50	40
Thrombosis, spontaneous, Mr. Tuckwell's case	49	104
Thudichum, Dr. J. L. W., on a new mode of treating diseases of the nasal cavity	51	141
Tincture of gelseminum (American), its efficacy in neuralgic affections	49	60
" of larch, its use in chronic bronchial affections	49	65
Tinea, Bazin's treatment of	49	127
" Mr. Milton on the treatment of	52	160
Tobacco, Mr. Tyrell on its application in a case of tetanus from burn	49	54
" M. Decaisne on its effects on the heart	50	49
" Mr. Hutchinson on cerebral amaurosis connected with the use of	50	195
" its local application in the treatment of tetanus	50	37
Tongue, tumour of, treated by the galium aperinum	50	108
" Prof. Syme on excision of the	51	140
" ulcer of, Dr. Leared's treatment of a case of	52	185
Tonsils, enlarged, Dr. Mackenzie on the treatment of	50	109
" " Dr. Smyly's treatment of	51	156
Toothache, Dr. Watson on carbolic acid in	51	276
Tracheotomy, Mr. Porter on importance of excising a small portion of windpipe	49	107
Transfusion, Dr. Hewitt on the operation of	51	237
Trephining in injuries of the head, Dr. Ashhurst on	50	100
" for compression of the brain, Mr. Switzer on	52	119
Trichina, spiralis, Dr. Muller on	49	34
Trichiasis, tonsurans, Bazin on the treatment of	49	128
" Dr. Wright on the operations for	51	193
Trichiniasis, Dr. Mosler on the efficacy of benzine in	51	193
Trousseau, M., on neuralgia	50	43
Truss, Mr. Salt on a new form of	49	116
Truss-pads for hernia, Mr. Wood's new	49	111
Tuberculosis, Dr. Jones on inflammation as an exciting cause of	51	53
Tuckwell, Dr. H. M., his case of spontaneous thrombosis	49	104
Tufnell, Mr. J., on the immediate treatment of fracture by fixed apparatus	51	120
Tumour, fibrous, of the uterus, Mr. Duncan on the source of hemorrhage in	49	224
" fibrous, of the uterus, Mr. Brown on the treatment of	49	222
" fibrous, of the uterus, Mr. Brown's operation for	51	247
" malignant fibro-cellular, Dr. Buchanan on	49	137
" vascular, of the meatus urinarius	49	249
" mammary, Mr. Bryant on the diagnosis of	50	261
" of the tongue, Mr. Bulley on galium aperinum in	50	108
" ovarian, Dr. Clay's cases of extirpation of	50	249
" uterine, Dr. Gairdner's cases simulating ovarian disease	50	240
" uterine fibrous, Dr. Routh on the diagnosis of	50	232
" uterine fibrous, Dr. Routh on the treatment of	50	236
Tumours of the jaws, Prof. Fergusson on the operations for	52	112
Turnbull, Dr. J., on the treatment of chorea	51	50
Turning in cases of disproportion, Dr. Inglis on	51	231
" in cases of shoulder presentation, Dr. Christie on	50	211
" in certain cases of labour, Dr. Hewitt on the advantages of	50	209
" versus forceps, Dr. Graily Hewitt on	50	209
Tympanum, Mr. Hinton on a simple method of inflating the	49	179
Typhoid fever in Penicuik, Dr. Thin on	51	24
" fever, Dr. Osborn on sulphuric ether and ammonia as an anti-spasmodic in	50	35
Typhus fever, Dr. Christianson on	49	17
" fever and its prevention, Dr. Mapother on	50	25
" and typhoid fevers, Dr. Gairdner on the treatment of	51	21
" and typhoid fevers, Dr. Wilks on the use of stimulants in	51	17
Tyrell, Mr., on tetanus from burn treated by the application of tobacco	49	54

	PART	PAGE
Tyrrell, Mr. A. J., on the local application of tobacco in tetanus	50	37
Ulcer of the tongue of twenty years' standing, Dr. Leared's cure of	52	185
Ulcers, chronic, Mr. Skey on the treatment of	49	139
Uranoplasty, Mr. Collis on	51	152
Urates, Dr. Harley on the pathology of deposites of	50	69
Uræmia, Dr. Harley on	49	275
Urea, Dr. Harley on	49	72
Urethra, stricture of, Mr. Holt's treatment of	50	132
" stricture of, Mr. Rutherford on the treatment of	52	155
Urethritis, Dr. Cruise on the treatment of	51	180
Uric acid calculus, Dr. Harley on the treatment of	50	70
Urinary analysis in obscure diseases, importance of	50	76
Urine, Dr. Harley on the	49	71
" Mr. Donovan's simple method of estimating the alkilinity or acidity of	49	69
" Dr. Clarke on detecting small quantities of albumen in	50	127
" Dr. Harley on the colouring matters of the	50	74
" incontinence of, in boys, Mr. Adams's treatment of	50	271
Urinary affections, Dr. Hassall on the action of perchloride of iron in	51	112
Urohæmatin, excess of, in the urine in various diseases	50	76
Uterine fibrous tumours, Dr. Duncan on source of hemorrhage in	49	224
" fibrous tumours, Dr. Sadler's case of removal of	49	223
" fibrous tumours, Mr. J. B. Brown on the treatment of	49	222
" fibrous tumours, Dr. Routh on the diagnosis of	50	232
" fibrous tumours, Dr. Routh on the treatment of	50	236
" fibrous tumours, Mr. Brown's operation for	51	247
" hemorrhage, Mr. Baker Brown on incision of the os and cervix for	50	222
" hemorrhage from fibrous tumours and polypi, Mr. Baker Brown on	50	222
" hemorrhage after delivery, Dr. Lumley Earle on	50	219
" sound, with movable head, Dr. Earle's	50	226
" tumour simulating ovarian disease, Dr. Gairdner on	50	240
" polypi, Dr. Marion Sims on the removal of	51	259
" porte-castique, Dr. Lente's new	49	241
" action in labour, Dr. Brown on the use of ergot in inducing	52	225
Uterus, Dr. Emmet on a radical operation for prolapsus of	52	257
" Dr. Dyer's case of rupture of the	52	229
" Dr. Kelley on inversion of the	49	188
" Mr. Sheppard on inversion of the	49	191
" bladder, and rectum, Mr. Brown on prolapsus of the	49	226
" irritable, Dr. Griffith on the treatment of	49	231
" physiological action of, in parturition	49	236
" prolapsus of, Mr. Davies on the treatment of	49	229
" Dr. Lumley Earle on introduction of the hand into the cavity of	50	220
" retroflexion of, Mr. Baker Brown on the treatment of	50	246
" rupture of, Dr. Crighton on gastrotomy in	50	215
Vaginal injections, Dr. Rasch's instrument for	51	267
Varicose veins, Mr. Skey on the treatment of	49	98
Vascular tumour of the meatus urinarius, Dr. Griffith on cases of	49	249
Veale, Dr. H., on syphilis and its treatment	50	197
Venice turpentine, solution of, as a dressing for wounds	52	186
Vesico-vaginal fistula, Mr. Bryant on the treatment of	52	256
Vesico-uterine fistula, Mr. Lane's case of	49	217
" fistula, Mr. Pollock on the operation for	49	221
" fistula, Dr. Banon on the surgical treatment of	51	268
Vinum ferri, its use in consumption	50	52
Wallis, Dr. G., on the causes of cholera and its effects on the system	52	269
Walton, Mr. H., on iridectomy	49	155
Ward, Dr. S. H., on intermittent fever	50	28
Wasting palsy, Dr. Radcliffe on	51	44
Walters, Dr. A. T. H., on ascultatory signs which precede crepitation in pneumonia	51	63
Watson, Dr. J., on carbolic acid	49	260
" " on the treatment of alopecia areata by carbolic acid	50	139
" Dr. W. S., on phlyctenular keratitis, and vascular ulcers of the cornea	51	195
" Dr. P. H., on acupressure as a hæmostatic agent	52	127
Wells, Mr. T. S., on hysterotomy	52	234
" " his syphon-trocar and hooked canula for ovariotomy	49	212

	PART	PAGE
Wells, Mr. T. S., on the diagnosis between ovarian dropsy and ascites	50	253
" on practical details in ovariotomy	50	256
Wenzell, Mr. W. T., on the active constituents of ergot of rye	50	227
" Mr., on the active constituents of ergot	52	225
Wilks, Dr., on the action of digitalis on the heart	52	76
" on the treatment of typhus and typhoid fevers without stimulants	51	17
Willow bark, M. Cazin on the febrifuge properties of	50	36
Wilson, Mr. E., on the nosophytodermata	49	126
" on the nature, varieties, and treatment of eczema	51	181
Wire loops as substitutes for pessaries, &c., Dr. Clay on	49	181
Wire compress as a substitute for the ligature, Mr. Dix on the	50	90
" ligature, Mr. Holt's cases in which it was left in the wound	50	98
Wood, Mr. J., on the operation for the cure of rupture	49	114
" on the radical cure of inguinal rupture	49	272
" on truss pressure in inguinal hernia	49	111
Wood-oil, or gurgina balsam, in gonorrhœa	51	226
Worms, round and thread, Dr. Anderson on the use of santonine in	49	284
Wounds into large joints, Dr. Monro on	50	88
" Mr. Holmes on exposure of articular cartilages in	51	129
" penetrating, of the knee-joint, Dr. Newman on irrigation in	51	129
" M. Kerner on solution of Venice turpentine as a dressing for	52	186
Wright, Dr. E. P., on the operations for trichiasis	51	193
Zymotic diseases, Dr. De Ricci on sulphites and hyposulphites in	49	22
" diseases, Dr. De Ricci on use of sulphites in	50	31
" diseases, Dr. Mapother on the prevention of	50	25
" diseases treated by alkaline sulphites, Dr. De Ricci on	52	17

THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

JAMES BRAITHWAITE, M. D., LOND.

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CONTENTS OF VOL. LIII.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE		AUTHOR.	PAGE
1	On Fevers	*Dr. H. Bence Jones*	17
2	On the Thermometer in Specific Fever		21
3	Thermometrical Observations on the Fevers of Children	*G. Stevenson Smith, Esq.*	23
4	On the Use of the Thermometer as a Guide in the Diagnosis of Pyrexial Diseases	*Dr. F. W. Gibson*	30
5	Observations on the Present Epidemic of Typhus.	*Dr. Robert Perry*	34
6	Researches on the Daily Excretion of Urea in Typhus Fever, with Remarks	*Dr. Keith Anderson*	38
7	On Typhus Fever	*H. W. Randolph, Esq.*	39
8	On an Outbreak of Typhoid Fever with Special Reference to its Etiology, and Complication of Diphtheria.	*Dr. D. Campbell Black*	41
9	On the Treatment of Typhoid Fever	*Dr. T. King Chambers*	43
10	On Acute Rheumatism and Acupressure	*Donald Ma gregor, Esq.*	44
11	On Blistering in Acute Rheumatism	*E. Howard Moore, Esq.*	45
12	Citric Acid as an Application in Cancer	*Charles J. Denny, Esq.*	45
13	On a Case of Removal of Cancerous Tumour of the Arm	*John Hilton, Esq.*	46
14	Polydipsia—Treatment by Large Doses of Valerian—Improvement	*Dr. Keith*	48
15	On the Treatment of Disease by Oxygen	*Dr. R. H. Goolden*	48

DISEASES OF THE NERVOUS SYSTEM.

16	On Progressive Locomotor Ataxy, or Wasting of the Posterior Columns of the Spinal Cord	*Dr. Julius Althaus*	50
17	A Case of Progressive Locomotor Ataxy	*Dr. Johnson*	54
18	A Case of Progressive Locomotor Ataxy	*Dr. Hughlings Jackson*	55
19	On Spasm	*Dr. C. Handfield Jones*	56
20	On the Causes, Diagnosis, and Treatment of Functional Nervous Affections	*Dr. C. E. Brown-Sequard*	60
21	On the Moral Treatment of Functional Nervous Diseases	*Do.*	63
22	On the Use of Bromide of Ammonium in the Sleeplessness of Functional Nervous Diseases	*Do.*	64
23	Cases illustrating the Use of Belladonna and Bromide of Potassium in Epilepsy	*Dr. Broadbent*	64
24	On the Treatment of Epilepsy	*Dr. C. E. Brown-Sequard*	66
25	On the Use of Chloroform in Epilepsy and Hysteria	*Do.*	98
26	On the Use of Ice Bags in Convulsions and in Cholera	*Dr. John Chapman*	69

ARTICLE		AUTHOR.	PAGE
27	Chorea Treated by Calabar Bean	*Dr. John W. Ogle.*	71
28	Subcutaneous Injection of Narcotics for Neuralgia.	*Do.*	72
	ORGANS OF RESPIRATION.		
29	On the Nature, Cause, and Treatment of Tuberculosis	*Dr. Horace Dobell*	73
30	New Bi-Valve Speculum for Examining the Larynx, Invented and Patented by Dr. Labordette, of Lisieux		81
31	On the Thermometer as a Means of Diagnosis between Tubercular and Non-Tubercular Diseases	*Editor of Medical Times and Gazette*	82
32	On a Certain Form of Hemoptysis Unassociated with Pulmonary Tuberculosis	*Dr. R. Payne Cotton*	85
33	On the Use of Phenic (Carbolic) Acid for the Cure of Phthisis	*Dr. J. R. Wolfe*	87
	ORGANS OF DIGESTION.		
34	Concerning the Treatment of Constipation and of Stoppage of the Bowels—with Special Reference to the Use of Atropia and of Galvanism	*Dr. Alex. Fleming*	88
35	On the Treatment of Diphtheria with Hyposulphite of Soda	*J. C. Maynard, Esq.*	93
36	On Tracheotomy in Croup and Diphtheria	*Dr. George Buchanan*	94
37	On Hepatic Disease amongst Europeans in the East Indies	*Inspector-General Sir Ranald Martin, C. B.*	97
38	On Wheat Phosphates	*Dr. Tilbury Fox*	101
	URINARY ORGANS.		
39	On Symptoms arising from Alkaline Urine	*Dr. H. Bence Jones*	102
40	On the Treatment of Ammoniacal Urine	*Do.*	102
41	On the Symptoms and Consequences of Interstitial Nephritis	*Do.*	104
42	On the Treatment of Tubular Nephritis	*Do.*	105
43	On Secondary Albuminuria (Cause not Primarily in the Kidney)	*Dr. George Harley*	106
44	On the Treatment of Bright's Disease	*Do.*	110
	SURGERY.		
	DISEASES OF BONES AND JOINTS, ETC.		
45	On the Use of Chloride of Zinc in Surgical Operations and Injuries	*C. De Morgan, Esq.*	112
46	Forcible Flexion of the Knee for Anchylosis in the Straight Position	*Dr. Philip C. Smyly*	121
47	Subacute Synovitis and Contracted Joints	*W. Adams, Esq.*	122
48	On an Easy Method of Reducing Ordinary Dislocations of the Shoulder-Joint	*W. E. Porter, Esq.*	123
49	On the Treatment of Fracture of the Radius at the Styloid Process by Means of Gordon's Splint.	*Lawson Tait, Esq.*	123
50	On some Injuries to the Head and Neck of the Humerus	*J. Hutchinson, Esq.*	126
51	On a New Form of Leg-Suspender and Bed-Guard	*Henry Greenway, Esq.*	128

ARTICLE		AUTHOR.	PAGE
52	Leather-Felt Splints		130
53	How to make Gutta-Percha Splints	*Edwin Truman, Esq.*	131
54	On a Case of Recurring Fibroid Disease of the Shoulder	*Sir William Fergusson*	131
	ORGANS OF CIRCULATION.		
55	On the Galvano-Puncture of Aneurism	*Dr. John Duncan*	133
56	On Ligature of the Femoral Artery	*Prof. Syme*	139
57	On the Treatment of Popliteal Aneurism	*John Hilton, Esq.*	140
58	On a Case of Wounded Ulnar Artery, followed by Sloughing and Secondary Hemorrhage, and requiring Amputation	*Do.*	142
	ORGANS OF URINE AND GENERATION.		
59	On the Proofs that Lithotrity is an Eminently Successful Operation	*Henry Thompson, Esq.*	143
60	On a New Instrument for Facilitating the Operation of Lithotomy	*Henry Lee, Esq.*	145
61	On the Treatment of Acute Orchitis by Puncture and Compression (Immediately after Puncture) by Strapping	*Dr. W. Spencer Watson*	145
	DISEASES OF THE SKIN.		
62	The Actual Cautery for Nævi	*J. R. A. Douglas, Esq.*	146
63	On the Nature and Treatment of Chilblains	*Dr. Henry S. Purdon*	147
64	Cases of Pruritus Senilis	*Dr. H. Beigel*	148
65	On Eczema	*John Gorham, Esq.*	148
66	Treatment of Acute Eczema	*T. P. Johnston, Esq.*	149
	DISEASES OF THE EYE AND EAR.		
67	On Examining the Fundus Oculi	*Dr. Roseburgh*	149
68	On the Treatment of Granular Ophthalmia by Pressure	*Dr. W. Stokes, Jun.*	149
69	On the Treatment of Ophthalmia	*Ernest Hart, Esq.*	151
70	On Black Cataract	*Haynes Walton, Esq.*	152
71	On the Treatment of Strabismus without Operation	*C. Holthouse, Esq.*	153
72	On Atropine Points	*J. Z. Laurence, Esq.*	154
73	Blue Glasses		155
74	Treatment of Malignant and Recurrent Tumours of the Orbit		155
75	A New Otoscope or Speculum Auris	*Dr. John Brunton*	156
76	On a New Method of Applying Remedial Agents to the Cavity of the Tympanum	*Dr. Edward Bishop*	157
77	On Difficulty of Hearing in Cases of Perforation of the Membrana Tympani	*Dr. F. E. Weber*	158
	SYPHILITIC DISEASES.		
78	On Syphilitic Inoculation in 1865	*Henry Lee, Esq.*	160
79	On the Treatment of Syphilis	*Do.*	165
80	On Syphilization	*James Lane, Esq.*	171

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

ARTICLE		AUTHOR.	PAGE
81	On Puerperal Fever	*Dr. Snow Beck*	173
82	On the Use of Wire-Ribbon in Some Cases of Difficult Turning	*Dr. Val. Heyerdahl*	176
83	A Remarkable Case of "Missed Labour"; the Fœtus being Retained in the Abdomen Forty-Three Years	*R. W. Watkins, Esq.*	178
84	On Enlargements of the Uterus which follow Abortions, and Premature or Natural Confinements; with Cases	*Dr. Snow Beck*	179
85	On Hemorrhage from Detached Placenta near the Eighth Month of Gestation: Labour Induced by Barnes's Dilator: Recovery	*W. J. Tubbs, Esq.*	181
86	Abstract of a Paper on Procidentia Uteri	*Dr. J. Marion Sims*	182
87	On the Radical Cure of Prolapsus Uteri	*Robert Ellis, Esq.*	185
88	On the Operation for the Radical Cure of Prolapsus Uteri by Caustic Applications	*Dr. C. H. F. Routh*	188
89	Case of Polypus Uteri: with Remarks	*Dr. J. G. Swayne*	192
90	On a New Uterine Support	*Dr. William Main*	195
91	On Extreme Flexions of the Uterus	*Dr. Henry G. Wright*	198
92	A New Uterine Dilator	*J. W. Hembrough, Esq.*	202
93	On Vaginal Rectocele	*James R. Lane, Esq.*	203
94	A Case of Ovariotomy in which the Pedicle was Tied and Returned, and the Ligature Removed in Forty-Eight Hours	*Dr. J. H. Aveling*	207
95	On Chlorate of Potass in the Treatment of Ovarian Disease	*W. Craig, Esq.*	209
96	On the Treatment of Ovarian Tumours	*I. Baker Brown, Esq.*	211
97	On Iodine in the Treatment of Uterine Leucorrhœa	*Dr. Murray*	211
98	On Dysmenorrhœa, Metrorrhagia, Ovaritis, and Sterility, depending upon a Peculiar Formation of the Cervix Uteri; and the Treatment by Dilatation or Division	*Dr. Robert Barnes*	211
99	On Improvement of the Blood in Chlorosis, and on the Elimination of Certain Poisons	*Dr. C. E. Brown-Sequard*	221
100	Glandular Tumour of Mamma, with Cysts; Injection with Compound Tincture of Iodine	*W. Martin Coates, Esq.*	221
101	Cure of a Large Ranula by Injection of Compound Tincture of Iodine	*Do.*	222
102	On a Case of Spina Bifida cured by Injection of a Solution of Iodide of Potassium	*Do.*	223
103	Practical Remarks on the Loosening of the Articulations of the Pelvis		224
104	On the Mortality of Childbed as Affected by the Number of the Labour	*Dr. J. M. Duncan*	226
105	On Liebig's Food for Infants and Invalids	*Dr. A. Hill Hassall*	228

ADDENDA.

106	On the Citric, Acetic, and Carbolic Acids in Cancer	*Dr. John Barclay*	230
107	On the Uses of the Medicinal Sulphites and Hyposulphites in Zymotic Diseases	*Dr. Polli*	232
108	Dr. Hassall's Flour of Meat		235
109	On the Concentration and Preservation of Meat	*Dr. A. Hill Hassall.*	236

ARTICLE	AUTHOR.	PAGE
110 On the Preservation of Milk	*Dr. J. Scoffern.*	237
111 A Case of Resuscitation, after Drowning, by a Combination of the Plans of Dr. Silvester and Dr. Marshall Hall		238
112 On the Action of Fungi in the Production of Disease	*Dr. Tilbury Fox*	239
113 Obstruction of the Bowels, Successfully Treated by the Use of the Stomach-Pump	*Dr. Ebenezer Fleming*	248
114 On the Sphygmograph	*Editor of the Lancet*	249
115 Therapeutical Notes	*Dr. Headland*	250
116 On the Physiological Operation of the Preparations of Iron	*Dr. A. Sasse*	251
117 Sinapine Tissue, or Mustard Paper	—— *Cooper, Esq.*	252
118 On the Subcutaneous Injection of Morphia and Atropine	*Dr. C. E. Brown-Sequard*	252
119 On the Absorbing Power for Medicines possessed by Certain Parts of the Body	*Do.*	252
120 Chloroform as a Means of Detecting Bile in the Urine	*M. Cuniasse*	253
121 An Antidote for Prussic Acid		253
122 On Alcohol	*Dr. Henry Munroe*	254
123 The Course of a Dose of Alcohol through the System	*Dr. H. Bence Jones*	255
124 On a New and Ready Mode of Producing Local Anæsthesia	*Dr. B. W. Richardson*	256
125 On Rhigolene, a Petroleum Naphtha for producing Anæsthesia by freezing	*Dr. Henry J. Bigelow*	265
126 On Anæsthesia by Mixed Vapours	*Robert Ellis, Esq.*	267
SYNOPSIS		272
INDEX.		291
List of New Publications		296

PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON FEVERS.

By Dr. H. Bence Jones, F.R.S.

[Perhaps the term zymotic disease is as good as any to designate the action of those poisons which get into the system in such a mysterious way that we cannot explain them. The term implies some action by which different vegetable or animal ferments, living or dead, can be introduced into the body and produce different diseases, such as small-pox, scarlet fever, measles, typhus fever, &c.]

Of all the modified peroxidations that can occur in the body, small-pox is the most definite, because the poison can be got apart, and the quantity necessary for producing the action can be fixed, and through the most glorious discovery of vaccination it can be set in action whenever we please. We can almost see it passing from the cellular tissue into the blood, and from the blood into every particle of every texture, rendering it incapable of undergoing the same action again.

Let us look a little closer at this action of small-pox poison. If the minutest particle of substance, a little dried albuminoid substance, in a peculiar chemical state of action, on a lancet, or in the dust of the air, is put into the cellular tissue or is inhaled into the lungs, it passes on to the blood, and through it into every texture. In a few days the chemical actions of oxidation and nutrition throughout the body are altered, and the particle of matter has reproduced itself immeasurably. The violently increased chemical action, the peroxidation, is shown by the increased heat of the body, the violent fever. The altered nutrition is evident not only in the eruption of pustules in the cellular tissue under the skin, but in the altered condition of the blood and in all the textures of the body; each particle of substance being rendered incapable of undergoing the same process again, and by assimilation every future particle that takes the place of every modified particle is also generally incapable of being modified again.

Throughout the course of the general peroxidation, and more especially at the end of the fermentation, local peroxidations frequently come on in any part of the body. Inflammations of the eyes, the ears, the mucous membranes, the joints, the serous membranes, the parenchymatous tissues, anywhere an unmodified peroxidation is ready to begin, and this easily gives rise to suppuration or causes obstructions which the feeble circulation cannot overcome.

The most striking facts concerning this small-pox ferment are, first, the very small quantity of substance that produces so much effect; secondly, the immeasurable increase of the poison in the body, each pustule having the same property as the original ferment; thirdly, the period of incubation during which the poison must at first slowly increase in every texture, and there give rise to the modified peroxidation and altered nutrition which constitute the attack.

The poison of scarlet fever, of measles, and of typhus, though less tangi-

ble, are not less substantial than the small-pox ferment. Like it, they can most probably be dried and carried from place to place and pass into the mouth with the dust which we each moment inhale or swallow. In chemical composition scarlet fever, measles, and typhus ferments most probably resemble albumen in complexity, and, like albumen, they may be altered in composition and action by heat, alcohol, arsenic, tar acids, and many metallic salts. As soon as they reach any spot where they can oxidise, they set up an oxidation and reproduction in each contiguous particle of albuminous substance. From the cellular tissue, the air passages, or the stomach or bowels, the contact action spreads into the blood, and there it multiplies, whilst it is carried into all the capillaries, and through them into every texture of the body; then the increased oxidation and formation of ferment becomes most violent, and fever to a greater or less degree is present. Long after the strongest action is reached, a slower action continues, and at any time or in any part or texture of the body, whilst the specific chemistry is going on, an ordinary local peroxidation may be lit up, and a more or less acute inflammation may be added to or follow the fever which the ferment has produced.

During the height of the fermentation in typhus fever, the heat may rise to 5, 6, or even 10° Fah. above the ordinary temperature; and when the fermentation is ended, the albuminous textures of the body are so changed that they are incapable of going through the same process again. Between these two results there are innumerable other products of chemical change, varying with the kind and degree of fermentation. In typhus fever it is said that urea is increased and carbonic acid diminished. To these and a multitude of other chemical questions regarding fermentation chemistry will give definite answers; but above all questions, one of the most difficult to answer and yet one of the most important, is the amount of oxygen that is consumed in the different kinds and degrees of peroxidation which can take place within us.

In each organ according to the intensity of the action set up by the ferment, altered functions may arise, and these may be still more altered when an ordinary peroxidation at the same time takes place. Thus the brain, heart, lungs, kidneys, liver, or any texture composing these organs may show by more or less wrong mechanical results the effect of the ordinary or modified peroxidation; and the effects of the fever and of the inflammation may be so mixed that neither during life nor after death may any accurate separation be possible.

Closely related in chemical composition to these violent ferments are the less active ferments of ague and typhoid fever. There is so little difference in the chemical composition of animal and vegetable substances that the distinction between animal and vegetable poisons is no longer possible. Vegetable albuminous matter undergoing change may produce almost, if not quite, exactly the same poison as animal albuminous matter. Hence, probably, the resemblance between ague poison, and the typhoid fever poison, and the possibility that sometimes one and sometimes the other of these poisons may be formed from the same changing matter under different circumstances.

Ague ferment is probably a highly complex nitrogenous substance, capable of being dried and carried by the wind far from the place where it was produced. It enters by the mouth with the dust, and, like animal or vegetable alkaloids, it passes from the blood into every texture of the body, and acts on each much or little, according to its chemical properties. Probably it acts most strongly on the nerves that regulate oxidation, causing for a time contraction of the arterial vessels, and consequent suboxidation everywhere. The increased obstruction of the small arteries reacts on the tension of the blood, and this produces increased contraction of the heart, which continues to increase until the obstruction yields, and a state of peroxidation is set up, by which the poison is partially destroyed. During the remission, probably the poison is reproduced until sufficient, in from one day to three days, is formed to go through the same action again.

This theory of ague admits of a reasonable explanation of the action of quinine and arsenic in stopping the paroxysms of the complaint. On the ague poison itself quinine and arsenic may have no action, but they pass into

every texture from the blood, and, combining with the nervous substance on which the ague poison acts, they form a compound on which the ague poison is incapable of producing an effect before it is oxidised and destroyed.

The ague poison, unlike the small-pox or typhus fever ferment, instead of protecting the body by making it incapable of undergoing the same action again, makes the nerves more ready, on the slightest renewal of the poison to undergo the same action again; so that it has been said that the ague poison may lie dormant for years. It is far more probable that a much smaller quantity of the poison can produce the return of the symptoms than that the original ferment should retain its properties for months, or even for years, after its first action had passed by. In this respect, and in some others, the action of ague poison proves that it is a very peculiar ferment, and hence, though I have placed it near to the typhoid ferment because of its origin, I must shortly point out to you the different effect which the true typhoid ferment produces.

The typhoid ferment is probably formed out of vegetable or animal albuminous substance. In sewers, in drains, in ditches, possibly even in the drains of the human body, a substance may be formed which is not volatile in itself, but by foul gases or currents of air can be carried into the mouth, and in some period between a few hours and fourteen days it sets up a modified peroxidation. More slowly absorbed and less rapidly reproduced and changed than typhus ferment, it passes into the blood, and from it into each texture; whilst some of the poison has a local action on the glands of the small intestine, and produces increased action, effusion, obstruction, and retrograde action, causing ulceration, sloughing, and even perforation, by which mechanically the contents of the bowel may escape and an uncontrollable simple peroxidation may be set up. The poison that has passed into the tissues acts on each organ more slowly than the typhus poison; still, like it everywhere, it gives rise to altered functions, and everywhere local peroxidations are ready to occur; bronchitis, pneumonia, peritonitis, tubular nephritis, cystitis—any of these and many other inflammations may be set up at any time during the course of the fever. Probably the substances produced by the increased chemical action in typhus and typhoid fever will be found to be very similar. There may be the same amount of heat, the same excess of urea, the same excess of antecedent substances from which the urea is formed; possibly the same consumption of oxygen when the same temperature in each fever occurs; but in the properties of the ferment formed in the body a distinct difference of diffusibility must exist, the typhus poison passing with greater ease into neighbouring bodies; whilst the typhoid poison rarely, if ever, is communicated by infection.

On the Mechanical Disorders that arise out of the Chemical Errors in Fevers.—In all fevers the loss of mechanical power is quite as remarkable as the increase of chemical action. The chief amount of energy liberated by the action of oxygen in the body seems expended in the production of heat, so that far less than the ordinary amount of power remains to be employed in the production of mechanical motion. The muscles may be considered as machines made for the conversion of chemical force into mechanical motion. How this is done cannot be explained in the present state of our knowledge of the mechanical, chemical, or electrical actions in the muscle; but that the muscular force arises from some equivalent force, and sooner or later must come from the chemical force in oxygen, hydrogen, and carbon, opens an immense field for investigation, and is easier of belief than that force should be each moment created and destroyed. The amount of sugar and fat present in any muscle would soon be used up if in the working of the muscle itself fresh fuel was not produced. The action of oxygen on the syntonin in the muscle may be direct, and may give rise to the force required; but it is more probable that the syntonin splits into substances of two classes, one ending in urea, which is incapable almost of combustion, the other in inosit, which would immediately give water and carbonic acid.

In fever the poisonous ferment in the muscle probably determines a different chemical action from that which takes place in the muscle in health.

The increased heat and increased urea mark the increased action, but loss of motor-power in the muscles shows that the conversion of chemical into mechanical force does not take place.

This mechanical disorder becomes by its action on the muscles of respiration or circulation the source of complications and dangers in fever to which I must shortly allude. Gradually in the course of fevers the sounds of the heart may be found to become more and more feeble, and the respiration, without any wrong in the lung, becomes shallow and weak from the diminished power in the muscular tissue. The diminished tension in the arteries has a direct effect upon the circulation through the capillaries, and the motion in the veins is more or less stopped; hence congestion of blood in the venous system occurs, hemorrhages, effusions, and coagulations in the veins may take place anywhere. The imperfect action of the muscles of respiration produces the same mechanical effects in the circulation through the lungs; imperfect oxygenation takes place in the lungs from the stoppage of blood in the pulmonary veins; without any inflammation, œdema of the lungs, hypostatic consolidation may occur. The circulation through the lung is so feeble that even the force of gravity acting on the blood in the lungs cannot be counteracted, and accumulation takes place in the most dependent parts.

The muscles of the bladder are also so weakened that the urine accumulates, and frequently external muscular pressure is required, after the catheter has been passed, to cause the urine to flow.

In another large class of zymotic diseases the qualitative and quantitative errors of oxidation are scarcely detectable, whilst the qualitative and quantitative errors of nutrition chiefly mark the action of the poison.

Of these diseases true syphilis may be taken as the type.

It can scarcely now be doubted that the actions of two poisons have been included under the term syphilis. The first, like impetigo, is capable of being communicated, and often repeated, because it exists only locally, or passes up to the nearest lymphatic glands; whilst in the true syphilis the poison from the local sore enters the blood and passes from it into each texture, where it multiplies and produces changes of nutrition, and, partly unchanged and partly changed in composition, passes out perhaps in each secretion.

This true syphilitic ferment resembles very closely the small-pox ferment in the universal diffusion of the poison, and in the consequent protection it gives from another attack by rendering a second similar change in each texture impossible. The protective power of the alteration is to a slighter degree extended to the progeny through the germ and spermatozoon; so that a race partly protected by inheritance may suffer less from these diseases than a purer race, whose textures are free to undergo the full change which constitutes the disease. Both poisons give rise to increased cell growths, effusions, oxidations, congestions, and ulcerations; and these may take place in any part of the body, for the poison exists everywhere.

True syphilis differs, however, altogether from small-pox in its definiteness of course as to time. Syphilis produces no fever to terminate the fermentation in a definite period, and it may consequently remain active or dormant in the textures for years.

It is vain now to ask what circumstances at the end of the fifteenth century produced the first modified albuminoid matter which gave rise to the first true syphilitic poison. In cancer, which bears a distant resemblance to syphilis, although the spontaneous generation of the first cancer cell is daily occurring in some predisposed texture, yet we are as yet quite unable to say what produces the first modified particle of matter which multiplies and communicates its composition to adjacent predisposed textures by contact, and is carried by lymphatics and blood vessels to every part of the body, and affects the nutrition of each part with which it comes in contact, provided the textures are in a condition to propagate the cancer cells.

Another instance of spontaneous generation of a poisonous ferment is presented to us in rabies; and with this poison also, unless a peculiar condition of system exists, the ferment when inserted has no action; and here also our

knowledge is at present unable to say what circumstances determine the formation of the first particle of poisonous saliva; except by its effects, the peculiar change in the albuminoid matter of the saliva in the present state of chemical knowledge could not be recognised.—*Med. Times and Gazette, March* 17, 1866, *p.* 275.

2.—ON THE THERMOMETER IN SPECIFIC FEVER.

The subject of the temperature of the body in disease is exciting much attention at the present time in the metropolitan hospitals. A few words respecting the thermometers in use will probably be interesting to such of our readers as may wish to commence observations for themselves.

The instruments which we have seen employed are of English manufacture. A cheaper form, of German make, may be procured; but it is said that these are not so exact or convenient. The thermometer is either simple or self-registering. The former must be read *in situ*; the latter can be removed from the patient, and read when convenient. It may be applied, therefore, by the nurse during the absence of the medical attendant, who can examine its record at his next visit. The contrivance by which the instrument is self-registering is very ingenious. It was suggested more than twenty years ago by Professor John Phillips, of Oxford, that, as no thermometer was perfectly free from air, the portion of air present might be utilized. By a little manœuvring a small portion of the column of mercury was detached from the remainder, the speck of air occupying the intermediate space. It is evident that, as the column rises under the influence of heat, this detached portion, or index, is carried before it. When the column recedes under contraction, the index remains at the point to which it had been propelled. This is a great improvement on Rutherford's register, in which the index was of steel, and could not be relied upon.

Dr. Aitken, to whom is largely due the credit of introducing this interesting subject into the medical practice of England, has arranged, with the aid of Mr. Cassella, the Admiralty instrument maker, two thermometers, graduated in Fahrenheit degrees, for the especial use of medical practitioners. One is an ordinary, but very sensitive, instrument, made with a curve, so that the bulb may be easily fitted into the axilla. The other is straight, and on the self-registering principle. Dr. Ringer employs a slighter and more sensitive instrument, without a register. Dr. Gull has had the registering thermometer made a few inches shorter, and thereby rendered it much more portable This latter instrument answers perfectly, but requires a little care in its use. The column of mercury, being necessarily shorter than in the ordinary thermometer, sinks, at a low temperature, into the bulb; and if the instrument be shaken under these circumstances (and a kind of jerk is necessary to arrange the index), there is danger of the speck of air getting displaced, and the self-registering principle consequently destroyed. This mischance, however, is easily obviated by warming the bulb before the necessary jerking movement is made.*

During all the acute specific fevers, the temperature of the body is abnormally elevated; and, as is the case with acute inflammation of any of the tissues of the body, the severity and duration of the fever are exactly measured by the degree and duration of the elevation of the temperature. This elevation of the temperature is constant. This, however, is not the case with the other symptoms met with in typhoid fever; for the pulse may remain normal throughout the attack, and the tongue may be clean; whilst even, in rare cases, during the greater part of the attack, the appetite may not be greatly impaired. It is thus evident that the temperature affords more reliable information respecting the severity of the attack of typhoid fever than the other

* All these thermometers can be obtained of Mr. Casella, 23, Hatton-garden. The case of Dr. Aitken's thermometers contains some very useful remarks upon the employment of the instrument extracted from Aitken's "Science and Practice of Medicine."

symptoms. The invasion of typhoid fever is almost invariably gradual. This is well shown by the temperature. During the first four days of the attack the elevation of the temperature gradually increases; but, at the end of the first week, it has usually nearly attained to the maximum temperature of the attack. It is true that this gradual increase of the disease is also shown by the other symptoms, and especially by the gradual increase of the weakness of the patient. The temperature of the body, however, does this more precisely. The other acute specific fevers, and all inflammations, mostly begin abruptly. The other disease that corresponds to typhoid fever in respect of the gradual invasion of the disease, is tuberculosis. Thus, if we meet with a malady the invasion of which has been gradual, we mostly have to do either with typhoid fever or tuberculosis. Though at present the temperature of the body has not afforded us much information in respect of the treatment of typhoid fever, it assists us greatly in arriving at a correct diagnosis and prognosis.

First, in respect of the diagnosis. During the first three or four days, beyond the gradual increase in the elevation, the temperature is in no way characteristic; but if on the fourth or fifth day the maximum temperature attained during the twenty-four hours be not 103·5°, the disease is most probably not typhoid fever. And further, if on the first or second day the maximum temperature reaches 104° the disease is some other than typhoid fever. The information given us by the temperature is great, for often the diagnosis of typhoid fever at its commencement is difficult, inasmuch as the characteristic rash does not usually appear before the sixth, and may be postponed to the twelfth, day of the disease; and, indeed, in children is not unfrequently absent throughout the attack. The temperature affords still greater information in respect of the prognosis. It is during the second week that such information is obtained. From the behaviour of the temperature during this time we can speak confidently as to whether we have a mild or severe case to treat.

In typhoid fever we have always at one period, and sometimes throughout the attack, great daily variations in the temperature. When such occurs, it is usually low in the morning, and attains its maximum in the evening, mostly between seven and twelve P. M. The greater these fluctuations at the end of the second week, the more favourable is the attack, and the shorter will be its duration. If the temperature falls considerably in the morning—that is to say, if it sinks below 102° Fahr.—even though the evening rise may be great, the prognosis is favourable. The greater the fall the more favourable is the prognosis. On each succeeding day the amount of the morning fall increases. If during this period (second week) the maximum temperature be great, the disease is proportionately severe; thus, a daily rise to 105° indicates a severe, and to 106° a still more severe attack. Should the temperature during the second week of the fever remain persistently high, we may expect a severe and long-continued attack. Thus, a continuous elevation of the temperature at 104° Fahr. is serious, whilst a morning temperature of 104° with an evening rise to 105° and 106° is still more serious, and calls for a grave prognosis. Such cases are, however, by no means always fatal.

In these cases of persistent elevation of the temperature, the earliest sign of improvement is a fall of the morning temperature. When such a fall occurs, and especially if it be repeated on the subsequent days, even though the maximum temperature reached in the evening remain the same, we may be sure that the fever has begun to decline. Though the temperature thus affords early and great information respecting the degree of severity of the attack, still it is not the only condition that determines the prognosis. It is necessary to take into consideration the state of health of the patient previous to the attack. If this has been good, the prognosis is far more favourable than if the health has been impaired by excesses of any kind, or by bad hygienic conditions of life. The problem to be solved is—Given a disease of known severity (and the temperature affords us this information in typhoid fever) can the life of the patient be supported during this period?

The temperature affords us still further information. Thus, in typhoid

fever, as in all febrile cases, a sudden increase in the elevation of the temperature above what has occurred on previous days proves the existence of some intercurrent disease. The temperature often gives us this information when the intercurrent disease would pass undiscovered; for the symptoms in such cases are often either masked by those of the first disease or are entirely absent. Thus, if in typhoid fever the temperature should one day rise to a much greater height than on the previous days, we must not rest satisfied until the cause of this second elevation has been ascertained. Such intercurrent disease adds to the gravity of the prognosis.

A sudden fall in the temperature in typhoid fever may be due either to diarrhœa or hemorrhage into the intestine. If it occurs very suddenly, it is most probably due to the latter. Usually, however, other and equally characteristic symptoms arise to indicate such an occurrence.

Diarrhœa, if often repeated, whether it lower the temperature or not, must be checked, as diarrhœa causes great prostration even in health, and consequently produces still greater weakness, sometimes sufficient to cause death, in persons suffering from typhoid fever. It is best controlled by injections of starch of the consistence of cream, and varying from one to two ounces in quantity; to this some laudanum, or one of the metallic astringents, especially the former, may be added with benefit.

After the temperature has fallen greatly or become normal, should it again begin to rise gradually, we are justified in diagnosing either a relapse or the commencement of an attack of tuberculosis—most probably the former; for relapses more commonly occur after typhoid than any other of the specific fevers. Such relapses are especially apt to occur in certain epidemics. If the temperature indicates a very mild attack, we expect the disease to terminate between the fourteenth and twentieth days. Should it indicate a more severe attack, the disease will probably last from twenty-five to thirty days; in very severe and very rare cases it is said to last even six or eight weeks.—*Lancet, Dec.* 9, 1865, *p.* 647.

3.—THERMOMETRICAL OBSERVATIONS ON THE FEVERS OF CHILDREN.

By G. Stevenson Smith, Esq., Resident Medical Officer, Royal Edinburgh Hospital for Sick Children

[More than a century has elapsed since the value of the thermometer in the diagnosis of disease was first pointed out; but it has not received so much attention as its importance demands. This has probably arisen from the facility with which increase of temperature of the body or a part may be judged of by the hand of the physician—the thermometer, however, corrects and renders exact this more ready method. It will probably prove more useful in fevers and acute inflammations, and in cases of suspected phthisis.]

A knowledge of the state of the temperature is of the utmost consequence in enabling the physician to arrive at a correct diagnosis in many of the affections of children; and it is here, I think, more than in the case of adults, that the medical man will derive the greatest benefit from the use of the thermometer. For how puzzling it often is, in the first stage of a febrile affection in a child, to make out with certainty the real nature of the disease! And how frequently does an illness, which was ushered in by almost all the symptoms of fever—a quick pulse, headache, thirst, loaded tongue, &c.—deceive both the practitioner and the parents, and pass away almost as rapidly as it came! Every one knows the difficulty of making a correct diagnosis in many of the diseases of childhood; and as an evidence of this, patients are frequently sent into the fever wards of an hospital, whose illness turns out to be only trifling, and never goes on to fever at all. Now, it is in such cases that the temperature of the body, as ascertained by the thermometer, affords information which renders mistake impossible.

Before proceeding to report the cases individually, it may be of some interest to make a few remarks regarding the instrument used and the mode of applying it.

The Instrument.—The instrument used in making my observations was Aitken's curved thermometer, made by Casalla of London; and while I have every reason to be satisfied, both with its delicacy and its accuracy, I beg to offer a few suggestions in regard to its construction, which, if carried out, would, in my opinion, be very useful. I have found in using it, especially on a dark day, or in gas-light, that it is extremely difficult to make out with exactness the figures and markings on the glass. It would be an immense improvement were the numbers and degrees tinted with black, or any other color, so that the eye could more readily distinguish the height of the mercurial column. It would also be advantageous, in making observations on young children, to have the curved portion of the instrument made considerably shorter.

How to apply the Thermometer.—In order to obtain the true state of the temperature of the body, the thermometer must be applied to some part which is not exposed to the air; and the best and most convenient place to insert the instrument is in the axilla. With a child in bed this is easily and accurately done by making the patient lie on the back, and then introducing the bulb of the instrument under the anterior margin of the pectoral muscles, so that the mercury is surrounded by the soft parts. A better way than this, however, is to make the child lie on the side, and introduce the bulb into the arm-pit from behind. I have found that in this way the instrument is more firmly and closely embedded in the parts, and the stem, passing up behind the shoulder, is not breathed upon.

The thermometer should be allowed to remain in this position as long as the mercury continues to rise. From three to five minutes is the time usually required. It saves time to heat the bulb a little in the hand before introducing it.

The height of the mercury must be noted while the instrument is *in situ*.

The Normal Standard of the Temperature in Children as compared with that of Adults.—It has been ascertained, by numerous and carefully-conducted experiments made by Davy and others, that in adults the normal temperature of the body is about, 98·4° Fahr. In infants, according to the observations of M. Roger and Dr. Holland, the temperature is somewhat higher, when they are placed in conditions favourable to its sustenance. When examined immediately after birth it is nearly 100°, but it quickly falls to 95·5°, and again rises in the course of twenty-four hours to 97·7° in weakly subjects, and to 99·5° in strong ones. Between four months and six years of age M. Roger found the average temperature to be 98·9°, and between six and fourteen years 99·16° (Carpenter's Physiology). Dr. Bennett again, in his Outlines of Physiology, states that in children the heat of the body is about 2° higher than in adults. As the age of the patients on whom the following observations were made range from three to ten years, I think we shall be pretty nearly correct if we assume the normal standard to be 99°. In disease the temperature has been known to rise as high as 112°, and to sink as low as 67°.

Three Cases illustrative of the Value of the Thermometer as a Diagnostic Agent.—Case 1.—M. M., aged 8, was admitted on 5th January, 1866. She was said to be suffering from fever, her illness having commenced five days previously with rigors and headache. On admission, the tongue was slightly coated, and red at the tip; pulse quick; but there was no rash. The thermometer soon removed all doubt, and enabled us to say with certainty that the child had no fever, and she was consequently sent home.

M. indicates morning, E. evening.

		Pulse.	Resp.	Temp.
Jan. 5.	E.	108	48	99°
6.	M.	108	30	98
	E.	96	30	98
Jan. 7.	M.	84	30	95°
	E.	84	30	95 2-5ths

The gradual fall in the temperature to 95° is remarkable.

Case 2.—R. N., aged 3½, was admitted on January 13, 1866, said to be affected with fever; his illness having been preceded about a week previously by cold shivers, headache, and thirst. When admitted he had a foul tongue, quick pulse, and other symptoms of continued fever; but the temperature

never rose above 98⅘°, and soon went down to 95⅗°, so that it became evident that he had no fever, and he was accordingly dismissed.

		Pulse.	Resp.	Temp.
Jan. 14.	M.	130	32	98 1-5th°
	E.	96	32	95 3-5ths
Jan. 15.	M.	110	32	97°
16.	M.	100	32	96 1-5th

Case 3.—C. F., aged 6, was admitted on January 2, 1866. Ill about a week, the illness having commenced with vomiting, headache, &c., and it was thought that she had contracted fever. On admission, the pulse was quick, tongue foul, but, as will be observed, the temperature never rose above the normal standard, and she was dismissed quite well.

		Pulse.	Resp.	Temp.
Jan. 2.	E.	120	36	98 2-5ths°
3.	M.	96	36	96
	E.	96	36	95 2-5ths

Here again the unusually low temperature of 95⅗ was observed.

Cases of undoubted Typhus, with daily Records of the Temperature, the Respirations, and the Pulse.—In all of the following cases the eruption of typhus was distinct. Most of them were admitted about the end of the first or the beginning of the second week; but it is always a difficult matter to make out with absolute certainty the very day on which the fever begins. We usually count from the day the child took to bed, or from the day it last appeared to be in its usual health.

Case 1.—J. J., aged 8, was first admitted, said to be ill with fever, on December 13, 1865; but he never showed any symptoms of fever, and was sent out apparently quite well in a day or two. Very soon afterwards, on December 26, he was readmitted on the recommendation of a medical man, who thought the boy was suffering from typhus. He had a fresh colour, a moist clean tongue, and appeared to be quite well, so that the nurse imagined that the boy was scheming. The steady rise of the temperature, however, indicated that some fever was impending, and the appearance on the 29th of the typhus rash confirmed the accuracy of the thermometer.

		Pulse.	Resp.	Temp.
Dec. 27.	E.	108	24	99°
28.	M.	108	24	101 2-5ths
	E.	120	32	103 3-5ths
29.	M.	108	36	102 4-5ths
	E.	120	32	103 3.5ths
30.	M.	120	36	102 4-5ths
	E.	128	37	104 1-5th
31.	M.	136	40	104 1-5th
	E.	140	40	103
1866.				
Jan. 1.	M.	132	40	101 3-5ths
	E.	132	36	102 2-5ths
Jan. 2.	M.	136	38	102°
	E.	152	36	102 3-5ths
3.	M.	120	36	100
	E.	115	36	102
4.	M.	120	36	101 1-5th
	E.	108	36	100
5.	M.	108	36	99
	E.	96	36	99 3-5ths
6.	M.	84	32	95 3-5ths
7.	M.	84	32	95
16.	M.	84	32	97 4-5ths
24.	M.	84	30	97

In this case the height of fever was reached on December 31st, when the thermometer stood at 104⅕°; after this defervescence went on gradually, and the fall to the normal standard on January 5th showed that the crisis had arrived. He improved after this date, and was dismissed quite recovered on January 29, 1866.

Case 2.—E. C., aged 9, admitted December 21, 1865. Ill about seven days; typhus rash distinctly visible. The attack was a severe one; there was a good deal of raving for some days, and the boy was deaf and stupid.

		Pulse.	Resp.	Temp.
Dec. 21.	E.	120	28	103 1-5th°
22.	M.	132	34	102 2-5ths
	E.	132	32	103
23.	M.	132	32	102 4-5ths
	E.	132	36	103 2-5ths
Dec. 24.	M.	130	32	103 2-5°
	E.	132	32	103
25.	M.	130	32	10
	E.	144	36	
26.	M.	120	34	

		Pulse.	Resp.	Temp.
Dec. 26.	E.	130	34	101 2-5ths°
27.	M.	120	28	103
	E.	108	36	101 2-5ths
28.	M.	108	32	101
	E.	114	28	100 1-5th
29.	M.	96	30	99 3-5ths
	E.	96	24	99 4-5ths
30.	M.	96	24	98 2-5ths
	E.	100	24	99
Dec. 31.	M.	100	24	97 3-5ths°
	E.	96	20	98
1866.				
Jan. 1.	M.	80	20	96 4-5ths
2.	E.	72	20	96 3-5ths
4.	M.	72	24	97 3-5ths
11.	M.	72	24	97 2-5ths
24.	M.	72	24	97

This patient made an excellent recovery, and was dismissed on January 29, 1866.

Case 3.—W. M., aged 10, admitted 17th January, 1866. Ill five days; typhus rash well marked.

		Pulse.	Resp.	Temp.
Jan. 17.	E.	132	24	103 2-5ths°
18.	M.	120	36	103
	E.	120	24	104
19.	M.	120	36	103
	E.	132	36	103
20.	M.	124	36	102
	E.	124	36	102 4-5ths
21.	M.	120	40	102 2-5ths
	E.	120	34	102 4-5ths
22.	M.	120	36	101 2-5ths
	E.	120	36	103 1-5th
23.	M.	132	36	102
	E.	132	36	102 4-5ths
24.	M.	124	36	102
	E.	120	36	103 3-5ths
Jan. 25.	M.	120	36	100 3-5ths°
	E.	124	36	103
26.	M.	120	30	101 3-5ths
	E.	108	30	101
27.	M.	96	30	99 3-5ths
	E.	108	30	100 3-5ths
28.	M.	100	30	98 4-5ths
	E.	90	30	99 1-5th
29.	M.	84	30	96
	E.	84	30	97 3-5ths
30.	M.	84	24	95 3-5ths
	E.	60	24	97
31.	M.	72	24	97 3-5ths
Feb. 2.	M.	84	24	97 1-5th

In this case there was epistaxis, pain, and gurgling in the belly, and almost constant noisy delirium for three days. On the 28th of January, the temperature fell to 98¼°, and he became much calmer and better. On the 29th, he vomited everything he swallowed, and seemed much worse; but as there was no rise in the temperature, we were assured that nothing serious was going to happen. He made a good recovery.

Case 4.—J. M., aged 6, sister of the above, admitted 20th December, 1865; ill about six days; copious typhus rash on face, hands, and legs.

		Pulse.	Resp.	Temp.
Dec. 20.	E.	136	28	102 1-5ths°
21.	M.	144	30	101 3-5ths
	E.	132	33	103
22.	M.	144	32	101
	E.	144	30	100 4-5ths
23.	M	120	32	101 1-5th
	E.	140	36	103 4-5ths
24.	M.	126	30	101 3-5ths
	E.	140	34	103 3-5ths
25.	M.	120	40	100 4-5ths
	E.	120	36	101
Dec. 26.	M.	120	30	99 2-5ths°
	E.	120	30	100 2-5ths
27.	M.	96	36	98
	E.	96	32	92
28.	M.	100	28	97
	E.	96	28	98 4-5ths
29.	M.	84	20	97 2-5ths
	E.	84	24	97
31.	M.	96	24	96 4-5ths
1866.				
Jan. 1.	M.	96	24	96 3-5ths
4.	M.	96	24	96 4-5ths

It will be seen that in this case the temperature fell on the 14th day of the fever, viz., the 27th December, to 98°. On the same day patient had a long sleep, and when she awoke she was much better. She improved daily, and was dismissed recovered on 20th January, 1866.

Case 5.—J. M., aged 4, admitted 15th January, 1866. Ill five days. Typhus rash came out on 17th January.

		Pulse.	Resp.	Temp.
Jan. 15.	E.	132	46	103 3-5ths°
16.	M.	144	42	103 4-5ths
	E.	138	36	102 1-5th
17.	M.	144	42	102 3-5ths
	E.	156	46	103
18.	M.	156	48	101
	E.	144	48	102 2-5ths
19.	M.	144	48	101 2-5ths
	E.	140	48	101 3-5ths
20.	M.	144	48	101 4-5ths
	E.	144	48	101 3-5ths
21.	M.	132	36	102 2-5ths
	E.	132	36	106 1-5th
Jan. 22.	M.	108	42	98 1-5ths°
	E.	108	36	99 4-5ths
23.	M.	120	36	99 4-5ths
	E.	120	36	100 3-5ths
24.	M.	120	30	99
	E.	108	36	100 2-5ths
25.	M.	120	36	99 3-5ths
	E.	112	36	100
26.	E.	96	36	98 3-5ths
27.	E.	96	36	97 2-5ths
28.	E.	96	36	97 1-5th
30.	E.	80	24	95

Here the temperature fell to 98⅕° on the 22nd, but it afterwards rose, on several occasions, to 100°, indicating that the course of the convalescence was irregular. The child, however, made a good recovery.

Case 6.—J. B., aged 5, admitted 5th January, 1866. Ill about seven days. Typhus rash on face and legs.

		Pulse.	Resp.	Temp.
Jan. 5.	E.	144	36	101°
6.	M.	144	48	101
	E.	156	34	102 2-5ths
7.	M.	132	36	100
	E.	144	42	102
8.	M.	132	42	101
	E.	152	40	103 4-5ths
9.	M.	152	32	102
	E.	152	36	102 3-5ths
10.	M.	144	48	101 4-5ths
	E.	132	36	102
Jan. 11.	M.	144	36	100°
	E.	144	36	102
12.	M.	132	36	100
	E.	144	36	99 1-5th
13.	M.	132	36	98 4-5ths
14.	M.	124	28	97 3-5ths
	E.	120	28	97 4-5ths
15.	M.	120	28	95 2-5ths
16.	E.	108	24	98 1-5th
21.	E.	96	36	95 3-5ths
Feb. 2.	E.	84	24	95 3-5ths

On the evening of the 12th January, it will be seen that, although the pulse numbered 144 beats in the minute, the temperature was down to near the normal standard, 99⅕°. This indicated the turn, for the boy did well ever after, and was dismissed recovered on 5th February. It will be observed that even at night, on two occasions before he left the hospital, the temperature remained so low as 95⅖°.

Case 7.—E. M., aged 7, admitted 20th December, 1865. Ill eight days. Typhus rash distinct on hands, forearms, and thighs.

		Pulse.	Resp.	Temp.
Dec. 20.	E.	152	42	104°
21.	M.	156	41	102 3-5ths
	E.	156	42	103 3-5ths
22.	M.	132	42	101 2-5ths
	E.	144	42	103 1-5th
23.	M.	144	44	102
	E.	156	52	102 4-5ths
24.	M.	144	38	102 2-5ths
	E.	132	42	102 2-5ths
25.	M.	108	40	101
	E.	108	37	100
Dec. 26.	M.	108	40	99°
	E.	108	40	100
27.	M.	108	34	99 2-5ths
	E.	108	36	99 4 5ths
28.	M.	108	34	97 1-5th
	E.	108	32	97 3-5ths
31.	M.	92	32	97 2-5ths
1866.				
Jan. 2.	M.	84	24	98 1-5th
9.	M.	96	24	95 1-5th

Here again the temperature which, up to the 26th December, the fourteenth day of the fever, had always been above 100°. fell on that day to 99°, and though the pulse kept up for a day or two longer, still the boy improved from that date. He left the hospital quite well on the 15th January, 1866.

Case 8.—E. C., aged 8½, admitted 21st December, 1865. Ill about eight days. Copious typhus eruption on face, hands, trunk, and legs.

		Pulse	Resp.	Temp.
Dec. 21.	E.	132	28	103 3-5ths°
22.	M.	132	28	101 4-5ths
	E.	144	28	103 3-5ths
23.	M.	144	30	101
	E.	144	30	102 4-5ths
24.	M.	132	28	101 2-5ths
	E.	132	30	102 4-5ths
25.	M.	120	24	98 3-5ths
	E.	120	36	101 4-5ths
26.	M.	96	36	96 4-5ths
	E.	96	36	98
Dec. 27.	M.	96	34	97 2-5ths°
	E.	84	36	96 2-5ths
28.	M.	84	36	95 4-5ths
	E.	84	32	96 2-5ths
30.	M.	84	30	95 4-5ths
1866.				
Jan. 2.	M.	72	28	96 3-5ths
3.	M.	72	28	95
6.	M.	72	28	97

On the 26th of December, the fourteenth day of the fever, the temperature fell suddenly from 101⅘°, at which it stood on the previous evening to 96⅘°, no less than five degrees; thus indicating that the crisis had arrived. After this date the child improved every day, and went home quite well on the 11th January, 1866.

Case 9.—M. B., aged 8, admitted 26th December, 1865. Had been ill about eight days. This was also a case of true typhus.

		Pulse.	Resp.	Temp.
Dec. 26.	E.	120	42	103 3-5ths°
27.	M.	120	42	101 2-5ths
	E.	108	30	103
28.	M.	108	40	101
	E.	100	24	102
29.	M.	108	36	100
	E.	96	36	100
30.	M.	96	30	97 3-5ths
	E.	110	28	98 2-5ths
Dec. 31.	M.	104	24	97 4-5ths°
	E.	104	28	98
1866.				
Jan. 3.	M.	88	24	95 3-5ths
6.	M.	84	24	97 2-5ths
16.	M.	84	24	96
24.	M.	84	24	98

It is rather strange that in this case the fall of the pulse preceded that of the temperature; for while on the evening of the 29th December the pulse was 96, the heat was 100°. The boy did well, and was dismissed quite recovered on the 29th January, 1866.

Case 10.—E. M., aged 4, admitted 25th January, 1866. Ill about a week. Typhus rash on face, hands, and body. Patient was very ill, restless, delirious, and very deaf for some days.

		Pulse.	Resp.	Temp.
Jan. 25.	E.	144	40	104°
26.	M.	132	40	102 2-5ths
	E.	144	40	102 2-5ths
27.	M.	144	42	102 2-5ths
	E.	142	40	102 2-5ths
28.	M.	144	44	102 1-5th
	E.	150	44	102 3-5ths
29.	M.	132	44	101 3-5ths
	E.	136	40	102 2-5ths
Jan. 30.	M.	132	44	100 2-5ths°
	E.	132	44	100 4-5ths
31.	M.	108	40	100
	E.	132	40	101 1-5th
Feb. 1.	M.	108	40	99 1-5th
	E.	96	38	98 4-5ths
3.	M.	100	36	97 3-5ths
4.	M.	96	36	97

This girl improved immediately after the temperature went down to 99⅕°, and made an excellent recovery.

These are all the cases of typhus fever on which thermometric observations have yet been concluded, and they may serve to show the general range of temperature of that disease in young persons.

Of late, typhoid fever has not been very prevalent amongst children; at all events, comparatively few cases have been admitted to the hospital. I give the temperature, etc., of one case which proved fatal about the twelfth day of the fever.

Temperature, Respiration, and Pulse, in a Fatal Case of Typhoid Fever.—M. S., aged 4; admitted 16th January, 1866. Ill six days.

		Pulse.	Resp.	Temp.
Jan. 16.	E.	132	36	104 2-5ths°
17.	M.	132	36	103 1-5th
	E.	120	36	104
18.	M.	132	40	102
	E.	132	40	103
Jan. 19.	M.	138	46	103 2-5ths°
	E.	144	48	103 4-5ths
20.	M.	144	40	101 1-5th
	E.	144	46	103 2-5ths
21.	M.	168	46	104 1-5th
	5 P. M.	168	46	104 2-5ths

Child died at 7 P. M., of the 21st.

It is of interest to observe that before death there was a considerable rise in the temperature. It would have been very difficult to have diagnosed this to be typhoid fever by the eruption alone, for it was very scanty, and by no means characteristic. But there were the pea-soup evacuations, and a tympanitic distention of the belly; and the post-mortem examination revealed changes in the mesenteric and Peyerian glands. There was likewise effusion into the chest.

The next case shows the range of temperature in a scarlet fever patient, in whom the eruption was copious, and the tonsils ulcerated. The rash first appeared on the 22nd.

Case of Scarlatina, with Daily Records of the Temperature, Pulse, and Respiration.

		Pulse.	Resp.	Temp.
Jan. 22.	E.	150	36	104°
23.	M.	144	36	103
	E.	144	36	103 3-5ths
24.	M.	144	36	102 2-5ths
	E.	144	40	103 1-5th
25.	M.	120	36	100
	E.	140	36	102
26.	M.	120	36	99
	E.	120	30	99 1-5th
27.	M.	96	30	97 3-5ths
	E.	108	30	97 3-5ths
Jan. 28.	M.	92	30	97°
	E.	108	30	98 2-5ths
29.	M.	96	30	97 4-5ths
	E.	90	30	98 2-5ths
30.	M.	96	36	97
	E.	108	36	98 4-5ths
31.	M.	96	24	98 1-5th
	E.	90	24	98
Feb. 1.	M.	96	24	98 2-5ths
3.	M.	84	24	98 4-5ths

Desquamation began on the seventh day from the appearance of the eruption. There was no renal complication, and patient made a good recovery.

Of course, these cases, the particulars of which I have here recorded, are far too few to enable me to make any correct generalization as to the range of temperature in the fevers alluded to. Nor do I intend to attempt to draw partial or premature conclusions; but convinced that the "foundation of all knowledge must be a careful and extensive acquisition of facts," I offer the results of my observations as a contribution to the thermometry of disease in children.

It is of great interest to remark that in *all the ten typhus cases the temperature fell considerably below the normal standard during convalescence;* and in seven of them it was so low as to be under 96° Fahr.

The greatest heat observed was in Case 1, where, on two occasions the thermometer stood at 104 1-5th°.

In the typhoid case the greatest heat was 104 2-5ths°, while in the scarlatina patient the maximum was 104°.

The lowest temperature occurred in the typhus cases 1, 5, and 8, during convalescence, when the heat of the body was only 95z.

There is just one other point I would beg to direct attention to, as it is of importance in connection with the diagnosis of typhus from typhoid fever. In Dr. Aitken's recent work he thus alludes to the different modes of defervescence in the two diseases: "The difference between the two (typhus and typhoid) is rendered still more striking by their respective modes of defervescence. In typhus the defervescence shows no gradual remissions; on the contrary, it is invariably sudden." And in his typical ranges of temperature,

he represents the fall in typhus to be as much as five degrees, when the crisis occurs.

Now, in only one of my cases (No. 8.) was the defervescence either so sudden or so great as this. In it the temperature, which, on the evening of December 25, was 101 4-5ths, fell on the following morning to 96 4-5ths—a fall of five degrees. But in most of the other cases the decline of the temperature to the normal standard or under it usually took place gradually, the different steps downwards scarcely ever measuring more than one or two degrees.—*Edinburgh Medical Journal, March*, 1866, *p.* 823.

4.—ON THE USE OF THE THERMOMETER AS A GUIDE IN THE DIAGNOSIS OF PYREXIAL DISEASES.

By Dr. F. W. Gibson, B.A., late House-Surgeon of the Taunton and Somerset Hospital.

Fluctuations of Temperature within the Limits of Health.—The normal temperature in the axilla, the only locality in which the temperature is now habitually taken, is 98·5 deg.; but in certain circumstances it may vary a degree or two above or below this point. These variations are, however, not persistent. In a few hours—at most, twenty-four—the temperature becomes normal. The important deduction from this fact is, that a *persistent* rise above 99·5 deg., or depression below 97·3 deg., is a sure sign of disease.

Exposure of the body to the air, even in warm weather, lowers the temperature considerably. I have frequently observed a depression of the temperature, continuing for some hours, after slight operations in which there was no notable shock or hemorrhage; *e. g.*, a boy, aged 11, admitted for phimosis and gonorrhœa, had a temperature of 100·4 deg. immediately *before* the performance of the operation of circumcision. For two hours *after* the operation, the temperature remained below the normal height, although he was in bed, carefully covered up.

The temperature is lowered after a full meal and the use of alcohol. It rises as digestion advances. Ingestion of improper articles of diet in typhoid (and other fevers?) may cause a remarkable rise in the temperature, as is shown by the following case: A man, aged 21, in the third week of typhoid fever, becoming convalescent, with a temperature nearly normal, ate, at two in the afternoon, a considerable quantity of heavy currant-cake. At eight in the evening, his temperature had risen to 105·6 deg.

The temperature is lowered by sleep.

Correlation of the Pulse and Temperature. The rule "that an increase of temperature of one degree above 98° corresponds with an increase of ten beats of the pulse per minute," though true in the main, is, as shown by the following cases, subject to many exceptions. In typhoid fever, I have known a pulse of 100 to exist with temperature of 105·6 deg.; in jaundice, a pulse of 58 with a temperature of 103°; in rheumatic fever, a pulse of 80 with a temperature of 103·5 deg.; in peritonitis, a pulse of 80 with a temperature of 102·2 deg.; in tetanus, a pulse of 72 with a temperature of 104 deg,; in shock following severe surgical injury, a pulse of 120 with a temperature of 98·5 deg.; in typhoid fever, a pulse of 120 with a temperature of 100·4 deg.

The temperature, is, therefore, a much more certain guide than the pulse, since it is not liable to be affected by nervous debility or excitement. The temperature is, in fact, the only certain means of diagnosis between the frequency of the pulse produced by weakness, and that caused by fever; and in those cases, also, in which extreme slowness of the pulse exists as an idiosyncrasy, the employment of the thermometer leads to the discovery of disease which would not otherwise be detected.

The objective temperature of the skin—that is, the sensation conveyed to the applied hand of the observer—is a most fallacious guide. I have known the skin to be objectively of normal warmth with the temperature of 105 deg., and cool with a temperature of 102 deg. The use of the thermometer is,

therefore, the only reliable method of ascertaining the actual temperature of the body.

As the thermometer has been but little employed hitherto in the diagnosis of pyrexial diseases, a few remarks as to the construction of the instrument, and the proper method of applying it, are necessary before I proceed farther.

The instruments with which I have made most of my observations, I obtained from Mr. Griffin, of Garrick Street, Covent Garden. The scale ranges from 55 to 130 deg., each degree being divided into fifths. Some are curved near the bulb, so as to fit more easily into the armpit; but the advantage thence derived is not great; and I find that the instruments thus made are rather more apt to slip down out of the axilla than those which are straight. The accuracy of the thermometers must be thoroughly established before they are employed, by comparing them with a standard instrument. Dr. Aitkin has arranged a maximum self-registering instrument, which does not require to be read *in situ*, but may be removed and inspected when convenient. Mr. Casella, of Hatton Garden, supplies this instrument in a box, with a second ordinary thermometer having a bent bulb. Negretti and Zambra also manufacture most excellent instruments. A thermometer, sufficiently acurate for all medical purposes, may be obtained for a moderate sum (seven shillings) of the first named maker (Griffin).

The patient must have been in bed well covered up, an hour at least before the temperature is taken. If he have been lying on his side, he must be turned round, in order that the thermometer may be applied in the axilla which has been next the bed. He must be placed in a medium position between the side and back; the bulb of the thermometer passed well up into the top of the axilla; the arm laid across the chest; and especial care taken that the instrument be thoroughly surrounded by the soft parts, that no clothes intervene, and that the patient keep his arm in place.

If an approximately accurate observation be all that is desired, the thermometer need not be retained *in situ* more than five minutes; but it is much better to allow it to remain for ten minutes. The plan which I usually adopt is to note the height to which the mercury has risen at the end of five minutes, and then wait for a minute or so; when, if I find that a rise has taken place, I allow the instrument to remain in three or four minutes longer. The rapidity of the rise of the mercury, the pulse, respirations, and state of the skin, should be simultaneously observed.

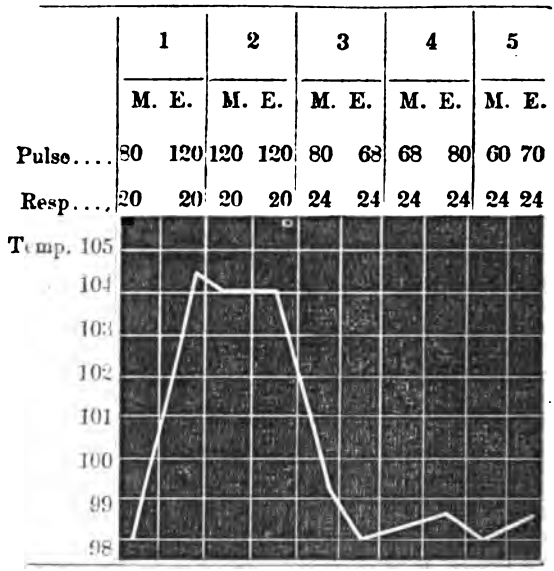
The daily thermometric changes, with the variations of the pulse and temperature, may be conveniently recorded by means of a diagram such as that given on the next page.

Two daily observations, sufficient in the majority of cases, should be made, as nearly as possible at the same time each day, between the hours of seven and nine in the morning, and five and seven in the evening. If more frequent observations be deemed desirable, midnight and noon are considered the most advantageous hours.

Range of Temperature in Disease.—The height of the temperature is usually a sure indication of the severity of the fever. If, in a case of typhoid, the temperature in the evening during the second week do not rise above 104 deg., the disease may be expected to run a mild course; whereas, if it amount to 105 deg. in the evening or 104 deg. in the morning, the patient is in considerable danger. In traumatic erysipelas, however, the temperature frequently rises with extreme rapidity to a very high degree, without the supervention of any alarming symptoms. For example, a man aged 60, under treatment for ulcer of the leg, was infected with erysipelas by a patient in the same ward. The attack began at eight o'clock in the morning with a fit of severe rigors; at nine, his temperature was 104 deg.; it remained high till the morning of the third day, when it had fallen to the normal standard. The disease was superficial, and did not spread above the groin. At no time were there any alarming symptoms. Again: a man aged 22, an epileptic, who had two or three small ulcers on his head, produced by tartar emetic ointment, contracted erysipelas. The attack was slight; no danger threatened; neverthe-

less, his temperature rose to 104·6 deg. In none of these cases, however, was the high temperature of long continuance.

[DIAGRAM.] *Traumatic Erysipelas.*



When, however, the temperature in any febrile disease approaches 106 deg., the patient is in imminent danger. "A temperature of 108 and 109 deg. indicates most surely the near approach of death." (Wunderlich, who records a case of idiopathic tetanus in which, at the moment of death, the mercury stood at 112·5 deg.; after death, it rose to 113·8 deg.)

The highest temperature which I have observed occurred in a case of pyæmia, in a woman aged 40, who had her forearm amputated for cancerous disease of the wrist. At two in the afternoon of the ninth day after the operation, she had a severe attack of rigors. At half-past three, a temperature of 103·6 deg. was noted. It rose *continuously*—a sign of evil augury in very many cases of pyrexia—until death, which took place thirty-eight hours after the attack of rigors. An hour before death, it had reached 107 deg.

In pneumonia, the height of the temperature gives a much surer indication of the presence and amount of pyrexia than either the rate of the pulse and respirations, or the physical signs. The latter continues in *statu quo*, in many cases, long after the temperature has fallen to the normal standard. The physician, therefore, who judges from these symptoms alone, will be in danger of falling into the error of unnecessary prolonging antiphlogistic treatment. On the other hand, the temperature is in some cases very high whilst the physical signs afford but slight evidence of the amount of impending disease.

For example, a man, aged 45, with signs of pneumonic consolidation extending as far upwards posteriorly as the sixth rib, had a respiration rate of 32 per minute, yet his temperature was perfectly normal. A woman, aged 19, having similar signs, with pulse 100, and respiration 32, had a perfectly normal temperature. A boy, aged 14, in whom the physical signs consisted merely of slight dulness with feeble bronchial respiration at the base, was found to have a temperature of 107 deg. How, in the first named cases, could the absence of pyrexia have been diagnosed—how, in the last, could the impending severity of the attack (for severe it became) have been prognosticated—without the aid of the thermometer?

Dr. Ringer (*Temperature of the Body in Phthisis and Tuberculosis*) has proved that there is a continuous elevation of temperature in all cases in which a deposit of tubercle is taking place; that the temperature is a more accurate indicator of the amount of disease than either the physical signs or symptoms; that by means of the temperature, we can diagnose phthisis long before any physical signs or symptoms can be detected; that, by means of the thermometer, we can decide when the deposit of tubercule has ceased; and that, by means of that instrument, we can diagnose phthisis from diseases which more closely resemble it—for example, carcinoma of the lung, aortic aneurism, and dilated bronchi. By means of the thermometer, we can diagnose tuberoular meningitis from cerebral tumour.

An abscess lying in front of a large artery sometimes so closely resembles aneurism that the diagnosis is excessively difficult. In every case of abscess which I have observed, the temperature has been above the normal standard—often considerably. An abnormally high temperature has, in not a few cases, led me to the diagnosis of impending abscess for some hours before fluctuation could be detected. The temperature begins to rise at the commencement of the process of suppuration. It rises rapidly until the process is mature, when it begins to fall.

For example, a man, aged 39, was admitted for a severe bruise of his thigh. On the evening of the third day after the accident, his temperature was 103 deg. Hardness and intumescence of the soft parts was to be felt near the seat of the injury, but no fluctuation. A poultice was applied. The next evening his temperature was 105 deg. The abscess became matured during the night. The temperature fell gradually next day.

In some cases of severe concussion of the brain, it is of extreme importance to establish with certainty the existence or non-existence of any inflammatory symptoms. Mere nervous shock will in not a few such cases cause the pulse and respirations to be very rapid. The following is a case in point: A boy, aged 14, admitted with severe cerebral concussion, had a pulse of 120 and respirations 36. The temperature, however, did not rise above 101 deg., which proved the absence of any severe inflammatory mischief.

In cases of intestinal hemorrhage in typhoid fever, the temperature falls for some hours before the blood appears in the stools, the pulse continuing as rapid, or even more rapid than before the hemorrhage took place. The thermometer enables the physician to determine the occurrence of the hemorrhage, and to adopt preventive treatment at once; whereas if, neglecting the use of that instrument, he wait until the blood appears in the motions, much valuable time will have been lost.

A knowledge of the range of temperature peculiar to each disease enables the observer, when he perceives any marked departure from the type, to pronounce either that his diagnosis is wrong, and that the disease is not what it appeared to be, or that some secondary lesion has supervened.

For example, the typical range or temperature in typhoid (Peyerian) fever, as established by Wunderlich, is as follows:—

	Morning.	Evening.
First day.....	98·5 deg.	100·5 deg.
Second day.....	99·5	101·5
Third day.....	100·5	102·5
Fourth day.....	101·5	104
Second half of week.....	102	104

At the commencement of the second week, in mild cases, the temperature begins to decrease; for, although the evening temperature may be 104 deg., the morning is only 102 deg. In severe cases, on the other hand, the morning temperature is above 103 deg.; the evening above 104·5 deg., and may reach 106 deg. In the third week, remarkable vacillations of four to six degrees between the morning and evening temperature occur. The fever terminates by a gradual defervescence (*lysis*), not by a sudden *crisis*, as in typhus. In mild cases, the fever lasts from three to four weeks; in severe, from four and a half to ten weeks.

Typhoid fever is not present, if on the evening of the first or second day of the disease, the temperature be 104 deg.; if the evening temperature do not rise to 103·3 deg. between the fourth and sixth days; if, in the second half of the first week, the evening temperature decrease considerably; lastly, if between the eighth and eleventh days, the temperature be below 103 deg.

As an illustration of the truth of the first proposition, I cite the succeeding case. A woman, in a ward of the hospital in which there were two or three cases of typhoid fever, had one evening an attack of rigors. Her temperature was 102 deg.; on the morning of the next day, 103 deg.; in the evening 105 deg. The symptoms in all other respects resembled those of incipient typhoid. The woman was much alarmed, for a patient had just died of typhoid fever. The existence of a temperature of 105 deg. in the evening of the second day enabled her to be assured that she was not threatened with that disease. This prognosis was proved to be correct by the symptoms developing shortly into those of erysipelas.

The next case is an example of the value of the second and third propositions A girl, aged 21, was admitted on January 15th, 1866, with symptoms of impending typhoid. The attack began with rigors on the evening of the 11th. She had great abdominal tenderness, a hot dry skin, pains in the limbs, muscular tremors, and one or two suspicious specks on the skin of the chest. Nevertheless, because the temperature did not on any evening rise to 103 deg., and a marked decrease of temperature occurred on the evening of the 18th, the disease was pronounced not to be typhoid. On the 20th, she was quite well.

The next case illustrates the last proposition. A man, aged 40, living in a locality in which typhoid had lately been rife, who had had rigors, followed by diarrhœa and pains in the limbs, a week previous to his admission, was found to have a typhoid tongue, frontal headache, diarrhœa, and numerous specks on his abdomen and chest, very closely resembling the eruption peculiar to typhoid fever. On the tenth evening of the disease, his temperature was 101·8 deg. This temperature forbad the diagnosis of typhoid. In three days after, the man was well.—*British Med. Journal, March* 10, *and* 17, 1866, *pp.* 249, 278.

5.—OBSERVATIONS ON THE PRESENT EPIDEMIC OF TYPHUS.

By Dr. Robert Perry, Physician to the Royal Infirmary, Glasgow.

[The cases mentioned by Dr. Perry were all characterised by distinct and decided febrile symptoms, and were accompanied by decided mulberry rash. Dr. Perry holds strongly to the opinion that the two forms of fever are quite distinct.]

With no desire to detract from the accuracy of the observations of the late Dr. Joseph Bell, one of my predecessors in the Glasgow Royal Infirmary, or from the subtlety of reasoning displayed in that admirable series of papers published in the Glasgow Medical Journal in 1860 and 1861, in which he strives to prove the identity of typhus and typhoid fevers, I must here remark that I have not been able to find in the intestines of patients who have died of undoubted typhus, those pathological conditions of Peyer's glands which he states to be present and identical in both forms of fever. It is true that there is frequently found in typhus a vascularity or congestion of the mucous membrane of the small intestines, and in a somewhat smaller proportion the mucous membrane of the large intestines is also found in a similar condition. We need not be surprised at this state of the intestinal mucous membrane, nor of the turgid condition of the glands at times seen, as hyperæmia of the various organs seems to be the principal pathological condition of typhus, and there would be matter for surprise were the glands never found in such a state.

When the two kinds of fever have been running their course in the same patient at once, then the lesion of Peyer's glands, peculiar to typhoid fever, might naturally be expected.

Although in the present epidemic neither Peyer's glands nor the solitary glands of the small intestines exhibit after death from typhus those lesions, which are now considered as pathognomonic of typhoid fever, I do not consider, on that account, we are at liberty to call in question the accuracy of those writers who have recorded the fact of similar lesions occurring in former epidemics of typhus.

Fifty-two cases were inspected by Dr. A. Anderson in the Glasgow Royal Infirmary in 1837-38, and in nearly all of them he found congestion and enlargement of Peyer's and the solitary glands, with five cases of ulceration in Peyer's glands.

I have seen ulceration of Peyer's patches in one case only out of thirty inspections. In one other case I found all the solitary glands of the small intestines enlarged and infiltrated with a soft yellowish white deposit, which, upon examination under the microscope, was seen to be of homogeneous character, without any trace of structure or cell development. From the fact of the enlargement or ulceration of Peyer's glands being rarely seen now in typhus, I think we may conclude that the type of the disease is not at all times the same. Adopting the views of pyrexia so ably enunciated by Dr. Parkes, of University College, we may suppose that at times the products of increased metamorphosis of the tissues are retained in the blood, and in the cases where congestion and ulceration of the intestines occur, such retained products are thrown upon the mucous membrane of the small intestines, thus producing a local inflammation there.

In the present epidemic, upon the same theory we may say that the retained products of tissue metamorphosis were thrown upon the pulmonary tissue, as congestive pneumonia proved to be the most frequent and most fatal complication. In the same way I might explain the frequent occurrence of congestion and ulceration of the urinary bladder, observed by myself in 1850 in the Glasgow Royal Infirmary, while acting as assistant to the late Dr. Robert Macgregor. I related to the Pathological Society the history of twenty-five cases, and exhibited some of the recent specimens, and some casts of the pathological appearances at that time so frequently found in the bladder. The cases were afterwards published by Dr. Macgregor in the London Medical Gazette, New Series, vol. i., 1851. Some of the preparations may still be seen in the pathological museum of the hospital.

With respect to the treatment of typhus fever, it is a trite remark, that after it is once fairly established no man can cure it. All we have to do is to guide our patient through it, treating symptoms and complications as they occur. During the earlier part of this epidemic, from the want of adequate hospital accommodation in the city, the managers of the Royal Infirmary showed an anxiety to provide room for as many patients as possible, and the consequence was that, until the city authorities erected a temporary hospital, the wards of the Royal Infirmary were very much overcrowded.

At present when all the beds in a ward are occupied, an average of 926 cubic feet of space is allowed to each patient. Before the erection of the temporary fever hospital, and when the wards of the Royal Infirmary were crowded, there was only a space for each of about 700 cubic feet; and in some of the temporary wards then opened, the cubic space was very much less than this.

In those smaller and temporary wards the mortality was much higher than in the regular and more spacious ones. It is proper, however, to mention that the higher rate of mortality in the smaller wards was not altogether attributable to the smaller amount of space, but in a great measure to the fact of many of the most severe cases being sent into them, as those wards were nearest to the reception room; and patients who were in a state of great prostration, or occasionally moribund on admission, were thus saved from the extra fatigue of being carried up stairs.

Dr. Murchison, in his treatise on Continued Fever (p. 247), states that "500 cubic feet of space, with 2 cubic feet of ventilation per minute, constitute the smallest amount that can be safely allotted to each person. In workhouses the amount of space erected by the Poor Law Board is 300 cubic feet

for a sick ward, or for a dormitory occupied by night only; and 500 cubic feet in a ward occupied day and night." And again: "In hospitals where typhus patients are admitted, there ought to be an allowance of at least 1500 cubic feet to each bed." Although under the present arrangement there is nominally only an average of 926 cubic feet of space for each fever patient in the Glasgow Royal Infirmary, there is in reality a considerably greater space, as it is not a very common occurrence for all the beds to be occupied at one time in a ward, and there is now no reasonable ground of complaint respecting the accommodation.

In a large majority of the cases, the treatment adopted was of the simplest description, and may be said, in a few words, to consist of a supporting diet, with good nursing, and constant and careful watching against the superventionof any complications or untoward symptoms—meeting such complications or symptoms, as they arose, with the treatment deemed most suitable to each individual case.

In order to give a fair trial to the acid treatment, so highly recommended by Dr. Murchison and others, I prescribed the nitro-muriatic acid in a considerable number of cases. The theory upon which the use of the mineral acids has been so strongly advocated, appears to be beautifully simple, viz., to neutralize the poison of typhus, which is supposed to be present in the blood as a peculiar compound of ammonia; since not only is there an increased quantity of ammonia exhaled by the lungs and the skin, but also the urine and the discharges from the bowels become highly ammoniated.

In the cases so treated, I must confess that I have not met with that very marked amelioration of the disease which many of the writers on fever have ascribed to the administration of mineral acids; and in not a few instances, when given in the proportions prescribed by Dr. Murchison—viz., 20 minims of dilute hydrochloric acid, and 10 minims of dilute nitric acid, every third hour—I have frequently been obliged to discontinue their administration on account of severe griping and diarrhœa.

When the acids are cautiously administered in rather smaller doses, in conjunction with a few minims of solution of muriate of morphia, if the bowels be irritable, sweetened with syrup of orange peel, and diluted with water, the draught so formed is generally relished by the patient, and often produces a noticeable effect upon the fever—the most marked improvement being seen upon the tongue, which becomes moist and clean, from being very dry and hard, and covered with a brown fur.

In a few cases I gave small doses of permanganate of potash, in order to ascertain if it exercised any antiseptic influence upon the typhus poison. In two of the cases with a very dark petechial rash, it seemed to hasten the disappearance of the eruption. The tongue also began to get clean and moist at an early stage; but as it produced no apparent diminution of the temperature nor of the pulse, and did not shorten the duration of the fever, and as no good results were observed to arise from its administration in the remainder of the cases, I was not encouraged to give it any further trial.

In No. 51 of the Glasgow Medical Journal, October, 1865, there is a communication by Dr. De 'Ricci, of Dublin, in which he advocates the use of sulphites and hyposulphites in the treatment of zymotic diseases. "Physiology," he says, "leads to believe that all zymotic diseases depend on a fermenting or catalytic principle in the blood. Chemistry teaches us that in the presence of sulphurous acid and the sulphites no catalytic action can take place, and practical experience confirms the teachings of science by showing us palpably that zymotic diseases, even in their most virulent forms, become completely neutralized by this remedy." This discovery he ascribes to Professor Giovanni Polli, of Milan. I may here remark, that it is much to be regretted that medical men are so frequently in the habit of making such unqualified assertions respecting the use of various remedies. One might suppose that, with such a remedy in hand, the treatment of fever would become a very simple matter indeed—neutralize the poison, and, as a natural consequence, the fever must cease.

Being satisfied that the bisulphite of soda—the preparation chiefly recom-

mended—is, at all events, innocuous, I determined to test its efficacy in typhus. After carefully watching the progress of at least a dozen cases in which scruple doses of the bisulphate of soda were administered every three or four hours, I am led to the conclusion, that in typhus fever, at least, no amelioration of the disease is produced by it; as to its action in scarlatina, small-pox, and puerperal fevers, for which it is so highly lauded in the paper just referred to, I am not prepared to speak, having had no experience of it in such cases. Had I been so rash as to have given an opinion upon the use of the sulphites without giving them a full trial, I might have fallen into the mistake of supposing them capable of completely neutralizing the poison of typhus, because, in one of the first patients who got this treatment, there was a very remarkable fall of the pulse, accompanied with an unusual defervescence or diminution of temperature on the 8th day of the fever—the pulse coming down from 120 on the 7th day to 96 on the 8th, and the temperature at the same time falling from 102.8° to 101°. On the 10th day both pulse and temperature were normal, the former being 74, and the latter 98.2o. Although, on admission, this appeared to be a very severe case of typhus, with dark and copious eruption, still I am not inclined to attribute the unusually early termination to the effects of the bisulphite of soda, but rather to a peculiarity in the constitution of the patient, as I found that after his complete recovery he had a remarkably slow and calm pulse. I was the more confirmed in this view from the fact, that in the adjoining bed to this case was a man whom I had dismissed about three weeks before, after recovery from pneumonia. During the time he was under treatment for pneumonia, it was observed that he had a very slow pulse, which never rose on any day above 72, whilst the temperature was 100°. He returned with a distinct typhus rash, and under symptoms of pyrexia; and although he had nothing but dietetic treatment, his pulse never rose above 90, and on the 11th day of the fever was only 72. It afterwards fell as low as 48, but the patient by this time felt so well as to wish to be dismissed. In none of the other cases did I observe the smallest degree of benefit from the use of the bisulphite of soda; all I can say is, that while it does not appear in the slightest degree hurtful, it is by no means an agreeable medicine to the poor patient.

The only other point, with respect to the treatment of typhus, upon which I wish to make a few remarks, is the administration of alcoholic stimulants, a subject which has of late been exciting a considerable degree of interest amongst the profession, chiefly owing to the "Facts and conclusions as to the use of Alcoholic Stimulants in Typhus Fever," embodied in a paper read to the Medical Society of Glasgow, 5th January, 1864, by Dr. Gairdner, and to some remarks upon the same subject, and by the same author, published in the Lancet, January 21, 1865. Several other communications from Ireland and elsewhere appeared about the same time in the public medical journals, in which the writers described the successful treatment of such and such a number of typhus cases without stimulants, indicating that such a line of treatment must be considered rare or novel.

I have never been in the habit of administering alcoholic stimulants in typhus in the nature of a routine, nor have I been at all guided in my use of them by the age of the patient; but in every instance I judge each individual case according to its own requirements, administering or withholding wine or spirits, quite irrespective of the age of the patient, as the particular case requires the stimulant or does not. Practically, however, the conclusions at which I have arrived from a retrospective review of my cases, are almost identical with those so forcibly and clearly enunciated by Dr. Gairdner.

Owing to the circumstance of a journal of one of the wards having lately gone amissing, which contains the details of a few of the cases, it is not in my power, without an amount of research for which I have not leisure at present, to classify the whole of my cases according as they have been treated with or without alcoholic stimulants. I have, however, ascertained that 534 cases were treated with wine or spirits during some part of their illness, and out of this number 138 died. On the other hand, 491 were treated without any alcoholic stimulants, with only 9 deaths.

From the much higher rate of mortality amongst the former than amongst the latter, I would caution any one against drawing the conclusion that the greater relative number of deaths was at all influenced by the administration of the stimulants. All the inference that I think may be deduced from those numbers is, that a large proportion, say fully one-half, of the cases of typhus in the present epidemic, may be advantageously treated without any alcoholic stimulants.

Doubtless a considerable number of those who got wine or spirits would have recovered without a drop of either; but I am satisfied that, in many of them, the risk of a fatal issue would have been much increased, and the patients would have had a much more tedious convalescence.

Of 245 patients below 15 years of age, only 29 had any alcoholic stimulants; and several even of that small number had only very small quantities, and for only a few days.

The state of the cardiac and radial pulses, as pointed out by Dr. Stokes, is the best guide for the administration of alcohol; and, as I before mentioned, the time for giving and the quantity required must be regulated by it in each individual case.—*Glasgow Med. Journal, Jan.*, 1866, *p.* 407.

6.—RESEARCHES ON THE DAILY EXCRETION OF UREA IN TYPHUS FEVER, WITH REMARKS.

By Dr. Keith Anderson, Resident Physician, Royal Infirmary, Edinburgh; President of the Royal Medical Society.

[The importance attaching to the amount of urea formed and excreted in fevers arises from the prevalent doctrine that fever essentially consists in an increased metamorphosis of tissue, and also from the question how far the comatose or head symptoms of fever are due to uræmia. The only analysis of the whole urine in cases of typhus, are those of one case by Dr. Parkes, and, since the author's own observations were made, of two by Mr. Dunnett Spanton. Dr. Keith Anderson has lately investigated the question at the Royal Infirmary of Edinburgh.]

The general results of the inquiry have been that, *in all of the cases the quantity of urea excreted daily, during the second week, was decidedly below the standard of health*, notwithstanding that the patients were in a state of high fever, with the temperature and the pulse much above the normal rate. These results differ markedly from those of Dr. Parkes' analyses, and are opposed to the belief which many entertain of an increased excretion of urea in typhus. With regard to the daily excretion, during the first week, nothing definite could be made out, as the patients entered hospital at too late a period of the fever to admit of a sufficient number of observations. In three of the cases, however, viz., Cases 1, 4, and 6, the excretion of urea was found to be comparatively high at the end of the first week, but not above the normal amount.

The cases may be divided into two classes: one, in which the crisis occurred on the fourteenth day, while the nervous symptoms continued to some extent for a day or two longer; the other, in which the crisis took place by a process of gradual improvement, beginning about the tenth or eleventh day, and completed about the fourteenth. In the *first* class, an increased discharge of urea took place at, and immediately after, the crisis. Subsequently to the occurrence of this increased discharge, the daily excretion of urea fell to a very low amount, and then rose gradually as the patient improved in health. In the *second* class, the nervous symptoms abated about the tenth or eleventh day, after an increase in the excretion of urea. The completion of the crisis on the fourteenth day was followed by a fall in the amount of urea excreted daily, which then gradually rose with the return of health, as in the other class.

In his Gulstonian Lectures on Pyrexia, Dr. Parkes has shown that the febrile condition is attended by increased metamorphosis of tissue, and in consequence, by an augmented formation of urea. It also appears from his obser-

vations that this increased formation of urea is not necessarily followed by an increase in the quantity eliminated, but that, in certain circumstances, a considerable amount of urea may be retained in the system. In my cases, such a retention seems to have taken place, since an increase of urea was observed, as before mentioned, either on the occurrence of crisis, or on the appearance of the first symptoms of improvement. That this increased excretion was due to a previous retention, and not to more active tissue-metamorphosis, consequent on the improvement in the condition of the patient, is rendered probable by the fact that the excretion of urea was greatly lowered after the sixteenth or seventeenth day, and rose in amount exactly in proportion to the rapidity with which the patients regained health and strength; and also by the fact of the excretion having been very low for some time previous to its increase.

It is generally admitted that in Bright's disease and cholera, and in many other affections, stupor, delirium, and other nervous symptoms of a low or so-called "typhoid" character, are due to a retention of urea. In regard to the head symptoms of typhus, a similar explanation has been proposed. In my cases, the fact of those symptoms being most intense while the urea-excretion was lowest, and of their improvement coinciding with, or following on, an increase in that excretion, in which the increased elimination took place on the days immediately following the fourteenth, the nervous symptoms continued to a considerable extent during those days, although the fourteenth had evidently been the critical day. In the cases in which the increased elimination took place before the fourteenth day, no delirium was observed after the occurrence of the increased discharge. The probability of this theory is strengthened by the fact that the other symptoms retained their febrile character, in the latter class of cases, till the ordinary period of crisis.

On the other hand, it may be argued that the retention of urea was merely a consequence of the abnormal condition of the nervous system, and not in any way a cause of that condition. This view would appear to derive support from my cases, in so far as, in the majority of them, the increased elimination did not take place till *after* the crisis had begun; but I think this support is more apparent than real, as, in half of these cases, the nervous symptoms continued to a considerable extent during the occurrence of the increased elimination, and did not finally disappear till the urea had fallen to the low standard natural in the enfeebled state of the patients.

It is very difficult to say why, failing organic lesion of the kidneys, urea should be retained, or why it should be retained to a greater extent in some cases than in others. Perhaps the retention is due to a defective innervation. The theory at present entertained, that in fevers there is a degree of paralysis of the nervous system, gives countenance to such a view.—*Edinburgh Medical Journal, Feb.*, 1866, *p.* 708.

7.—ON TYPHUS FEVER.

By H. W. Randolph, Esq., Milverton, Somerset.

[It is generally supposed that whilst typhoid fever owes its origin to badly constructed drains, typhus fever is produced by overcrowding: thus Murchison in his work on Continued Fevers, p. 25, says, "The conditions under which the poison is developed, *de novo*, are overcrowding of squalid human beings with deficient ventilation; in other words, the poison is generated by the concentration of the exhalations from living beings, whose bodies and clothing are in a state of great filth." All the recorded examples of isolated outbreaks of typhus point to this cause. We are therefore somewhat surprised to see an example in which the causes of typhoid produced typhus fever. It would have been well if Mr. Randolph had described the character of the rash.]

As it is still an unsettled question in the profession, whether typhus fever be contagious or the result of an epidemic; whether it be specifically con-

veyed from person to person, or influenced by atmospheric causes—a poisoned state of the air we breathe, overcrowded habitations, placed over foul cess-pools, or badly constructed drains, I think I shall be enabled to advance evidence to prove that an outbreak of fever in Milverton, in 1863, owed its origin entirely to the latter cause.

Situated in the lowest part of Milverton is a nest of houses, which, some twenty years since, was the parish work-house, but is now converted into seven cottages. The families are small, and the rooms capacious; but at the head of the row, turning at right angles with the top house, and facing you as you enter the premises, are several pigsties, and a large open privy for the accommodation of all the inhabitants of the row. The stench from this putrefying accumulation, openly exposed, without the slightest means of escape, was perfectly sickening, and highly offensive to the senses of all but those who were habituated to it. The consequence was that, in the fall of 1863, a fever of a typhoid character broke out, of a very malignant nature. No fewer than twenty were struck down; four died, whilst all the others went through a very protracted convalescence. The fever assumed a low type, presenting no acute symptoms, being ushered in with great prostration. There was no cerebral disease; no derangement of the digestive organs; no symptoms, in fact, indicating other treatment than stimulants and light nutritious diet.

The only important and interesting feature in the case worthy of attention, is the cause that produced this sudden attack of fever, and how it happened that the inmates of the several houses, one after another, without any communication with each other, were smitten with fever. The first case was that of a boy, 11 years of age. He was taken with mild febrile symptoms; and, with all the best care and attention that could be bestowed, the attack ran through the very worst form of typhus, and the boy, though he ultimately made a good recovery, was months under treatment. The case thus cursorily mentioned may be said to represent the type of fever that prevailed throughout the several infected houses.

The first family attacked lived in the house at the head of the row, contiguous to the pigsties and privy; whilst the second case, which ended fatally, was that of a man in the last house but one of the row, who had had no communication whatever with the family of the first house. The third case was two doors from the second. All the inmates of this house were struck down. Then came others in quick succession, until all the inhabitants of the several houses in the row were prostrated in fever. There was no evidence, from first to last, of any personal contact; and this sudden outbreak, I think, may fairly be attributed to the poisonous atmosphere arising from the causes before mentioned.

As the property belongs to an individual with whom I have some influence, I lost no time in representing the necessity of an entire and immediate removal of the great cause of offence. All the pigsties, &c., were swept away and built afresh far up in the garden. The premises were thoroughly drained, and the houses white-washed. The epidemic subsided; and, as this row of houses is detached by several hundred yards from other dwellings in the place, it was satisfactory to find that no fresh cases presented themselves in the neighbourhood, but that it was strictly confined to this locality; though, had it been contagious, it could not have failed to have been conveyed to other parts of the town by the nurses attendant on the sick.

Knowing that many high authorities attribute the spread of this disease to contagion of a specific character, and as the sanitary measures now being enforced in both town and country are, in my opinion, so essential to the happiness and well-being of the community, I think it desirable that the profession should be in possession of all the evidence that can be procured, to enable them to determine for themselves the simple question, whether the disease emanates from a cause not within the control of man, or whether we may look upon our crowded dwellings, choked-up cess-pools, and exposed decaying vegetable matter, as the fruitful sources of that most wide-spread malady, the typhus fever of this country.

I may add, in conclusion, that since the revolution was effected in the

locality referred to, the dwellings made clean, and nuisances removed, not one case of fever or other disease has occurred; and the district is now as healthy as any other part of the neighbourhood.—*British Med. Journal, Nov.* 25, 1865, *p.* 553.

8.—ON AN OUTBREAK OF TYPHOID FEVER WITH SPECIAL REFERENCE TO ITS ETIOLOGY, AND COMPLICATION OF DIPHTHERIA.

By Dr. Donald Campbell Black, Oban.

[Small places are much more favourable for accurate observations as to the natural history of fever, than are large towns, where, fever never being absent, the origin of cases frequently cannot be traced with certainty.]

On the 27th of August last, I saw, in company with a medical friend from Edinburgh, a case which I believed to be one convalescent from enteric fever, the history and then present appearances thus indicating. On the 30th of that month I visited a distinct case of this fever in a different part of the town, and occurring in a person who could not possibly have been in contact with the former case, or otherwise exposed to contagion. I saw this case at the commencement, and my diagnosis was confirmed by the gentleman alluded to. On the 3rd of September I was called to another case at a considerable distance from the foregoing, and to which the above remarks equally apply. Between the last date and the 12th September *seven* new cases had appeared, many of them almost instantaneously; from the 12th to the 20th September *ten* additional cases came under treatment; and in a space of four weeks I had under my care over thirty cases, and my friend, Dr. McGillivray, a greater number. Thus the occurrence of at least seventy cases of enteric fever in an isolated town, obviously independent of importation, created no small amount of uneasiness, and many and conflicting were the opinions entertained as to the probable cause.

Considering the suddenness of the outbreak; that no fever of any kind existed in the neighbourhood; that rich and poor were promiscuously attacked; that the fever was almost exclusively confined to the best and most airy part of the town; that it occurred during the prevalence of unusually hot and sultry weather, thermometer ranging from 55° to 80° in the shade, being as high as 93° in the sun—these considerations, I say, naturally indicated to me a general influence, and, in my capacity of medical Officer of Health, I accordingly suggested to the Commissioners of Police the propriety of examining the tank or reservoir *from which that portion of the town in which the fever prevailed* was almost exclusively supplied by means of a common pump. To this suggestion the authorities at once acceded. I watched the examination. On removal of the covering, a putrescent smell was very intensely perceived. A large quantity of mud, in a state of decomposition, occupied the bottom. The *rose* through which the water filtered was imbedded in this material; and some dead mice, &c., were found amongst the débris. It transpired on inquiry that the cistern had not been depurated for over a period of twelve years; and that within about twenty yards of the cistern, and certainly within its possible soakage, the sewage drain of a large hotel passed, and that this drain could not be water-tight, being of rough *rubble* build. The water had a somewhat milky colour, and, on standing for a night or so, deposited an abundant sediment. By tests which I applied, no organic gases were detected. Unfortunately, no portion of the water was retained for minute chemical analysis; but it was manifest to sight and taste that it was contaminated. Together with this, it may be added that there is no thorough system of drainage throughout the town; there is not an abundant supply of water; and such drains as do exist are only flushed during wet weather—conditions certainly favourable in hot weather to the formation of putrescent gases and the production of disease, but which it is in contemplation to remedy without delay.

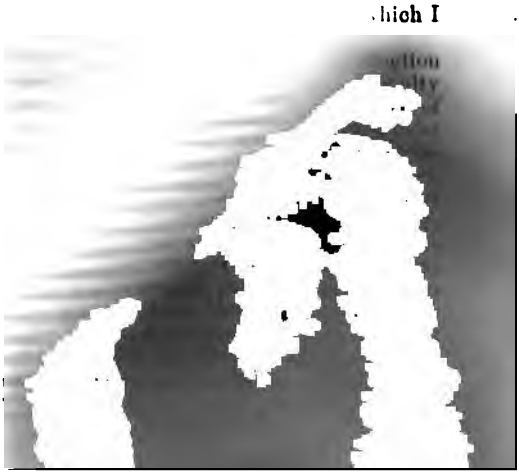
It was undoubtedly enteric fever we had to deal with. It unmistakably

showed itself in the gradual accession of symptoms, alternate flushings and chills, pains referred "to the bones," headache, profuse perspiration, great prostration, bowel complication, delirium, and rose coloured spots, well marked, at first as a rule, faint and disappearing under pressure, but subsequently elevated and unaffected thereby, &c. Nine-tenths of the cases occurred in the part of the town supplied by water as above; and such cases as occurred elsewhere came directly or indirectly within the focus of contagion or infection. It could not have been imported, the sudden outbreak and general prevalence in a particular locality militating against this assumption. And, lastly, the question of etiology. I submit that it is almost proved to a demonstration that the occurrence of the fever, the contamination of water, the defective drainage, and the unusually hot autumn weather, stand in the irrefragable relation of cause and effect.

On the occurrence of a single case of enteric fever in a family, without any subsequent spread.—During this epidemic several such cases were observed; but from those in town it is obvious that no inference could be drawn as to the contagiousness of the fever, as all were alike subject to the same influence. I am aware of only one instance in which a single case occurred in the country, without subsequent spread—i.e., being in the country throughout the whole course of the disease. The patient was frequently in town, and no doubt caught the infection there. I attended a case in town where the patient was, contrary to my express instructions, removed to the country during convalescence. She was progressing favourably up to the period of removal (Sept. 3rd); but from that period a relapse had taken place, and, till within a short time, recovery seemed very doubtful. Great emaciation ensued, and copious hemorrhage from the bowels took place. While in town the patient had severe epistaxis and spitting of blood. In this case, which was quite isolated, no subsequent spread took place. But on the other hand, cases have occurred in the country in which, doubtless, the infection was caught in town, where the disease did spread to other members of the family, and in which I failed to discover any reasonable cause determining its spread or the reverse. I am at a loss to comprehend how it happens that in some cases enteric fever does not spread from the person affected, and how in other cases it affects only certain members of a family, though all are equally exposed to contagion.

Complication of diphtheria with enteric fever.—I have never in the course of my reading, if my memory serve me well, observed allusion to this disease as a complication of enteric fever, nor have I ever before seen it as such in practice. In the course of this epidemic, true diphtheria supervened in *three* cases on the subsidence of the fever proper, if the expression be permitted. None of the cases, however, occurred in my practice. One of them I saw frequently. The whole fauces were covered with white patches of plastic exudation, implicating in all probability the larynx. There was extreme prostration, and subsequently epileptiform convulsions of a violent description ensued. During these fits, there was perfect insensibility, unequal dilatation of the pupils, and total blindness. None but the most sanguine could anticipate recovery. The pulse was extremely feeble, about 150, and suffocation seemed imminent. The strength was assiduously kept up by diffusible stimulants, such as brandy, ammonia, &c., and enemata of brandy and beef essence administered at given intervals, with as much of the same as could be given by the mouth. In the course of time several patches of the exudation material were coughed up, and from that time, to the wonderment of not a few, convalescence went on uninterruptedly. Dr. James Paterson, of Glasgow, saw the case while on a visit to this town, and will doubtless recollect it. I saw the same in another case, not nearly so severe, however, which also did well. In the third case, which I did not see, but which was described to me by my colleague as similar in all respects to the foregoing, the result was fatal, convulsions having also occurred before death. Of the identity of this throat affection, I may add, there could be no reasonable doubt.

To epitomize, I have arrived at the following conclusions on the subject



method to apparently comparable series of cases can show, they have a less striking victory over the simple treatment by cleanliness and nourishment of Dr. Cotter. It is the old problem of art *versus* nature. A comparatively slight advantage is, however, probably all that Dr. Chambers would claim or anticipate, for he studiedly attributes a very large share of success in treatment to continuous liquid nutriment and sponging of the skin: and his whole aim throughout these clinical studies is to show that those physicians are wrong who would represent disease as a positive agency alien to the body, and arrayed against the normal life, which is to be cast out and destroyed; and that he is right who looks upon disease as a deficiency of vital power—a disordered balance of vital functions—something less than life—to be treated by "restoring," not by "weakening" life, as Van Swieten and Boerhaave held, and as Schönlein and other moderns still believe.—*Lancet, Nov.* 25, 1866, *p.* 593.

10.—ON ACUTE RHEUMATISM AND ACUPRESSURE.

By Donald Macgregor, Esq., House Surgeon and Medical Superintendent, Barnhill Hospital and Asylum, Glasgow.

[Mr. Macgregor has lately tried both the treatment of acute rheumatism by blistering, and the arrest of hemorrhage by acupressure. The application of blisters to every joint affected by rheumatism must certainly have much evidence in its favour before it could be ventured on in private practice, as many persons would prefer the disease to the cure.]

In every case of acute rheumatism coming under my care for some time back, I invariably order the application of blisters to all the joints chiefly affected, and with the best results. I have now tried it in a good many cases, and never in a single instance found it fail to give relief.

One case, that of a man who had a severe attack two years ago, laying him up for more than six weeks, with most agonizing pain in his joints and great constitutional disturbance, was lately admitted with an attack similar in all respects, according to the patient himself, to the first. Blisters were at once applied to all the principal joints complained of, both in the upper and lower extremities, giving only a purgative internally. The following day he was free from pain, except what was caused by blistering the surface. No bad symptom followed, and in five or six days he was dismissed well.

Here, therefore, was an attack of acute rheumatism similar in every way to a former one in the same patient, which incapacitated him for six weeks, cured in as many days. The heart was slightly affected by the first attack, but was certainly not aggravated by the second. From my experience in all the cases I have treated in this way, I think that blistering is the most speedy and effectual mode of treatment yet adopted for this most painful malady. I find the blistering liquid the most simple and easily applied preparation of cantharides. It may be painful over the surface, however uneven, while there is often a difficulty in keeping the ordinary emplastrum cantharidis in contact with some joints, as the knees. It acts quicker also, which is an advantage in very painful cases.

Acupressure.—It is in one case more particularly that I found this method of arresting hemorrhage very useful, when a ligature could not be used. The case was an amputation below the knee in an adult male, necessitated by phadedenic sloughing of the whole of the soft parts of the leg below the middle, after typhus fever. The tissues were so soft and friable that no hold could be got of the posterior tibial. It gave way, either under the forceps or after a ligature was applied. A common darning needle was passed over it, and at once arrested the hemorrhage. It was removed on the third day. No secondary bleeding took place, and the stump was completely healed up within a fortnight—one of the best stumps I ever saw.

In such a case as this there can be no doubt of the value of acupressure, and my opinion is that, with a little more experience of its use, it will supersede the ligature in every case where it can be employed.—*Lancet, Nov.* 25, 1866, *p.* 593.

11.—ON BLISTERING IN ACUTE RHEUMATISM.

[The following cases are related by Mr. E. Howard Moore, and exhibit "the rapidity, efficacy, and safety of the plan" of treating acute rheumatism by free blistering. They were treated at the Infirmary of the Bethnal Green Workhouse.]

Case 1.—W. H., admitted February 17. All joints affected, but chiefly the right hip, both knees, and right ankle. To these blisters (acetum lyttæ) were applied, which gave relief immediately the serum was evacuated. Next day the right shoulder and wrist were intensely painful and swollen, and were treated in the same way and with a similar result, which shows the necessity of blistering all joints simultaneously, whether affected much or little. Discharged cured March 1, without any cardiac mischief.

Case 2.—W. L., admitted March 3, aged 19. Second attack. Rheumatic pains confined to the knees and ankles, but the fever in this case ran very high. Blisters applied to the affected joints, as in the other case, with an equally satisfactory result. Discharged cured, March 15, without any heart affection.

Case 3.—C. N., admitted March 21. Blisters were applied to right hip, both knees and both ankles, as in these joints only was pain felt. Much relief was afforded by the following day; but feeling some pain in right shoulder he asked to have a blister applied to it. Discharged cured, April 3, without any heart affection.

Case 4.—J. L., admitted April 6. Blisters applied to right elbow, right wrist, and right ankle, and also over the heart's region, on account of a slight mitral regurgitant bruit. Discharged cured, May 1, without any heart affection.

Case 5.—J. B., admitted April 12. In this case the wrists only were affected. Blisters were applied, as in the other cases, above the seat of pain. Next day able to move the joints without much pain. Discharged cured, April 24.

Case 6.—S. J., aged 58, admitted April 28. Joints affected were right hip, right and left knee and right ankle. To these blisters were applied. The fever in this case was well marked. Discharged cured, May 23, without any heart affection.

N. B.—In all these cases no medicine was given, except in Case 4, when a morphia pill was ordered at night, and in Case 6, a saline mixture as a placebo. —*Medical Times and Gazette, Feb.* 3, 1866, *p.* 131.

12.—CITRIC ACID AS AN APPLICATION IN CANCER.

By CHARLES J. DENNY, Esq., Birmingham.

The following case may be taken as a good example of the facts which I am desirous of laying before the profession:—

Mr. D., an old sailor, aged 70, consulted me in January last for an affection which caused intense pain along the whole side of the face, and great difficulty in mastication. His medical attendants had been treating it as a case of "tic." On examination, I found it to be scirrhus of the tongue, caused most likely in the first instance by a carious tooth. In this diagnosis I was confirmed by Mr. Oliver Pemberton and Mr. Thomason, of this town, and more recently by Mr. Crompton. The disease had extended too much to think of removing it. In every other respect he appeared a strong, healthy man. Life was miserable to him, and all I could do was to palliate. Large doses of morphia, chloroform, chlorodyne, and conium were tried at intervals, with little benefit. He was sinking from bleeding and constant pain. A curious fact is that he could invariably tell some days before the bleeding began, as the tongue at those times felt swollen, and the pain was more intense than usual; whereas after the bleeding there was little pain for some days. He showed me a paper one day in which citric acid was recommended as an application

in cancer, I had no confidence in it; but at his urgent request I ordered him a mouth-wash, containing one drachm of citric acid to eight ounces of water. He came to me a few days after, and said he felt greatly relieved from its use. When at all suffering he applied it, and the pain disappeared like magic. I may say here that it must be used two days before much effect can be expected. He can now sleep well, and go about his business comfortably, which is that of letter-carrier for a large works. He takes no opiates, and firmly believes as I do, that he has found out the means of "soothing the way to death."

The application may possibly prolong life, in so far as it relieves pain, and so prevents, at least in part, the break up of the constitution consequent thereon. It will be seen that this treatment is applicable in many ways internally and externally. I have tried it in two cases of cancer of the breast and one of cancer of the uterus, and with perfect success so far as relieving pain goes.

I have purposely avoided any theoretical views on this treatment, which will, I hope, receive an impartial trial on a large scale, and be duly reported upon.—*Lancet, March* 24, 1866, *p.* 312.

13.—ON A CASE OF REMOVAL OF CANCEROUS TUMOURS OF THE ARMS.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[The patient was sixty-two years of age, and underwent removal of a tumour, undoubtedly cancerous, from the upper arm, on the 12th of December.]

Jan. 9th, 1866. The patient had been going on remarkably well until today, the wound rapidly cicatrizing. This morning he complains of loss of appetite, has furred tongue, and pulse 100. Erysipelas has broken out in the arm, which is swollen and painful. Skin of a dusky red from the wrist to the wound, situated near to the insertion of the deltoid muscle. Tincture of sesquichloride of iron is painted over the part affected, and the patient ordered to take twenty minims of the same tincture in infusion of quassia three times a day; the arm to be raised upon pillows.

13th. Better; the arm much less swollen, and the wound again looking healthy.

The instinctive treatment of erysipelatous inflammation when you see the redness of the surface and feel the heat of the part, is to apply cold to it. But I would say to you, "Never use any really cold application, which will lower the temperature of the part, as you would by permitting a stream of cold water to trickle over it, or by putting on a bag of ice." But some of you are mentally exclaiming, "Oh, I have often seen cold lotions applied in erysipelas, and do good." I ask you whether it is not a fact that these cold lotions soon become warm applications by contact with the skin? At any rate, my advice certainly is that you never apply cold to cases of erysipelas.

Erysipelas must be regarded as a general blood-poison. Like all blood-poisons, it attacks those parts of the body which are the deteriorated portions of the system, causing an evolution of the poison either into them or upon their surface. But some one will observe, "Is it not much more likely that the erysipelas starts from the wound itself, and spreads to the neighbouring tissues, seeing that the margin of the wound is almost always the part first attacked?" I reply that the margin of the wound is the most deteriorated part; hence, erysipelas there manifests primarily its local action. This man was fortunate, inasmuch as the erysipelas attacked the skin of his arm, in preference to manifesting its local action on some of the deeper tissues; it was certainly better for him that an evolution of the poison should take place on the surface than that he should have pneumonia, or that his brain or liver should become disordered. Had a bag of ice been applied to his arm, probably the poison might have been diverted to some other locality,

where its attack would have been attended with more serious consequences. I think it right, when erysipelas has commenced on the surface, to solicit its evolution by warm fomentations, medicated with poppy or hemlock, so as to reduce the sensibility of the skin, for an erysipelatous part is usually hyperæsthetic.

But a question naturally suggests itself for our inquiry: If the generation of the poison does not start from the wound, where does it commence? Most frequently I imagine in some part of the alimentary canal. Take this man's case. Just before the erysipelas showed itself, he had loss of appetite, which you will find one of the most frequent premonitory symptoms. He does not seem to have experienced a desire to vomit, though this is often superadded to the loss of appetite. Does not this appear to indicate that the stomach is desirous of getting rid of something which is irritating the organ? I so interpret it, and act upon the suggestion by giving an emetic, and afterwards clearing out the bowels by a purgative. The treatment I have over and over again seen quickly followed by the most beneficial results, the erysipelatous blush quickly disappearing from the surface, the appetite returning, and the patient expressing himself as feeling much relieved by the vomit. But why does the erysipelas cease? I must assume that there is a morbid fluid secreted by and within the stomach, which is reabsorbed by the stomach or intestines, and which poisons the blood and gives rise to erysipelas. I can give no positive proof of the truth of this statement; but I am sure that, by receiving it as true, and adopting as the earliest step the treatment by an emetic and a purgative, you will frequently in your practice be gratified by witnessing the rapid arrest and disappearance of existing erysipelas.

I just now laid it down as a principle that, in constitutional disorders dependent upon blood-poisons, the local symptoms are manifested at the spot where the tissues are most deteriorated. You see this expressed in the sequelæ of small-pox, measles, scarlet fever, typhus fever, &c. A patient, apparently previously healthy, goes through the fever process, and just when about to get up, some local manifestation of mischief is discovered, such as inflammation in the knee or at the hip-joint, an empyema, a large chronic abscess forming at some part, &c. This is the more remarkable as far as the joints are concerned, because the patient may have been lying down during some weeks, and the joints have been comparatively free from any disturbance as regards motion at the part. But what is the explanation of the events? The hypothesis is that, previous to the occurrence of the fever, a slight strain or bruise was received at the part, or that from some pre-existent latent cause of deterioration in the structures, these parts subsequently manifest locally the general morbid condition of the system. If a small principle like this can be recognised, you may see how widely it is capable of being applied, and it will help to explain many points which might otherwise be clothed with the obscuring title of "anomalous," or lost in the meaningless term of "caprice of nature."

The skin of the patient's arm was of a dusky-red tinge, and the inflamed part was very little swollen or raised above the level of the surrounding healthy integument. These local symptoms marked the erysipelas as of an asthenic character. His vital powers were low; he had to repair a considerable wound; so that the local inflammation was of an asthenic type. The inflamed parts were painted with tincture of the sesquichloride of iron. Why so? It may act as a direct astringent, or it may excite the capillaries to contract, and thus tend to diminish capillary congestion. Hence it is one mode of applying pressure to the blood-vessels which are dilated, as you can see, by the redness of the part. It moreover unites with the cuticle, shrivels it, and forms a thick covering to the inflamed skin, and, acting as a bandage, makes pressure upon the capillaries, and keeps off the oxygen of the air that otherwise acts as a stimulant to the capillaries of the surface. I did not give this man an emetic, because he did not complain of a feeling of nausea; but I ordered him the tincture of sesquichloride of iron internally, with infusion of quassia, so as to improve his appetite and keep up the gen-

eral powers of his system. Or did the steel act upon the contents of the stomach as an antiseptic, and so act beneficially?—*Lancet, Feb.* 24, 1866, *p.* 196.

14.—POLYDIPSIA—TREATMENT BY LARGE DOSES OF VALERIAN—IMPROVEMENT.

Case under the care of Dr. Reith, at the Aberdeen Royal Infirmary.

[The patient, who was 24 years of age, was admitted for typhus, but no symptoms of that disease was discovered, and the case was regarded as one of febrile catarrh. The nurse, however, called attention to an insatiable thirst of which the patient complained, and to the enormous quantity of urine daily voided by her. The patient said that she had suffered from these symptoms for the previous nine months, since her baby was weaned. Both her father and brother had had diabetes; the brother died of it; but the father died of consumption. The patient's urine was of sp. gr. 1000, and contained no albumen or sugar. She was ordered ʒ i. of tincture of valerian three times a day. The spec. gr., however, remained 1001. The powder of valerian was then given instead of the tincture, and during eleven days the daily dose was increased from 60 grains to 140 grains, and the spec. gr. rose during the same period to 1006.]

I lost sight of the patient owing to an attack of illness; but I was told that before she left the hospital the specific gravity of the urine had risen to 1008. I afterwards had a sample of the urine sent me on March 3, 1865; the specific gravity was 1006. I have not seen the patient since.

Clinical Remarks.—Polydipsia, once established, is generally incurable. Although at first a mere inconvenience, and not interfering materially with the general health (the subjects of it, like our patient, being often stout and ruddy), it sooner or later—it may be after the lapse of years—tells on the system, and at last it ends in tubercular phthisis. The present case will thus terminate, for there were symptoms of tubercular deposit in the right lung during her stay in hospital. This case is an example of the undoubted connexion existing between glycosuria and polydipsia. Two of the patient's near relatives were affected with the sugar disease, and although the father was said to be cured, it is evident that the phthisis of which he died must have been induced by the glycosuria, of which it is so often the termination. The polydipsia, therefore, in this patient, as in most, if not all cases of the same kind, had come by hereditary descent, so to speak, from Glycosuria. In fact, it is now generally admitted that glycosuria, polydipsia, and even albuminuria are interchangeable; that is, if an individual be affected with one of those diseases his descendants are liable to either of the others. The well-known experiments of Bernard show that these diseases may be produced by injuring certain parts of the nervous system in close proximity to each other. Hitherto, polydipsia, or, as it is most commonly called, diabetes insipidus, has been regarded as not only incurable, but also irremediable. M. Rayer was, I believe, the first to point out that valerian administered in large and rapidly increasing doses was a most serviceable remedy. Trousseau relates some cases which were perfectly cured, at least for the time, by this treatment. The result of its employment in the present instance was satisfactory, and there is reason to believe that had opportunity been given to persevere in its use, and had the patient been more tractable than she was, she would have been still further benefited.—*Med. Times and Gazette, March* 24, 1866, *p.* 310.

15.—ON THE TREATMENT OF DISEASE BY OXYGEN.

By Dr. R. H. Goolden, Sussex Gardens, Hyde Park.

[The following paper gives the result of some experiments which have been carried out by Dr. Goolden, at St. Thomas's Hospital.]

I had remarked that the effect of the Turkish bath in cases of gout was not so satisfactory as in cases of rheumatism, though I must admit that in many great relief was given by it. The acute paroxysms were relieved for a time, but I could not but remark that the lithates continued to abound in the urine, and not unfrequently in greater excess. This may be accounted for by the diminished quantity of the secretion. This lithic acid condition of urine appears to me to be more prevalent in cases where, from lung-disease or from vitiated atmosphere, the respiratory functions were imperfect, as well as in cases of degenerating tissue, abscesses, and fevers.

Lithic acid does not appear in the cutaneous excretion of the Turkish bath, even where it is known to abound in the blood, and it occurred to me that the formation of urea, instead of uric or lithic acid, might be determined by supplying oxygen.

The idea of inhaling oxygen was mooted by Sir Humphry Davy and by Dr. Eddoes in the year 1804, but not adopted generally, probably from the difficulty of obtaining the gas in sufficient quantity and purity. This difficulty has now been happily overcome, and the means of procuring it in a portable form is afforded us by Mr. Barthe, of Long-acre, and also by Messrs. Garden and Robbins, of Oxford-street, by the manufacture of the oxygen water by the former, and of the binoxide of hydrogen by the latter. For the method of obtaining this latter preparation in its present perfection I believe we are indebted to Dr. Richardson.

Till I could obtain an instrument for inhalation in the hospital, I commenced with the use of the two latter remedies. The binoxide of hydrogen I exhibited in one-drachm doses, diluted in about two ounces of water, and in many cases I found it to have a marked influence on the biliary secretion, of which it increased the quantity and improved the quality, often producing excessive biliary dejections, and thus relieving congested livers and secondary bronchial congestion. I procured a large vulcanite bag with a tube, stop-cock, and mouth-piece, which was filled with a mixture of oxygen and air in the proportion of one to four. The gas is inhaled by the patient, and expired into the atmosphere. The result has been pretty uniformly to clear the urine, and in many cases of chronic gout the continued use for half an hour daily, very slowly inspired at intervals, filling the lungs as much as possible, has given very great relief, and in some cases cures have resulted.

I have adopted this plan without the use of any medicine or other appliance which might complicate the treatment; but latterly I have found, in carefully selected cases, the Turkish bath a great expeditor of the absorption of enlarged joints, and that in cases where the Turkish bath alone had previously failed.

There are, however, two cases that are particularly deserving attention. The first was that of a married woman admitted under my care in August with phagedenic (and as I thought syphilitic) ulceration of the soft palate. A large opening existed between the mouth and nostrils, so that she could not swallow fluids but with the greatest difficulty. The sides of the ulcerated and divided palate were swollen and purple, and the edges everted, and secreting an unhealthy pus and ichor. There was no history of syphilis. Aware of the benefit in these cases of the inhalation of the fumes of peroxide of mercury when thrown on plates of hot iron, also that mercury in any other form is usually injurious, and considering that such fumes were those of metallic mercury and oxygen, and having the oxygen bag at hand; I caused her to use it twice daily. The result was a very rapid improvement, and in the course of three weeks she left my care, still with a divided soft palate and loss of uvula; but the edges of the opening were perfectly healed and the throat healthy. I may remark that no other remedy was used. The result was beyond my expectation, and I determined to try it in the next case of phagedenic ulceration that should present itself.

This opportunity soon occurred in the case of John M., aged 28, admitted on the 28th November, 1865. The patient had several large ulcers on rose-coloured and swollen bases—covered with brown crusts in the centre, with elevated and everted edges, from which was secreted an unhealthy ichorous discharge—one large one on the vertex, another on the forehead, two on the

left shoulder, with many cicatrices, the marks of former sores, and his forehead much disfigured. But he was also suffering from double pneumonia, coughing up quantities of rust-coloured and viscid mucus; the bases of both lungs solid, and crepitations over the other parts of the chest where the air entered; great dyspnœa. The smell of the patient was so offensive that he had to be removed to a separate room, and it required some effort to approach him for examination. The course adopted was to remove, if possible, the stench, for which purpose we used the solution of chloride of lead, formed by mixing solutions of nitrate of lead with common salt (by far the best and cheapest deodoriser, and long in use in our wards). His mouth was washed with a solution of Condy's ozone water constantly. He took one drachm of binoxide of hydrogen in two ounces of water three times daily, and inhaled from the oxygen bag twice daily. I employed no other remedy, but ordered wine and brandy, and any thing he could be induced to eat. This was on the 29th of November. My next visit was on the 2d of December, when I hardly expected to find him alive; but I found him sitting up in bed, and very cheerful. The lungs had recovered themselves. He took his food with a good appetite. No smell whatever about him, and the ulcers had all assumed a healthy character. He continued these remedies, and no other, till he left the hospital on the 27th of December, the scabs only being removed by poultices, and the sores then dressed with simple dressing.—*Lancet, March* 10, 1866, *p.* 270.

DISEASES OF THE NERVOUS SYSTEM.

16.—ON PROGRESSIVE LOCOMOTOR ATAXY, OR WASTING OF THE POSTERIOR COLUMNS OF THE SPINAL CORD.

By Dr. Julius Althaus, Physician to the Royal Infirmary for Diseases of the Chest, City-Road.

[This disease, recently described by French physicians as progressive locomotor ataxy, was first described more than thirty years ago as tabes dorsalis, yet the nature of the complaint and the connection which exists between the symptoms manifested during life and the structural changes found after death have, until quite recently, been misunderstood.]

Tabes dorsalis is first spoken of in the works of Hippocrates, and was by the father of medical literature believed to arise from excesses in sexual intercourse, the chief symptoms of the disease being spermatorrhœa, marasmus, and hectic fever. This meaning of the term, however, has gradually changed, and those authors who wrote on tabes in the first decennia of this century understood by it atrophy of the posterior portion of the spinal cord, brought on, not merely by sexual exhaustion, but also by exposure to wet, rheumatism, gout, and other causes, the chief symptom being a peculiar form of paraplegia. The disorder was chiefly investigated by English and German physicians, such as Abercrombie, Hufeland, Steinthal, Romberg, and others. Their descriptions, although in some instances most eloquent, were however, to a certain extent, wanting in accuracy, inasmuch as several different affections of the cord were comprehended under the name of tabes, and a clear distinction was not drawn between tabes and paraplegia. It was only after a more careful clinical study of the symptoms had been made, and after pathological anatomy, aided by the microscope, had stepped in, that a peculiar disease of itself, and one characterized by uniform structural lesions, could take its place in our nosological system. The chief credit of the anatomical investigations is due to Professors Virchow, Türck, Rokitansky, and Leyden, and in this country to Dr. Gull and Mr. Lockhart Clarke, who have shown that, in well-marked cases of tabes, an actual waste of nerve-fibres of the posterior columns of the spinal cord takes place, together with the formation of amyloid corpuscles and considerable proliferation of connective tissue.

The first who drew a distinction between this disease and paralysis was

Dr. Dodd. He said, in an article on the nervous system in his Cyclopædia, that two kinds of paralysis might be noticed in the lower extremities; the one consisting simply in the impairment or loss of voluntary motion; the other distinguished by a diminution or total absence of the power of co-ordinating movements. In the latter form, while considerable muscular power remained, the patient found great difficulty in walking, and his gait was so tottering and uncertain that his centre of gravity was easily displaced. In these few words we have a good description of the system of *ataxy*, upon which lately so much stress has been laid by French physicians. The term "ataxy" is as old as that of "tabes," for it also originated with Hippocrates; and it has likewise entirely changed its meaning in the course of time. Some authors have applied it to chorea, others to fevers, others to various nervous disorders. At present, however, we understand by ataxy, not a disease of itself, but merely a symptom to which various disorders may give rise, and which essentially consists of a want of co-ordination of voluntary movements, and a tendency on the part of the patient to lose his balance, but without actual loss of power, and apart from tremor, chorea, and paralysis. This symptom may be observed in disease of the cerebellum, and in poisoning by alcohol, lead, and mercury; but it is more especially connected with that disease which has been long familiar to us as tabes. The best clinical study of this symptom we owe to M. Duchenne de Boulogne, who, from 1858 up to this time, has published a number of memoirs, in which he described what he thought to be an entirely new disease, which he called "progressive locomotor ataxy," and which he believed to be a functional disorder of the cerebellum. His apparent discovery was hailed as a real one in France, and Professor Trousseau actually proposed to call the new stranger "Duchenne's disease;" but, on looking more closely into the matter, we find that Duchenne's description is altogether applicable to our old friend, tabes. I have not the slightest hesitation in acknowledging the great ability and originality of M. Duchenne's researches, which were perhaps more strikingly displayed in this case just on account of his being unacquainted with the previous literature on the subject; yet, if I thought *it* desirable to attach a proper name to this affection, I should prefer calling *it* "Todd's disease," as Todd first drew the distinction between ataxy and paralysis eleven years previous to Duchenne. But the best plan is, perhaps, merely to drop the term "tabes," as being too vague, and to call the disease under consideration "progressive locomotor ataxy, or wasting of the posterior columns of the spinal cord."

The following details of a case of this affection, which I have had under my observation for about five years, are well suited to illustrate the symptoms and progress of the disease:—

R. B., a commercial traveller, aged 37, a tall and rather spare man, with a sallow complexion, first consulted me in February, 1860, when he gave me the following history. His mother had always been healthy, but his father had for the greater part of his life suffered from epilepsy, and died in a fit. The patient was brought up to the law; but in consequence of a disappointment he left that profession, and enlisted as a soldier. He served in Australia and Canada, and during that time suffered much from rheumatism. He never exceeded in drinking, but occasionally in sexual intercourse. He twice had gonorrhœa, but no syphilis. He suffered for a long time from hemorrhoids, for which, in 1855, he underwent the operation by ligature, and he ascribed the commencement of his present illness to that operation, saying that he never felt quite the same man after the hemorrhoidal flow had ceased. He left the army in 1856, and married. He is now father of a healthy child. For the last eighteen months he has been a commercial traveller, and as such he is always on the move, and frequently exposed to cold, damp, and great fatigues. In January, 1857, he first noticed that his sight became weak, and he had some difficulty in writing and reading small prints. Soon after he felt pains of a peculiar character, which he described as electric shocks through the legs, and as if the muscles were being rent asunder. These shocks came every two or three minutes. He underwent treatment by liniments and other external applications, but without relief. His gait now became tottering, and

he had considerable difficulty in walking. He never goes out without a stick, and sometimes he is obliged to use two. In the summer of 1859 he consulted the late Dr. Todd, who told him that his case was incurable, and that he would have to be on the sofa for the rest of his life. He prescribed strychnine and iron, and after having taken it for some time the patient felt rather stronger, but there was no improvement in the special symptoms of the disease.

Present State.—February 14th, 1860: The patient's intellect, memory, and speech are quite normal. He does not suffer from headache, giddiness, strabismus, or ptosis. Both pupils are enlarged, the left more so than the right. He complains of weak sight, and the opthalmoscopic examination shows the arteries of the optic nerve smaller than usual. His senses of hearing, smell, and taste are natural. Respiration and heart-sounds normal; pulse at the wrist rather feeble, but quite regular. His digestion is tolerably good. Tongue clean, appetite satisfactory, but bowels rather costive. He complains of a sensation as if a net were tightly drawn round the abdomen. He is occasionally troubled in passing the urine, but there is no stricture. The urine is of 1030 specific gravity, and contains a sediment of urates, but no excess of phosphates, and no albumen or sugar. The sexual power has not notably diminished of late. On examination of the back by pressure, percussion, galvanism, and hot sponges, no place can be discovered which is particularly sensitive. The patient complains of numbness in the hands, more especially in the third and fourth fingers of the left hand. He can distinguish heat and cold, and feels the prick of a pin and pressure distinctly; yet the æsthesiometer shows a considerable diminution of tactile sensibility in the fingers. The upper extremities are pretty well nourished, and the muscles answer well to a galvanic current of moderate power. He can bend and stretch the arms with force, but he finds it difficult to button his shirt and to feed himself. The lower extremities are more affected than the upper ones. The patient has had sensations of "pins and needles" in the feet, but these have for some time past given place to numbness. He says that in walking he has a sensation as if the steps rose under his feet. He must continually look at his limbs in order not to lose his balance, and can scarcely walk at all in the dark. If told to shut the eyes or stand with both feet together, he begins to stagger. In walking he throws the legs forwards with a jerking motion; and, as he cannot measure his distance from the ground, he puts his foot down with great force. If lying down he can bend and stretch the legs with considerable power; but he seems to exaggerate every movement, all muscular contraction being, not slow and equable, but violent, sudden, and jerking.

I ordered the patient thirty minims of the syrup of iodide of iron three times a day, sulphur baths twice a week, and a pill of aloes and myrrh at bedtime. After having used these remedies for about a month, there was a marked improvement in every respect. The pains were nearly gone, the sight was rather better, the walking decidedly steadier, and less fatiguing, and the feeling of numbness slighter. He went on favourably until March, 1861, when, having a long and very laborious journey, to perform, he returned considerably worse, and from that time the disease gradually gained upon him, in spite of all treatment. In 1863 he had a course of nitrate of silver, but the affection was then evidently too far advanced for any medication to do good. The sight got rapidly worse, and he became at last completely amaurotic. The sensation in the lower extremities was also entirely lost. The patient was now confined to his room, and during the last six months of his life he never left his bed. His intellect remained unimpaired to the last, and his disposition was always cheerful. He died in January, 1865, of a sharp attack of bronchitis.

[The author then describes in detail the anatomical changes which are found in patients who have succumbed to this disease; after which he proceeds to an elaborate analysis of the special symptoms, laying chief stress on the affection of the cerebral nerves and the pains in the first stage, and on the symptoms of ataxy and the loss of the different kinds of sensibility in the second stage of the disorder. The causes generating ataxy are then mentioned, and the diagnosis is fully gone into. He then continues:—]

The prognosis is not favourable, for up to the present time not a single

case is on record in which perfect recovery has ensued. Indeed, as late as 1851, Romberg wrote that there was no hope for patients of this class, that a fatal issue was inevitable, and that it was but common humanity to inform them that therapeutic interference could only injure. This sad confession of impotence need no longer be made, and we may certainly congratulate ourselves on having far more control over the disease than we had formerly. Moreover, its progress is seldom rapid, although acute intercurrent disorders, such as erysipelas, bronchitis, and pneumonia, are grave complications, which may carry the patient off suddenly. Much must depend upon the period at which the case comes under treatment. If all the symptoms of the disorder are fully developed, the hope of a cure may be slight, although even then much may be done to alleviate the patient's suffering. The case is different if the patient presents himself in an early stage of the disorder. The fact that the cerebral nerves, with the exception of the optic, generally recover from their affections in the course of the disease, goes far to prove that previous to the structural changes in the cord there is a functional stage, in which much may be done by medicines. Moreover, we must bear in mind that Messrs. Charcot and Vulpian have found nervous fibres in the process of reparation in the cord of a man who had died from ataxy; and that therefore even at a later time we must not give up hope altogether, especially if the patient is placed in favourable circumstances.

I now come to the treatment of ataxy, and will first say a few words about diet and regimen. This must depend a good deal upon the condition in which the patient is at the time he comes under treatment. If he is in a weakly state, plain and nourishing diet, with iron, quinine, and cod-liver oil, should be prescribed. I have never seen a case in which lowering did any good. Hippocrates has recommended milk diet in erotic tabes, and Eisenmann speaks highly of the same in progressive ataxy. I have often given milk-and-brandy, two and even three times a day, with decided benefit to the general health; but have never found it expedient to insist on an exclusive milk diet. Exercise should be very moderate; and for those who have undergone great fatigue, rest is most beneficial.

Counter-irritants to the spine have been used by many physicians, but, as a rule, the benefit obtained has not been proportionate to the sufferings inflicted by their application. The moxa, the hot iron, issues, blisters, and leeches have been employed. I give the preference over all of these to the continuous galvanic current, applied to the lower and middle portion of the spine. It has in several cases seemed to me of decided benefit in lessening the pains, and also the disagreeable feeling of constriction which is often felt at the abdomen and the chest. The action of this agent is as rapid as that of the iron; its application entails far less trouble and suffering than the latter; and its efficacy is superior to that of issues, blisters, and leeches. Dry cupping on both sides of the spine is also useful.

Iodide of potassium has been recommended by Duchenne and others; but no cases have been published in which this remedy has proved successful. On the contrary, several are on record in which it seemed to accelerate the progress of the disease. I have given it in two cases, but without any effect, and am certainly not in favour of its administration. Iodide and bromide of iron are useful, but do not effect a cure. Mineral waters have been very frequently employed, both externally and internally; and it seems that for a time they do good. Amongst the French spas, Barèges is one of the most recommended; amongst the German waters, Marienbad and Wiesbaden have a special reputation; while chalybeates and indifferent thermal springs, which often prove useful in certain forms of paralysis, have entirely failed in ataxy. On the whole, I should be averse to sending atactic patients on a journey to some distant spa, as rest at home, with certain remedies to be mentioned hereafter, is more beneficial than all the mineral waters in the world. If, however, there should be costiveness and abdominal plethora, Carlsbad, Marienbad, or Kissingen water may be drunk at home with benefit. Sulphur baths may also be taken at home; and I think highly of them. I have never employed them alone; but they certainly seemed, in conjunction wtih other remedies,

to do much in relieving the pains and diminishing the numbness. The patients feel more brisk and supple after the baths, and are almost always desirous of repeating them as often as possible. A sulphur bath may be prepared by dissolving from three to six ounces of the sulphuret of potassium in two pints of warm water, and adding this to the bath. Vapour baths and Turkish baths should be avoided.

Nux vomica and strychnine have frequently been used, but generally seemed to do harm. Duchenne recommends faradization, and the late Professor Remak, of Berlin, praised galvanization as a curative agent. From faradization I have never seen any benefit; galvanization has proved useful at my hands not only in the pain and feeling of constriction, but also in the affections of cerebral nerves, which are so frequent in the commencement of ataxy; on the other hand, it has been powerless against the disease itself, more especially against the symptom of ataxy.

Amongst the other remedies which have been used in this affection without much result, I will mention opium, bromide of potassium, secale cornutum, essence of turpentine, and arsenic. The only remedy which seems as yet to have some good, in a very large proportion of cases, is the nitrate of silver, given in doses of one-tenth to a half grain, two or three times a day. Professor Wunderlich, of Leipzig, was the first who employed silver in seven cases of this disease, in none of which, it is true, he obtained an actual cure, but in most of them considerable improvement. In 1862, Messrs. Charcot and Vulpian, in France, took up Wunderlich's idea, and used the nitrate in five other cases, and in each of them there was much amendment. Since then this remedy has been employed in most cases of ataxy, and with somewhat variable success. In some it has so disagreed with the patients that it was necessary to discontinue its use; in others it had little or no effect; while in the majority of cases the remedy has proved, if not curative, at least very useful, and it is the one upon which most reliance can be placed in the treatment of this disease. I am in the habit of giving the silver combined with the hypophosphite of soda, and it seems to do far more in this combination than either of these remedies singly. I have, indeed, now a case of ataxy under my care in which the improvement has, for the last six months, been so considerable under this medication that I am hopeful of a cure. Certain precautions should, however, be taken in administering the nitrate. I generally employ it for four or six weeks consecutively, and then discontinue it for a fortnight or three weeks, giving in the mean time a slightly aperient mineral water. After this the use of the remedy may be safely recommenced, and continued for a month or so. The gums must be inspected from time to time, as the peculiar coloration which silver produces in the long run first appears in the mucous membrane, and only afterwards in the skin. With the precautions mentioned, however, no disfigurement of the patient need be feared. I have never gone beyond the dose of half a grain, and perhaps this is another reason why in my cases the remedy has been borne without any inconvenience. I should, however, not recommend all cases of ataxy to be treated alike; in this affection we must, as in every other, study each individual case by itself, and advise for it what seems, under the special circumstances, most likely to do good. Thus hysterical or hypochondriacal patients in whom ataxy may supervene, will require a different treatment from plethoric persons, or such as have long suffered from rheumatism, or have been subject to privations and anxiety. Much is, therefore, left to the tact and discrimination which, together with knowledge, should in all cases brought before him, guide the doings of the physician.—*Lancet, Dec.* 30, 1865, *p.* 726.

17.—A CASE OF PROGRESSIVE LOCOMOTOR ATAXY.

Under the care of Dr. JOHNSON, at King's College Hospital.

[The disease known as locomotor ataxy has been much discussed within the last few years by Duchenne and others, under the name of "ataxie locomotrice." This disease had until recently been included in the class Paraplegia;

but in well-marked cases there is no actual paralysis of the muscles, but loss of power to guide them. The patient is obliged to "lean on his eyesight as on crutches," and if he shuts his eyes he stops, and sometimes falls. The posterior column of the cord is the part diseased. The following is the account of a case in a man forty years of age. He had from boyhood been engaged in the cod-fishery off Iceland, and thus for the greater part of the year was much exposed to wet and cold. The symptoms of the disease dated four years back.]

On admission, he was noted as a short, well-built, powerful man; no loss nor thickening of speech, and no deafness. He seemed to be intelligent, and gave a clear account of himself. The most characteristic point was his walk. He could stand very well with his feet apart, but when he put them close together he could not maintain his position, even when looking on the ground. On taking a step, he would advance in a hesitating, tottering manner. He always leaned forwards, and seemed to have some difficulty in balancing himself, especially when turning round. These points were better seen when he shut his eyes, for he was only able to totter a few yards, and then would fall. He walked pretty well when his eyes were off the ground, as when he was looking at the ceiling; but directly he closed his eyes he seemed to lose all confidence, and reeled like a drunken man. Although there was this loss of power in co-ordinating his movements, there was no loss of muscular power. He would lift heavy weights, and even carry patients round the ward. When sitting in a chair with bent knees, he could resist all attempts to straighten the legs. There was no muscular atrophy, and but slightly diminished sensation in the left hand. The left side was rather more affected than the right. It appears he had a chancre four years ago, but it came on after the first appearance of numbness in the leg. There were some syphilitic patches on the tongue. There was no sore-throat nor rash on the skin; no nodes, nor glandular swellings anywhere. Lung and heart sounds healthy. Urine pale; specific gravity 1010; no albumen. Does not pass urine more than three or four times a day. His intellectual faculties do not seem impaired; he has slight loss of memory. His sight is not quite so good as it was. He has no pains in his head nor down the spine. Hearing and speech good. No affection of the cranial nerves apparently. Appetite good. Bowels open; no loss of power over the sphincters.

He was put on a liberal diet, and treated with iron and quinine. Galvanism was applied daily, with apparent benefit.

During his stay in the hospital he seemed to improve a little. At times he would walk fairly well, but on wet days he was always worse, and depression of spirits would come on. He was discharged Feb. 8th, 1866.—*Lancet, March* 31, 1866, *p.* 344.

18.—A CASE OF PROGRESSIVE LOCOMOTOR ATAXY.

Under the care of Dr. HUGHLINGS JACKSON, at the London Hospital.

[Dr. Jackson observed that this was one of the most interesting cases of locomotor ataxy that he had ever seen. The patient was 39 years of age. The first symptoms observed, when 14 years of age, was loss of sight; but this improved so that in nine weeks he was able to see tolerably. He then became almost quite deaf, and then the sense of smell was lost. Taste has never been affected. There was also clear evidence of diminished sensation in the legs. The patient is liable to sudden and violent pains in the legs.]

The patient can get along when his eyes are open; but his gait is precipitate, and he staggers. When he shuts his comparatively good eye, he can only manage to keep his legs for a moment. It is scarcely right to say that he can stand; a fairer description would be, he does not fall at once. He cannot stir a step with his eyes shut. Dr. Jackson particularly drew our attention to the fact that this patient could manage to walk when looking at the ceiling, although, of course, not so well in so awkward a position. He (Dr. Jackson)

told us that a year ago he had had under his care a man suffering from locomotor ataxy who could walk whether he looked on one side or at the ceiling. And he remarked that, although most patients suffering from ataxy say they "must look at their feet," some can walk without doing so if their eyes are open. The last-mentioned patient could walk with his eyes shut if he merely touched a wall with his fingers without resting any of his weight on it. Another patient, who was quite blind, and whom we saw with Dr. Jackson at the Hospital for Epilepsy and Paralysis, averred that he could walk for a long distance in the park if he just touched the railing with one hand; and, on trial, it was found that he could get along much better than we could have expected in this way. More than one patient has said that, when they shut their eyes, they "feel as if there was nothing under them." It seems only necessary in these cases, Dr. Johnson said, for the patients to establish some definite relation with the objects about them. The patient's wife said that he began to stumble about a year after the blindness. He had first pain in his back, and there was some lumps on it; but no precise information could be obtained on these points.

The patient has had difficulty with his bladder eleven years; and this may possibly be essentially a motor defect. If so, it is the only paralysis. The patient can stamp well with his legs, can resist flexion and extension fairly, and his legs are well nourished. There is no paralysis of any one of the cranial motor nerves, nor any history of squinting.

The man seems intelligent. His talk is rather odd, but there is none of the thickness of general paresis about it. He talks in the loud harsh way some people who have long been very deaf get into. He was so deaf that it required much patience to obtain a few facts from him. Dr. Jackson at first tried to communicate with him by writing words of monstrous size on paper. In this way, he got to know that the patient denied sexual excesses, and that he used to drink hard, and also, a fact that is worth notice, that he had smoked an ounce of tobacco a day for eighteen years.—*Lancet, March* 31, 1866, *p.* 346.

19.—ON SPASM.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital, and Lecturer on Medicine.

[By spasm the writer means that kind of muscular contraction which we observe in tetanus, and other similar affections.]

A point which seems to me very worthy of notice is, the affinity which is not obscurely marked between spasmodic contraction and paralysis, however widely at first sight the two states appear to contrast with each other. I will adduce some instances. Exposure to cold is a recognised cause of tetanus, and we have seen that it is also of facial and some other forms of paralysis. Dental irritation will sometimes cause trismus (a spasm), sometimes ptosis (a paralysis). Mr. Moodeen Sherriff has recorded some instances of paralysis not depending on organic lesion, in which there was rigidity of one limb, and simple immobility of the other on the same side, the same cause having given rise to both conditions, and the same treatment removing them. Dr. Little, many years ago, expressed the opinion, as the result of extensive observation, that with regard to spasm and paralysis the nature of the primary lesion in the central organ is the same in either case. He regards, however, paralysis as the result of a higher degree of lesion than spasm. A patient of mine who suffered from brachial neuralgia, which yielded to quinine and iron, stated that if she worked hard in the day she had not only severe pain at night, but also complete stiffness of the hand, induced, of course, by spasm of the muscles of the forearm. If she worked but little, the spasm did not occur. In this instance, the influence of exhaustion in inducing muscular spasm is very apparent, as also the relation of the latter to the paralytic sensory disorder. A case of cataleptic affection is related by Mr. Buchanan, in which the muscles were rigid as iron during the attacks, which lasted twenty-four hours. They were induced by

excitement, want of food, and fatigue, and subsided with the aid of rest, warmth, and an opiate. In most persons, languor and weariness would have been the result of the above-named exciting causes rather than spasm. The inference, however, seems just that these states, dissimilar as their outward show is, are not in their origin essentially opposites. Mr. Bryant relates the case of a female, aged 17 who was suddenly attacked with contraction of the flexor muscles of the left hand and forearm, a week before she came under his care. Her health was good, catamenia regular; the sensation of the part affected, perfect. Iron was given, and in three weeks the natural movements of the part were restored. The nerves of sensation, however, now became involved, and complete anæsthesia of the whole hand and forearm made its appearance. This condition also disappeared in two months under the same treatment, sensation and movement being at last perfect. Here it can hardly be questioned that the spasm was the equivalent of the sensory paralysis. In a boy who was undergoing the operation of resection of the knee-joint, and had been rendered completely anæsthetic by chloroform, I observed the limbs in a state of marked rigidity, the arms more so than the legs. Here sensory paralysis coincided actually with spasm. A case is recorded by Plagge where a strong, healthy soldier, aged 25, after eight hours' march, when the temperature was 63°, fell down suddenly, and lay pulseless, pale, and without consciousness, with half-flexed limbs and closed jaws. Stimulants were given, and consciousness soon returned, and the rigidity ceased; but the patient could not move, and complained of severe pain in the back. Examination of the spinal column brought on immediately the most violent tonic contractions, without trismus or loss of consciousness, accompanied by the most frightful pains. An attempt to put the patient into a conveyance brought on the paroxysm afresh. Twenty ounces of blood were taken, and morphia administered, whereupon the spine ceased to be painful. After a short journey the spasmodic contractions returned, but yielded to cold applications to the loins and stimulation of the extremities. The next morning the patient was quite well. Plagge entitles this a case of hyperæmia of the cord, but it seems to me to have much more claim to be regarded as one of exhaustion with consecutive nervous disorder. Had active hyperæmia of the cord really existed, recovery could not have ensued so rapidly. The primary sudden syncope, the severe pains, and spasms, are much more consonant with the view of a tetanoid than of an inflammatory affection. It is most probable that the above was an instance of the malady termed by Trousseau tetanilla, which is induced by diarrhœa, fevers, lactation, and cold, and is treated advantageously by venesection and sedatives or quinine. In the description given of this disorder, it is stated that the intermittent contractures are usually preceded and accompanied by loss of muscular power; the hands, though half flexed, cannot be completely closed, and the tactile sensibility is also much impaired—in fact, both in the causes and phenomena of this neurosis there is an evident affinity indicated between spasm and paralysis. I remember when taking my first lessons in aquatics, with youthful enthusiasm, that my fingers for some time afterwards were contracted, and retained the position they had taken while grasping the oar—a contrary result to what one would have expected from the prolonged fatigue of the flexor muscles. A friend tells me he experienced the same, and so I suppose did the Israelitish worthy who arose and smote the Philistines until his hand was weary and *clave* unto his sword. In a case recorded by Mitchell, severe spasms of the facial muscles, cervical and lingual, were found to depend on the presence of carious teeth. This cause would most commonly have given rise to neuralgia, which, as we have just seen, has decided affinities with paralysis. If Earle records a case in which trismus was cured immediately by the discharge of a tapeworm, a case I have cited in Lecture II. proves that the same cause may give rise to facial paralysis. If we pass from tonic spasm to clonic, we find still more frequent indications of its affinity to paralysis. It may be sufficient merely to allude to choreic affections, which often contain quite as much of one morbid element as of the other. The same is exemplified in disorders of the internal organs depending on debility. I

met a medical friend some years ago in the street, who was looking pale. Said he to me, "My solar plexus is worn out; I want a new one. I am not ill otherwise, but I can't keep any thing on my stomach. I have not been out of London more than a day consecutively for six years." Here was surely a state essentially depending on nervous paresis, but presenting phenomena of irregular muscular action. Such cases are sometimes cured by strychnia—a fact which further illustrates their true nature.

The preceding observations appear to me to afford considerable evidence that spasmodic contraction is a very different thing *quoad* innervation from voluntary, and is a morbid mode of action related to paralysis, much in the same way as neuralgia is to anæsthesia. It is, of course, impossible to state in what respect the molecular condition of a nerve-cell which gives rise to a spasm differs from that which gives rise to paralysis. The constituents of the living tissues are not accessible to our microscopes; and, if they were, it is by no means certain that we should be able to distinguish any visible deviation from their healthy appearances in conditions of mere functional derangement. It is conceivable that some such changes in the nervous currents as occur during polarisation in luminous undulations may be the cause of neuralgia and spasm in the sensory and motor nerves respectively. I am utterly disinclined to enter into barren speculation, but it seems to me by no means such to endeavour to form a just estimate of the *nature* of the morbid actions which come before us, and to ascertain in what direction they differ from the healthy. We may be quite unable to acquire a full and complete knowledge of a disease, but we may, nevertheless, obtain very practical and useful knowledge respecting it. Thus we may never find out precisely what malaria is, or be able to demonstrate exactly in what way it acts upon the frame; but it is a highly practical and important point to know that it enfeebles and lowers nervous energy, and that most of its morbid effects can be explained on this view of its operation.

It seems to me an established point that the *quality* of nervous action differs greatly in different instances of disease, and that we cannot regard it as either simply increased or diminished in energy. This does not seem to me to have been always kept in view. In the motor nervous apparatus we have the state of healthy vigorous action replaced by tonic or clonic spasm, by tremors, choreic jerkings, and finally by paralysis, all without organic lesion. Muscular contractions, which our common persuasion holds, and I think correctly, to be an evidence of existing nervous power called into play, are most evidently not in all cases produced by the same *kind* of nerve action. Many actions might indeed be classed as *actions of weakness*, in apposition to actions of *strength*. The depression of the jaw in yawning when we are weary and languid is surely, *quoad* nervous power, not the same kind of action as that which occurs when an hungry man is eating, and the jactitations of chorea are not prompted by the same kind of nervous impulses which direct the strong steady strokes of an oarsman rowing a wager. The movements of chorea are usually readily controlled by a steady grasp of the agitated limb; but I had recently under my care a girl whose arm I was unable to keep still by all the force I could exert with both my hands. At the same time the legs were weak and tottery. Here the morbid condition of the lower part of the cord must evidently have been different to that of the upper. In some epileptic attacks there is much more tetanic rigidity than in others, yet we cannot think that different nervous centres are affected in the one case and in the other. In the sensory apparatus we have gradations from healthy sensation by hyperæsthesia, endless varieties of dyæsthesia and neuralgia, down to complete anæsthesia.

Without recognising the different quality of nervous action in different instances, it would be impossible to explain the great varieties we find in the action of certain medicines. This remark leads us again to consider whether, while we view spasm as the result of a particular kind of action in nerve cells, there may not be varieties of this special mode of action itself, something of a more sthenic, others of a more asthenic character. Certain cases of tetanus are beneficially affected by aconite, and show a very remarkable

power of resistance to this potent agent; others are not at all relieved by it. The same may be said of nicotine. Conway records two cases of traumatic tetanus cured by the exhibition of tartar emetic in large doses, while in other instances free stimulation and tonics appear to have been successful. The spasm of asthma is sometimes markedly relieved by depressants, as emetics and tobacco, sometimes by free doses of alcohol and ether. Some cases of chorea are much benefited by antimony, some by sedatives, some by tonics. The above-mentioned facts seem to me to indicate plainly that all spasmodic actions are not of the same quality, and that the same sort of difference prevails among them as there does between instances of hyperæsthesia and neuralgia. To recognise this is, I think, important, because it does away at once with any idea that the disorders of which spasm is the characteristic element are to be treated in any unvarying manner, even when we have arranged them in their classic groups. Nothing impresses me more as I grow older in practice than the manifoldness of disease, and how insatiable is the variety which Nature exhibits in her deranged as well as in her healthy aspects. We are too prone to imagine that where we see an outward show of resemblance there exists an internal and real identity; and this idea is hard to displace, though it be negatived again and again by the testing action of remedies. I will now attempt to apply the above views respecting neuralgia and spasm to a disease respecting the true nature of which authorities are by no means agreed—I mean angina pectoris. Dr. Stokes says, "If we inquire into the nature of a paroxysm of angina, two completely opposite views present themselves. Is it to be explained by spasm of the heart or referred to diminished action?" Now, the position I would take is that these views are not completely opposite; that spasm of the heart is much more akin to paralysis of the organ than to healthy contraction. The latter is unattended by pain, or indeed by any feeling, except it be a certain indefinite sense of "wohlsein," and what we may term wholeheartedness. The former is essentially and distressingly painful, and attended with that sensation of imminent peril of life which is characteristic of all, even of minor, cardiac dysæsthesia. The pain I regard as the more constant and essential phenomenon, and the muscular spasm as a secondary and varying. It is of course possible that death might result during an attack from closure of the cavities in tetanic contraction; but though Lussana is inclined to adopt this view, the evidence in favour of it seems to me but scanty. There are only three observations in which the heart is stated to have been decidedly contracted after death, and it is reasonable to conclude that in many of the cases where the muscular tissue has been found more or less diseased, the condition of the cavities was very different. But even could it be shown that the fact was otherwise it would be of no great weight, for it seems most probable that the post-mortem condition of the heart is determined in this disease, as we saw that it was in peritonitis and in similar states, by the amount of contractile energy possessed by the cardiac muscle. If this is great, the cavities close in a kind of rigor mortis, the contractility of the fibre outliving the inhibitory influence of the neuralgic pain; if it is the reverse, as one may conclude where the organ is found thinned, softened, or fatally degenerated, the cavities will remain more or less open. The treatment found most beneficial in angina is evidently such as contemplates a stimulating and invigorating rather than a sedative effect. Opium is, indeed, to be given in tolerably full doses; but this is for the purpose of counteracting and quelling the pain, and not of producing any narcosis of the nervous system. Stokes seems to be averse from employing this remedy when the heart's action is very weak. Stimulants are by general consent accounted our chief remedies in angina, and I am free to confess that I hold with those who look on opium as primarily and essentially (at least in moderate doses) a powerful though peculiar nervine stimulant. The most practical point for consideration in all cases of angina is whether the breast-pang which threatens to, and sometimes actually does, arrest the heart's action, is of organic or non-organic origin. If we are obliged to consider that the former is the case—that the neuralgia has its starting point in a nervous

filament pressed upon by a patch of calcareous deposit in the coats of the aorta or coronary arteries—the prospect is of course very unfavourable. If, on the other hand, we can hope that some existing rheumatic, gouty, or malarious disorder has produced the neuralgia, we may be more hopeful, though I have known a patient die in whom an autopsy demonstrated the absence of any organic lesion.

I will now enumerate the principal conceptions which I have endeavoured to lay before you in these lectures.

(1) That of the direct paresis of a nerve or nervous centre as the result of the action of some imponderable influence.

(2) That of inhibitory paresis where the result is produced by some morbid irritation, the vasomotor nerve centres seem to be more liable than others to suffer in both these ways.

(3) That of the influence of nerve-prostration in altering the texture of capillary vessels at the same time that it dilates arteries, thus giving rise to extravasation of blood in greater or less quantity.

(4) That of the paralytic character of neuralgia and hyperæsthesia.

(5) That of the morbid affections of the nervous centres being very analogous to those of the nerves.

(6) That of the affinity between spasm and paralysis.

I am well aware that the grounds which I have assigned for the views above enunciated are insufficient to demonstrate their truth, and when I compare my own work in these matters with the long laborious researches of others, I feel very conscious of short-coming. Still, it is to be remembered that when we come to stand at the bedside, we often feel the want of more than the bare facts which we can ascertain by observation; we need to have also ideas appropriate to the facts, and which may enable us to interpret their meaning aright. I am not without hope that these lectures may have done something to supply such conceptions, or to make them more familiar, and if this be the case I shall be well content.—*Med. Times and Gazette, Feb.* 17, 1866, *p.* 167.

20.—ON THE CAUSES, DIAGNOSIS, AND TREATMENT OF FUNCTIONAL NERVOUS AFFECTIONS.

By Dr. C. E. Brown-Sequard, F.R.S., &c.

One of the most important of the recent advances in physiology and pathology consists in the demonstration that the nervous conductors of the various kinds of sensitive impressions and of the reflex phenomena, and also for the transmission of nervous force to muscles, blood-vessels, &c., are absolutely distinct one from the others as regards their functions. Leaving aside the nerve-fibres of the brain, some of which have functions altogether different from those of other parts of the nervous system, I have ascertained that, besides the four distinct kinds of nerve-fibres in the higher senses, there are at least eleven distinct kinds of nerve-fibres in the spinal cord and in the cranial and other nerves. The following table shows what are the distinct functions of these eleven kinds of nerve-fibres. They are—

1st. Conductors of impressions of touch.
2nd. " " tickling.
3rd. " " pain.
4th. " " temperature.
5th. " " contraction (muscular sense).
6th. Incito-motor conductors.
7th. Incito-nutritive and secretory conductors.
8th. Voluntary motor conductors.
9th. Involuntary motor conductors.
10th. Vaso-motor conductors.
11th. Nutritive and secretory conductors.

I hardly need to say that the number of distinct nerve-fibres is probably much greater than is shown by this table; but the demonstration of the dis-

tinction of other kinds of nerve-fibres (such as those serving to sensations of hunger, thirst, pressure, or to voluptuous sensations, &c.) is not yet sufficient.

Almost all the symptoms of functional (and I might say also of organic) nervous affections take place through one or other of three modes of alteration of the properties and functions of the fifteen kinds of conductors above-named. Those three modes consist in: 1st, a diminution or loss of power; 2nd, an increase of power; 3rd, a morbid state producing a great variety of phenomena. In the nine kinds of conductors serving for the transmission of senitive impressions, the first of these three modes of alteration constitutes *anæsthesia*, the second *hyperæsthesia*, and the third is the cause of *morbid sensations* (including the so-called *referred sensations*).

I will not enter into details here about the kinds of nerve-fibres which I call incito-motor, incito-nutritive or secretory, or about those which I simply name nutritive or secretory; but as it would be impossible to understand the future lectures of this course without some knowledge of the properties and functions of these nerves, I will give, as briefly as possible, a few notions concerning their physiological and pathological history.

1st. *Incito-motor nerves.*—These are the well-known excito-motor nerves of Marshall Hall. A great many facts prove that they are absolutely distinct from the sensitive nerves. All the reflex movements of the muscles of organic or animal life (including the excretory ducts of glands, the blood-vessels, &c.) originate through an irritation transmitted by these incito-motor nerves.

2ndly. *Nutritive and secretory nerves, and incito-nutritive and secretory nerves.*—That more appropriate names should be given to these nerves, I will not deny; but so long as their real mode of action remains somewhat doubtful, I think it is better to call them by names which only imply what we positively know of them—i.e., that they are agents of modification of secretion and nutrition. The *secretory and nutritive nerves* are in many respects the antagonists of the *vaso-motor nerves*. While these last nerves, when put in action, produce a contraction of blood-vessels and all the phenomena which I have shown to ensue from that contraction (such as diminution in the quantity of blood, a diminution of sensibility and other vital properties and of secretions), the nutritive and secretory nerves, on the contrary, when put in action, occasion a dilatation of blood-vessels and all the phenomena that ensue from it, as shown first by experiments of Czermak and of Cl. Bernard. I proposed long ago (in my lectures at the College of Surgeons in 1858) an explanation of the action of these nerves, which becomes more and more probable with the frequent discovery of new facts concerning these nerves. The explanation is, that the secretory and nutritive nerve-fibres act upon the tissues so as to increase their chemical interchanges with the blood, and that, in consequence of this increase of chemical interchange, blood is more attracted to the part, and, as an effect of this greater afflux of blood, there is a dilatation of blood-vessels. In many of the future lectures I will mention phenomena due to this dilatation of blood-vessels from an irritation of this peculiar kind of nerves. I will only add now to what I have already said on this subject, that it is chiefly by a reflex action that these nerves produce their peculiar effects. The redness, the congestion of parts attacked with neuralgia or of neighbouring parts, the secretion of tears when the eye is irritated or the skin of the face pinched, the secretion of mucus from the nose, or from laryngeal, trachial, or bronchial mucous membranes, or the diarrhœa produced by cold, and an immense number of facts in which there is a congestion or a secretion as a result of an irritation of cutaneous or other nerve fibres, are all effects of the peculiar influence exerted by secretory and nutritive nerve-fibres on glands and other tissues.

These nutritive and secretory nerve-fibres are, as I will try to prove in future lectures, the agents of production of inflammation, suppuration, and ulceration, by a reflex action. They are the channels of production of meningitis, of encephalitis or myelitis, when those inflammations are due to cold, to a burn, or to a visceral affection. They also have a great share in the

production of many functional nervous affections, and particularly tetanus. Besides, through the agency of these nerves much may be done for the treatment of nervous affections, as I will show hereafter.

The incito-secretory and nutritive nerve-fibres are the incident or centripetal nerve-fibres which, by a reflex influence, act upon the centrifugal secretory and nutritive nerve-fibres. Their distinct exi-tence is not fully demonstrated, but many facts render it extremely probable.

The causes of functional nervous affections can be divided into several groups. 1st. An irritation by worms, by teething or decayed teeth, by cold, by a burn, a wound, an inflammation, a neuralgia, &c., of centripetral nerve-fibres (the incito-motor, the incito-nutritive, and some others). 2nd. An alteration in the quantity or the quality of blood. 3rd. Both the two preceding kinds of causes coëxisting together, as in cases of typhus fever, of variola, of diphtheria, of uræmia from disease of the kidneys, &c.

I will not now insist on the mode of action of these causes, as that will be better done in treating of each nervous affection separately; but there are a few general remarks which will be in their proper place in this introductory lecture. There are two general rules which particularly deserve attention :—

1st. *The same peripheric cause of irritation acting on the same centripetal nerve may produce the greatest variety of effects, including every functional nervous affection or disorder.* This is well illustrated by the effects on the eye of a neuralgia of the infra or supra-orbital nerves. There are cases showing: 1st, spasm of the sphincter of the pupil; 2d, mydriasis; 3rd, spasm of the orbicularis palpebræ; 4th, paralysis of the orbicularis; 5th, paralysis or spasm of one or several of the muscles of the ocular globe; 6th, photophobia; 7th, amblyopia or amaurosis; 8th, congestion or inflammation of the conjunctiva or of other parts of the eyes; 9th, diminution or increase of the secretions of the lachrymal and other glands; 10th, a cataract; 11th, a glaucoma.

Other instances, though less striking, might be adduced. I will only mention what we know as regards the effects of cold air on persons coming out of a theatre. One may be attacked with a sore throat, a second with ophthalmia, a third with enteritis, a fourth with nephritis, and many others with any other visceral inflammation; while one may be attacked with a facial paralysis, and others with almost every other partial paralysis, or with chorea, with contracture, with meningitis, or an inflammation of some part of the nervous centres. It is true that here it is not the same part of the skin which is acted upon by the sudden lowering of temperature; in most of such cases, however, it is the front part of the chest and the neck that are subjected to the influence of cold air.

It is probable that the great variety of phenomena following an irritation of centripetal nerve-fibres depends on differences of excitability of some parts of the nervous centres in different individuals. I have examined carefully, on a large number of men, the effects of tickling the sole of the foot, and found that these phenomena differed considerably in different individuals. In one, laughter predominated; in another, involuntary screaming, or shedding of tears, or jerks either in the irritated limb or in both lower limbs, or a general trembling, or a spasm of the diaphragm, or an almost tetanic rigidity of the irritated limb. I need not add that in some individuals there was hardly any effect produced when, being prepared for the irritation, they fought against its influence—a fact which, with many others, shows that by an effort of the will we sometimes can, at least in a certain measure, prevent the production of reflex phenomena.

I will not say more at present on this subject, leaving for other lecturers the demonstration that it is through a real reflex action (*i. e.*, through a mechanism similar to that of reflex movements) that the morbid effects of an irritation of centripetal nerve-fibres take place.

2nd. *The degree of excitability of the different parts of the nervous system may increase or decrease considerably in the same person under the influence of various causes.*—This proposition is of so great an importance in the diagnosis and treatment of nervous affections, that it is only by the light it throws on many otherwise obscure cases that we are enabled to recognise their nature,

and to apply the proper treatment. Many parts of the nervous system that are completely, or almost completely, inexcitable in a healthy condition, become excitable, and sometimes in a wonderful degree, under the influence of several morbid causes. Amongst these causes, I will point out the grey matter of the spinal cord, and the nerves of the tendons, aponeuroses, dura mater, periosteum, bowels, bladder, kidneys, and some other viscera. Many causes, very different one from the others, may increase the excitability of the various parts of the nervous system. I will mention here only the principal of these causes: 1st. I have found that muscles, nerves, and the spinal cord become more excitable when laid bare, and particularly when the air in contact with them is richer in oxygen than the ordinary atmospheric air. 2nd. A congestion or an inflammation will increase the excitability of nervous tissues everywhere, but nowhere so markedly as in the grey matter and some parts of the white columns of the spinal cord, which, in consequence of that increase, will become able to produce referred sensations of pain, of cold or heat, of tickling, &c., and phenomena due to an irritation of motor, vaso-motor, and nutritive nerves. 3rd. An afflux of blood, such as occurs merely by gravitation, or after a section or a paralysis of the sympathetic nerve, or a lesion of the spinal cord, the medulla oblongata, or the base of the brain, will also increase the excitability of peripheric nerves in the parts where this afflux takes place. 4th. Certain remedies or poisons (strychnine particularly) will increase the reflex excitability of the spinal cord to a wonderful degree; while others, such as atropine, chloroform, &c., will diminish it considerably. 5th. Certain diseases, such as tetanus and hydrophobia, will increase extremely the reflex excitability of some parts of the cerebro-spinal axis. 6th. A great loss of blood, anæmia, chlorosis, will also increase the reflex excitability of the nervous centres.

The increase of reflex excitability in cases of extreme debility, as in old age, or after a loss of blood, or other causes of insufficient nutrition, would be very difficult to understand if we did not know that the reflex excitability of the spinal cord can be increased under the influence of certain substances (strychnine principally) when no blood at all remains in the blood-vessels of that nervous centre, as proved beyond the possibility of doubt by Messrs. Martin Magron and Buisson. The excitability of sensitive nerves may also be increased when the quantity of blood is much diminished, as we often observe in fingers that have been exposed to cold air or cold water. It seems, from a review of all the facts I know bearing on this point, that certain substances contained in blood altered in quantity or quality will act on the excitability of nerve-fibres in the nervous centres, or in the nervous trunks and branches, so as either to increase it, as is done by strychnine (on the grey matter) and oxygen (everywhere), or to decrease it, as is done by carbonic acid, atropine, chloroform, &c.

To conclude what I wish now to say on the excitability of the nerves, I will only mention—1st, that I have ascertained that the excitability of the same nerve varies in different parts of its length, and to such a degree that in some parts the excitability seems nil or is very slight, while in other parts it is considerable; 2nd, that I have shown by positive experiments that the excitability of muscles, of nerves, and of the spinal cord may be very much increased while the force developed by the action of those parts is very small. For instance, atrophied muscles unable to contract with half the force shown by healthy muscles, will, however, contract under the influence of an excitation that will produce no effect on healthy muscles.—*Lancet, Jan. 6. 1866, p. 1.*

21.—ON THE MORAL TREATMENT OF FUNCTIONAL NERVOUS DISEASES.

By Dr. C. E. Brown Séquard, F.R.S., &c.

[In the treatment of functional nervous diseases it is of the utmost importance that the patient should have some serious aim before her—to prevent or to check nervous disturbances.]

The applications of this principle are, of course, very difficult and often impossible in certain neuroses; but in those cases in which any kind of serious work, not too fatiguing or exciting, is liked by the patient, she should be induced to do it. In cases of hypochondria, of hysteria, of chorea, and even of epilepsy, a great benefit can be derived from a serious employment of the mental and physical activity of the sufferer. How often have I not seen young epileptics kept in idleness (alas! by medical advice), and, having gained more or less of the vices it leads to, improve rapidly from having their minds occupied at regular hours, in the same way as healthy people of their age. The second principle of which I will speak now is that we must, in the interest of our nervous patients much more than in our own, give them confidence and hope in the treatment we recommend. In hysterical and all nervous complaints allied with it, and also in hypochondria, and in several other neuroses, a great hope of cure will do much to work out the cure. No doubt you will say, How to give hope? I answer that the best means for that purpose is to have hope ourselves, and to express our hope with the accent of conviction. And as you would ask, How can we command hope in ourselves? I answer that the very knowledge of the truth of the principle I am now speaking of is enough to render one hopeful. I need not repeat that I am now speaking only of those neuroses in which the power of the mind upon the body is immense, and so much so that in some forms of these neuroses a sudden or almost sudden cure is not very rare.—*Lancet, Jan.* 27, 1866, *p.* 87.

22.—ON THE USE OF BROMIDE OF AMMONIUM IN THE SLEEPLESSNESS OF FUNCTIONAL NERVOUS DISEASES.

By Dr. C. E. Brown Sequard, F.R.S.; &c.

It is of the utmost importance to improve the sleep, which is generally so bad in patients attacked with a morbid increase of the reflex excitability. For this purpose an invaluable remedy has recently been discovered: it is the bromide of potassium. Excepting when pain is one of the causes preventing sleep (in which case opium, aconite, and belladonna should be employed together), I have found that this remedy has a most wonderful power to produce a quiet and refreshing sleep, without any drawback that I am aware of. I usually give to adults a dose of thirty grains of that salt a quarter of an hour before the last meal, and a second dose of from thirty to fifty grains at bedtime. In cases in which, without any nervous complaint, there is sleeplessness owing to some cause of cerebral excitement, as well as in all neuroses, excepting hydrophobia, tetanus, very severe cases of delirium tremens, and some forms of insanity, sleep is almost always induced by that remedy. In some cases I have found it necessary to increase the dose of the bromide, and to give also one grain or one grain and a half of codeine an hour before bedtime. In those affections in which the bromide of potassium is not powerful enough as a sleep-inducing agent, a warm bath of four, five, or six hours' duration is often successful.—*Lancet, Jan.* 27, 1866, *p.* 87.

23.—CASES ILLUSTRATING THE USE OF BELLADONNA AND BROMIDE OF POTASSIUM IN EPILEPSY.

Under the care of Dr. Broadbent, St. Mary's Hospital.

Dr. Broadbent has for some time been engaged in observing the effects of two remedies—belladonna and bromide of potassium—in epilepsy, in cases where no positive indication for treatment has existed in deranged function of abdominal or other viscera. The belladonna is given in the form of extract in gradually increasing doses till physiological effects—dilatation of the pupil, with dryness and redness of the fauces, &c.—become apparent. The quantity is then slightly diminished, so as to allow the unpleasant symptoms,

such as the sense of constriction of the throat, to pass away; but the pupil is kept dilated for about a month, when the administration is suspended for a short time, to be again resumed. No precise rule has been followed with the bromide. It has generally been given in some bitter infusion, with compound spirit of ammonia.

A few of the cases which have been sufficiently long under observation to furnish results are given, classified according to their probable causation, malformation, or organic disease of the brain, injury to the head, sexual irregularities, &c.

Organic affections of the brain.—E. T., an idiot boy, aged 11 years, partially hemiplegic on the left side, the limbs not contracted, the arm hanging, the leg dragging, had, when brought to the hospital on the 6th of April, 1865, three or four fits every day, and six or seven in the night. His mother had paralysis of the left facial nerve, which came on twelve months before he was born. She had a fall also about a fortnight before she was delivered of him.

The boy was ordered one-third of a grain of extract of belladonna three times, and then four times a day. On the 20th of April, half a grain, with the same directions. Considerable improvement followed; and in the week ending May 18th he had only three fits. On the 25th, however, he had been poorly all the week, did not walk so well, and had had fits every night. The pupils were greatly dilated. It was not considered prudent to go on with the belladonna, and the bromide of potassium was given in doses of six grains, increased on June 15th to eight grains. The fits at once diminished in frequency, and he improved in every way till June 13th, when he was worse than ever. The bromide was suspended for a time, and again resumed; but in October, when he ceased to attend, there was no marked improvement, though the fits were not so numerous as when he first began to attend.

A. H., when first seen, September 16th, 1863, was four and a quarter years old. He had nine or ten fits every day, the first having occurred when he was a year and a half old, and in it the left arm became paralysed and contracted, and had remained in this condition. He was well nourished. The head was large and remarkably long; narrow, projecting, and almost pointed posteriorly; flattened at the top; concave at the situation of the anterior fontanelle. He had an intelligent look, and was said to be talkative.

Belladonna was given in gradually increasing doses, and the fits slowly diminished in number till, on November 25th, he was said to be better than at any time for two years. On December 4th numerous fits occurred, and he was insensible for two days afterwards. There was now loss of power in the right arm as well as in the left; the arm could be moved, but it jumped. The belladonna was continued. The fits averaged seven a week till the 23rd, when he had seven, and on the 26th twenty-five. On January 20th, 1864, he had only had two fits in twelve days; then he was in fits for an entire night.

These alternations continued—the intermissions being at times so long as to give encouragement in the use of belladonna—till July 11th, when the bromide was given, at first in doses of three grains, and it seemed to have some good influence. The quantity was raised successively to five, six, and eight grains; and from the beginning of September, 1864, to the end of January, 1865, he had only occasional slight attacks, the weekly reports being "no fits," or one, two, or three (once seven), but "not to go off." He could also open the left hand, which had hitherto been firmly clenched, and seemed better in every respect. On February 3rd, however, and on the 16th and 17th, he had numerous fits. The dose of the bromide was increased on the 3rd to ten grains. On March 31st he had again severe fits, which affected his speech; also on April 12th, after which he was deaf for a time. Early in May the remedy was suspended for ten days, but, the fits returning with great severity, it was resumed. He again became better, and ceased to be under Dr. Broadbent's care in June.—*Lancet, Jan.* 27, 1866, *p.* 92.

24.—ON THE TREATMENT OF EPILEPSY.

By Dr. C. E. Brown Sequard, F.R.S., &c.

[Probably the principal mode of action of the following remedies is through the irritation of the excito-motor nerves of blood-vessels, and excito-nutritive and secretory nerves.]

1st. *Ligatures, pinching, rubbing, and circular blisters.*—When I first began to make use of ligatures in epilepsy I had hardly any other than theoretical objections against the view that in that convulsive affection ligatures serve only to prevent the passage upwards of an irritation travelling from the periphery towards the nervous centres. I soon saw several cases similar to a few already known, in which the cause of the aura was clearly in the nervous centres, and in which, however, ligatures acted as marvellously in preventing attacks as in cases in which an aura had undoubtedly its cause in a peripheric part. When the extremely various sensations which accompany an aura, like the aura itself, have their only cause in the nervous centres, as it was in the cases I have just mentioned, it is quite evident that there is nothing going from the periphery to the nervous centres, and as evident that ligatures in such cases must act by some other means than by preventing the passage of something. This first point being clearly established, I found that ligatures, in cases of an aura from a centric origin, acted with greater efficacy when they suddenly induced powerful irritation of the nerves of the skin than when they were applied gradually and slowly, either tightly or not. After having made this step, I ascertained that it was so even in cases of an aura starting distinctly from the periphery. I found afterwards that in both kind of cases, pinching, pricking, striking, or rubbing the part which is objectively or only subjectively the place of origin of an aura, may prevent a fit of epilepsy or of hysteria, or simply of local or general convulsions. I was led therefore to look upon ligatures in those cases as acting chiefly as a means of irritation of the nerves of the parts on which they are applied, irritation producing a favourable change in the nervous centres. The following results I have subsequently obtained:—1st. That it is not necessary to apply an irritation (by a ligature, pinching, &c.) on the very limb from which an aura seems to start, as the same means applied elsewhere may succeed: but the chance of success is much greater by the former than by the latter way. 2nd. That a constant or a frequent irritation (by a blister, an issue, a seton, the actual cautery, &c.) on the place from which an aura seems to start may not only prevent fits, but, by some change of nutrition locally (if the aura is really of peripheric origin) and in the nervous centres, may reduce or even destroy altogether the tendency to fits, and lead to a complete cure. 3rd. That, as a circular ligature may procure a temporary good effect, so a narrow *circular blister* applied all round a limb, a toe, or a finger, or a circular cauterization with a white-hot iron, may cure epilepsy, hysteria, neuralgia, &c., in cases with a distinct aura. 4th. That even in cases in which there is no known aura, felt or unfelt, ligatures, pinching, and other means of irritation may prevent fits when applied on persons expecting a fit. Before I pass to another subject, I ought to say that for a long while a positive fact has been known—not generally, however—which shows that ligatures applied to the limbs when a fit of fever and ague is about to take place may prevent the fit. As malarial poison produces fever by a peculiar influence upon the spinal cord, so the success of ligatures against fever and ague is due to a favourable change in a nervous centre, induced by a peripheric irritation of incident nerves.

2nd. *Cold.*—Ice is the most powerful means we possess to produce a reflex contraction of bloodvessels. Before a positive demonstration was obtained on this point, it seemed already extremely probable from the following well-known facts: that ice applied on the abdomen or on the back, or in the vagina and rectum, often stops a hemorrhage from the womb: that, applied to the head, it will do some good in cases of congestion or inflammation of the brain or its meninges, or in cases of symptomatic or idiopathic headache; that it may stop a hæmoptysis when applied to the chest—a hemorrhage from the

bowels, the stomach, or the kidneys, when applied to the abdomen or the loins; that it may be employed with marked benefit against an inflammation of the viscera or serous membranes of the chest and abdomen, and especially against metroperitonitis, as shown particularly by Kiwisch and Béhier; that, applied all along the spine, it may cure tetanus, and be useful against spinal meningitis. These facts gave a great probability to the view that ice acts as an irritant of incident nerves, and through the influence of this irritation on the spinal cord or base of the brain produces a reflex contraction of bloodvessels; but, so far as I know, there was no positive fact establishing this peculiar influence on bloodvessels, until the experiments I made about fifteen years ago with my friend Dr. Tholozan clearly showed this important influence of ice. We found that dipping one hand into water at freezing-point produces in the other hand considerable contraction of bloodvessels and a corresponding diminution of temperature. This fact, of which I shall have to speak again in another lecture, leaves hardly a doubt as regards the mode of action of ice in the cases in which it is daily employed with the view of producing a remote effect. Recently, Dr. Chapman has had the idea of applying ice to the spine, with the purpose of paralyzing the vaso-motor nerves of the limbs or of the head. He states that the application of ice to the lumbar region of the spine produces a dilatation of the bloodvessels of the lower extremities, and all the consequences of a greater afflux of blood: and that a similar application between the shoulders produces the effects of the section of the two cervical sympathetic nerves. Whether he be right or wrong in his explanations, he at any rate relates facts which deserve to attract the attention of the profession. We are not frequently enough successful in our treatment of neuroses to neglect means which, according to his statements, have cured so many cases of epilepsy or of other nervous affections. It is to be hoped, therefore, that physicians of large hospitals will give a fair trial to the means he proposes, and ascertain the real value of cold applications to the spine in the treatment of neuroses. Before dismissing this subject, I will mention that a patient of Mr. Ernest Hart has been cured of epilepsy and progressive atrophy of the optic nerve by his applying ice to the spine, and that the same patient had only derived a slight benefit from the use of the valerianate of atropia under my care. This would be the place to speak of hydrotherapy, if I had time to treat of all the therapeutical means. I will only say that an important addition to the proofs of the reflex influence of cold water on bloodvessels has been obtained by Dr. L. Fleury, who, by careful measures, has ascertained that, after the application of a cold douche on the skin, a congested spleen or a congested liver will sometimes diminish in volume much more than had been stated by other practitioners. Several interesting facts lately observed by a talented young physician, Dr. Beni-Barde, give a confirmation to the view that the therapeutic influence of hydrotherapy is due to a reflex action on the internal organs from the excitation of the skin by cold water.

3rd. *Actual cautery and other applications of heat.*—Jobert de Lamballe, Vallieix, and others have exaggerated the advantages of the so-called transcurrent cauterization against neuralgia; but there is no doubt that this means is one of the most valuable in that affection, and is very useful against other neuroses. I may say that it is not at all necessary to produce a burn that will give a great discharge. If the iron employed has a smooth surface; if, besides, it is applied only at white heat, and passed quickly over the skin with a gentle pressure, it hardly gives pain. The epidermis and a very thin layer of the skin dry up and fall off within two or three days, leaving a reddish mark, which disappears before very long. This means I have employed with great advantage in a case of angina pectoris, and in many cases of epilepsy and neuralgia, and with some benefit in cases of contracture, of torticollis, of paralysis agitans, &c. The application of heat in many other ways (by Mayor's hammer, for instance) is very useful in a great many cases of nervous affections. In a case of coma with convulsions, due to a complete arrest of the urinary secretion after an attack of renal hæmaturia, in 1851, Dr. Tholozan and myself, after twenty-four hours of unsuccessful treatment, decided to

apply heat on the skin of the loins and sides of the abdomen. A large silver spoon was dipped into boiling water, and was applied on eight or ten places. To our surprise and delight, we soon found the respiration improving, and in less than a quarter of an hour the convulsions ceased, and the patient came to his senses and passed a little water. The return of the urinary secretion had saved him. No doubt the irritation of the skin had acted by a reflex action on the secretory nerves of the kidneys, and produced the secretion of urine.

4th. *Alternative applications of cold and heat.*—Led by the well-known powerful influence of alternative immersions of newly-born children into cold and warm water in restoring respiration, I have often applied, with more or less good effect, cold and hot water to the spine, with a view of producing a contraction of bloodvessels of the spinal cord. Whether these vessels are already somewhat contracted, as is most likely the case in reflex paralysis or reflex anæsthesia, or dilated, as is often the case in hysterical paraplegia, some good may be obtained in these two opposite conditions from the use of cold and hot applications, owing to causes which I will explain in another lecture. I have found alternative applications of ice and hot poultices very useful against neuralgia. Dr. Ramskill has obtained some benefit from alternative immersions of a paralysed limb into very cold and hot baths.

5th. *Cauterization of fauces and larynx.*—A man of some talent, who unfortunately allied eccentricity to a genuine love of his profession, Dr. Ducros, used to treat many nervous and other complaints by the application of ammonia to the fauces. More rational researches have shown that a cauterization of the fauces, and still better, of the mucous membrane of the larynx, is of the utmost value in the treatment of whooping-cough, of several other spasmodic affections of the larynx and the bronchial tubes of diphtheritic or hysterical paralysis of the larynx and pharynx, and in that form of epilepsy which is due to laryngismus.

6th. *Other kinds of irritation of sensitive or incident nerves.*—Many applications on the skin have a great power in changing the state of nutrition of the nervous centres or of the peripheric parts attacked with pain, spasm, &c. Of the means of treatment of this kind that have recently been proposed, I will only point out the use of nitrate of silver and of sulphuric acid on the skin in cases of neuralgia; the use of the continuous galvanic current, as proposed by Remak in many nervous affections, and the singular, but certainly sometimes successful application of sulphuric ether in the external ear against functional deafness.—*Lancet, Feb.* 10, 1866, *p.* 139.

25.—ON THE USE OF CHLOROFORM IN EPILEPSY AND HYSTERIA.

By Dr. C. E. Brown Sequard, F.R.S., &c.

[Besides being useful as a means of preventing an expected attack of hysteria or of epilepsy, inhalations of chloroform are useful in three particular kinds of cases.]

1st. When the comatose state due to uræmia or following a violent attack of epilepsy is frequently interrupted by tonic or clonic spasms of the respiratory muscles, increasing the profoundness and duration of the coma, I have employed inhalations of chloroform without any appearance of greater harm than is usual in surgical cases, and with the beneficial result of shortening the duration of the stupor by preventing the spasmodic contractions or diminishing their violence. This mode of treatment is known to be particularly useful in almost analogous circumstances—i. e., in cases of puerperal convulsions. In many cases of epilepsy in which a large number of attacks usually occurred in a day or a day and a half (such a series of seizures being followed by complete freedom from fits for three, four, or six weeks), and in which also a comatose state usually appeared soon after a few attacks had taken place, I have found the benefit due to chloroform to be very great, the fits being much lessened in frequency and violence, the comatose state rendered shorter and

less profound, and the period of great mental aberration after the cessation of the coma considerably diminished.

2nd. In the maniacal or delirious state due to hysteria, epilepsy, or uræmia, and also in cases of aggravated chorea preventing sleep, chloroform is of immense service, particularly if morphia or some other somniferous remedy is given to the patient, either by the hypodermic method or by injection into the rectum, as soon as chloroform has produced sleep. Dr. Briquet states that nine times out of ten he has stopped attacks of hysteria by inhalations of chloroform. I have not been quite so successful as Dr. Briquet in cases of hysterical convulsions, but attacks of delirium and mania due to hysteria are almost always quickly brought to an end by chloroform.

3rd. In the case of a gentleman who had pretty regular weekly attacks of epilepsy, I once employed chloroform by inhalation, almost without interruption for two or three days successively, with the object of preventing the expected fit or fits. It was of the greatest importance in that case to prevent a fit, as the patient in a preceding attack had fractured and dislocated one of his arms. The inhalations of chloroform saved him from the expected attack, and the callus had time to be formed before he had another fit. This patient was seen with me by Mr. Robert Dunn.—*Lancet, March* 10, 1866, *p.* 247.

26.—ON THE USE OF ICE BAGS IN CONVULSIONS AND IN CHOLERA.

By Dr. John Chapman.

[Dr. Chapman's theory is that cold and heat applied to the spine produce definite and constant effects. He considers that ice along the spine increases the general circulation, stops the cramp of voluntary and involuntary muscles, proves an effective remedy in epilepsy and other convulsive affections, cures sea-sickness, restrains the sickness of pregnancy, arrests diarrhœa, recovers patients from the cold stage of cholera, and, finally, promotes menstruation. On the other hand, heat along the spine lessens the general circulation, overcomes congestion in all parts of the body, lessens fever, restrains hemorrhage, and lessens or arrests the menstrual flow.]

First as to the power of ice along the spine to influence the spinal cord:—The late Dr. Todd, who entirely misconceived the nature of the influence on the general circulation which is exerted by ice applied along the spine, and who stated it to be exactly the reverse of what it is, testified that "cold has a powerful effect in depolarising the spinal cord. I have tried it," he says, "with great advantage in tetanus, in laryngismus, and in the convulsions of children.....In the cases in which I have tried it," he adds, "I have found it manifestly beneficial in diminishing the intensity of the spasms." [Unfortunately, Dr. Todd's false doctrine that ice along the spine "has a very powerful influence in diminishing both the force and frequency of the heart's action," precluded him from freely availing himself of this remedy.]

In the Medical Times and Gazette for March 12, 1864, there is a report of a case of "Emprosthotonos(?)," treated by Dr. James Edmunds by means of ice. The following is a summary of that report:—Jan. 21.—A married woman, 28 years old, healthy, intelligent, and free from flightiness or hysteria, went to bed quite well; on rising next morning at 7.30 her fingers were slightly benumbed; between 9 and 10 they suddenly turned in; the flexion became rapidly more intense; a cramp-like pain in the arms ensued; and soon the lower limbs from the knees downwards became affected in a similar way, but less severely. The symptoms continued to increase throughout the day, and at 10 p.m., when Dr. Edmunds first saw the patient, there was extreme and rigid flexion of her hands and arms; her head was so drawn forwards that the backs of the wrists were fixed close to her mouth; her spine was bent forwards, and her knees were bent and drawn upwards. Her forearms were hard and rigid, and the wrists were so fully flexed that her hands were forced open, and her legs were similarly affected, but in a much less de-

23.—SUBCUTANEOUS INJECTION OF NARCOTICS FOR NEURALGIA.

By Dr. JOHN W. OGLE.

Subcutaneous Injection of Aconitine and Morphia for Neuralgia.—Mary K., aged 22, became an out-patient September 11, 1865, suffering from neuralgic pain, which she had had for some weeks, down the back of the left thigh and in the muscles forming the left buttock. This was at times excruciating, and could not be connected with disease of any organ. She was, however, not a very robust person.

Dr. Ogle at once ordered the subcutaneous injection of $\frac{1}{40}$th of a grain of aconitine, and enjoined that she should come again to the hospital in three days' time, and have $\frac{1}{30}$th of a grain of aconitine injected. Valerian, steel, and quinine were also prescribed.

She experienced a certain amount of relief from the subcutaneous injection, but not so much as was expected; so that on the 18th half a grain of acetate of morphia was injected. After this she complained of feeling "very queer" for three hours, and subsequently passed a restless night, "sleeping and waking," as she said. The next day she felt "weak;" but for two or three days decidedly less pain was experienced. On the 25th the pain had returned very acutely, and therefore $\frac{1}{15}$th of a grain of aconitine was injected. At the end of a week she returned, saying she had received very great good from the last injection, and had been quite free from pain ever since. She was accordingly discharged, as being no longer in want of treatment.

Subcutaneous Injection of Atropia and Morphia in Neuralgia of the Thigh.—Emma G., a rather delicate and pale young woman, was admitted under Dr. Ogle's care on September 6, 1865, with acute pain down the back of the left thigh of a persistent character. There seemed to be no actual disease of any organ. It appeared that she had been previously in the hospital with a similar attack, which was benefited by subcutaneous injection of some sedative. Accordingly Dr. Ogle at once ordered $\frac{1}{30}$th of a grain of atropia to be injected beneath the skin of the thigh and a senna draught to be given. This produced no good result whatever, and then a draught with half a grain of morphia every night was prescribed, and in a day or two quinine in 7-grain doses twice a day and occasional pills with colocynth and hyoscyamus.

On the 18th Dr. Ogle ordered half a grain of morphia to be injected subcutaneously. This gave a considerable degree of relief, lasting a whole day, and on the 25th it was repeated. She quickly improved after the latter injection, and on Oct. 2 was discharged free from pain. It is right to state that she also had gone on with her quinine daily, and had been taking ordinary meat diet.

Neuralgia of the Face (Infra-Orbital Branch of the Fifth Cranial Nerve)—Relief from Steel, Quinine, and Subcutaneous Injection of Morphia, &c.—Anne V., aged 58, a widow, thin, and delicate looking, had for four months been subject to acute pain extending from below the right orbit to the middle of the upper lip, which was rather swollen. She became an out-patient January 9, 1865. This was wont to come on suddenly, lasting a minute and a half, as well during the night as day. The pain was described as being like scalding with hot water or violent pricking, and was brought on by lifting up the lipe with the finger, sucking, swallowing spittle, eating and drinking, &c. Very rarely the pain was also felt *above* the orbit. Moreover, all along the course of the pain the surface of the skin was *exquisitely hyperæsthetic*. There was a slight degree of drooping of the right upper eyelid, but there was no paralysis of the facial muscles, or of those of the arms or legs, &c. The pupils were natural. There were no teeth in the upper jaw, as they had been extracted or had fallen out from being decayed when young.

The patient thought that her pains began with "cold and great discharge from the nose." The mental powers seemed intact. For several weeks she was treated with steel and 5-grain doses of quinine. Belladonna ointment was applied to the painful part, but the patient thought that this made her

worse at the time; perhaps it was from the friction. On the whole she much improved, the pain still being described as "jarring," "pricking," "throbbing," but in the intervals of pain she was much stronger and more comfortable. As this appeared to be especially so after the use of night pills containing half a grain of acetate of morphia, the same quantity of morphia was injected (February 27) beneath the skin of the right cheek. This gave great temporary relief. She afterwards took quinine with four minims of the tincture of aconite. On March 20 one grain of morphia was injected subcutaneously. Since that time the patient has not presented herself at the hospital. —*Medical Times and Gazette, Feb.* 17, 1866, *p.* 174.

DISEASES OF THE ORGANS OF RESPIRATION.

29.—ON THE NATURE, CAUSE, AND TREATMENT OF TUBERCULOSIS.

By Dr. Horace Dobell, Physician to the Royal Infirmary for Diseases of the Chest, &c.

[The hypothesis set forth at the commencement of the following paper is the result of much study, and is one interesting alike from its novelty and its importance.]

Tuberculosis is due to defect in the action of the pancreas on the fat taken as food (especially the solid fat). The supply of properly prepared fat is cut off from the blood: 1, by the fats not being brought into a proper condition by the pancreas; 2, by loss of absorbing power in the small intestine, due to the contact of unhealthy pancreatic juice and of defectively prepared food with its mucous membrane. Thus the blood becomes deficiently and defectively supplied with fat-elements from the food; is unable to afford those required for direct combustion; does not replace those taken up during interstitial nutrition; but, on the contrary, takes up more to compensate the deficient supply from the food. This having gone on up to a certain point, the fat-elements of the albuminoid tissues are seized upon, and these tissues are minutely disintegrated in the process. This disintegrated albuminoid tissue is nascent tubercle; and this process of disintegration is tuberculisation.

Tuberculisation will take place first wherever the following combination of conditions is most marked.

1. Greatest activity of interstitial nutrition.
2. Smallest amount of fat-elements able to be spared by the tissues.
3. A double process going on, consisting of (*a*) ordinary interstitial nutrition in albuminoid tissue; (*b*) interchange of oxygen and carbonic acid, or carbonaceous matter through this tissue.

The tubercle thus formed may be allowed to remain on the spot where it is formed, constituting a *primary deposit at the point of origin*, and this will especially occur when formed under the three conditions just specified; or, it may not be allowed to remain on the spot where formed, but be at once carried away by the lymphatics in a minutely divided condition, and either arrested in the lymphatic glands or carried on into the blood to be deposited from it, constituting a *primary deposit distant from the point of origin;* or, having been primarily deposited in either of these ways, it may be taken up by the lymphatics and deposited in the lymphatic glands, or carried into the blood and deposited from it, constituting a *secondary deposit.*

In the *advance* of these diseased processes, any part of the body in which nutrition is going on in albuminoid tissue may become both the source and seat of tubercle; and any part, whether albuminoid or not, if supplied with lymphatics or blood-vessels, may become the seat of tubercle.

Pure tuberculosis commences when fats, properly acted upon by the pancreas, cease to pass in normal proportions into the blood.

Tuberculisation commences when albuminoid tissue is abnormally seized upon for its fat elements.

A secondary state is superadded when tubercle has been carried into the circulation, which constitutes tuberculæmia or tuberculous blood-poisoning.

Tuberculosis may stop short of the production of tubercle (tuberculisation), if the normal supply of fat, properly acted upon by the pancreas, be restored to the blood before the albuminoid tissues have begun to be disintegrated; and it will not return so long as the supply is kept up to a normal standard. Tuberculosis may stop short, after the production of tubercle, if the normal supply of properly prepared fat be restored; but, in this case, the tissues must have become so poor in fat-elements that it will be difficult to escape the formation of fresh tubercle in the defective tissues, while the reconstruction of normal tissues is going on. A very rapid and superabundant supply of properly prepared fat to the blood will, therefore, be required.

The balance of fat-elements in the blood and tissues having been restored, tuberculosis will cease; but tuberculæmia with all the difficulties due to the presence of foreign decomposable and absorbable matter in the tissues and blood, may remain.

In progressing tuberculosis, after the occurrence of tuberculisation, tuberculæmia is added to and combined with the original disease. An ordinary case of consumption is a combination of these three states.

If the second portion (*b*) of the third condition especially favouring tuberculisation—viz., "an interchange of oxygen and carbonaceous matters through tissues nourished by blood deficient in fat-elements"—can be provided for by supplying carbonaceous matters to the circulating medium of the part otherwise than by the normal processes and channels, a temporary protection from tuberculisation will thereby be given to the tissues through which the interchange occurs.

If this part were previously the one in greatest danger of tuberculisation, it will for a time give precedence to the part not so protected; *e.g.* the lungs may thus be protected by the introduction of oleine through the portal system, the small intestine and mesenteric glands taking precedence in the order of tuberculisation from the want of such protection.

But as nothing can permanently protect the albuminoid tissues from disintegration except solid fat properly acted upon by the pancreas, the protection by the means above described is only temporary; and, unless the normal conditions be soon restored, the previously protected part will become tuberculous at last. By these means, however, the succession in which parts ordinarily suffer may be changed; and if the normal state be restored in time, the part temporarily protected may permanently escape disease.

The defective function of the pancreas causing defective action upon fats may be temporary or permanent, and a tendency to such defect may be hereditary.

When the normal function of the pancreas had been restored, the mucous membrane of the small intestine may not have recovered its absorbing powers and thus the fats, though properly prepared by the pancreas, may not enter the blood.

Defect in the function of the pancreas may be produced:

1. By any cause which for a prolonged period greatly reduces its activity, by diminishing the normal demand for carbonaceous matters in the blood.
2. By the action on the nervous system of powerful or prolonged depressing influences.
3. By inflammatory and other abnormal conditions of neighbouring parts.
4. By prolonged loss of absorbing power in the small intestine by which the function of the pancreas is rendered useless. The normal function of the pancreas may be restored (within certain limits as to the duration of the existence of the cause of its defect) by means which make a healthy demand upon its active functions, remove the depressing influences from the nervous system, and permit the absorption of properly prepared fats by the mucous membrane of the small intestine.

This hypothesis having been constructed step by step to explain the phenomena of tuberculosis, I feel safe in asserting that there is hardly a well-established fact connected with the origin, course, terminations, and therapeutics, of tuberculosis, which cannot be more satisfactorily explained by this hy-

pothesis than by any other yet suggested. I have carefully compared all its parts with the best established views in modern physiology ; and, so far as my knowledge and judgment are competent to decide, there is nothing essentially inconsistent with these views in any portion of it. At the same time, I am quite aware that there are many important points in physiology, unavoidably involved in every theory of nutrition, whether normal or abnormal, on which our knowledge is, at the best, vague and unsatisfactory. All that can be expected, therefore, in this respect, is that conditions shall not be required by a theory of morbid nutrition which are forbidden by the knowledge we have of normal nutrition. I believe it will be found that I have studiously kept within these limits ; but, as I have already said, I am conscious of my deficiency in profound physiological and chemical attainments ; and I am, therefore, the more desirous that my views shall be examined by those who possess these qualifications, in order that their truth may be strictly tested.

It is not my intention, then, to enter into a discussion of the purely physiological and chemical details of this hypothesis, but rather to direct attention to some of the most striking points in which it is consistent with the clinical history and pathology of tuberculosis, and especially to those in which it reconciles and explains apparently incongruous phenomena.

In the first place, it is necessary to impress the importance of the position occupied by fat in the processes of life ; and I cannot do this better than by quoting the following statements by recognised physiologists, in which my own opinions are embodied.

"In considering fat as an important agent in the various phases of the metamorphosis of animal matter, we cannot, however, refer its action solely to mere contact or catalytic force ; but we are constrained to assume that it co-operates in the metamorphic action, and experiences metamorphoses, combinations, and decompositions." (Lehmann's *Chemistry*, vol. i, p. 269.)

Under certain circumstances and for a certain time, "animals can be maintained by a nourishment from which fat is entirely excluded" ; and, when the increase of fat in their bodies is in excess of the substances soluble in ether taken as food, "the formation of fat is due to a process of life." (Letter from Professor Liebig to the author, April 3, 1853.)

"In all the forms in which albumen naturally occurs, it is combined with fatty matter." (Kirkes's *Physiology*, 3rd edition, p. 12.)

"The usual function of fat is unquestionably, like that of starch or of sugar, to keep up the heat of the animal.... We require the combustion of all the non-nitrogenous constituents of food to enable us to account for animal heat.... But it is, nevertheless, a fact that fat is always present in healthy muscles ; and it is desirable to consider its relation to muscular action..... The experiments made by Bidder and Schmidt on starving cats, and by Bischof and Voit on a starving dog, throw light upon this subject.... When Bischof and Voit supplied their starving dog with fat, the waste of the body, as evidenced by the lessened amount of urea excreted, was diminished, because the fat supported the respiration, which before had partially to depend on wasting tissues. The fat cast over them a protective influence, and limited their waste to the support of their own dynamic functions ; and in this fact would seem to be the use of fat after it is stored up in the muscle. We allude to its chemical use ; for its mechanical advantage in lessening friction and its possible histogenetic employment in the formation of cells are not under consideration .....A man in ordinary health and activity wastes daily 1750 grains of dry flesh or 7000 grains of fresh muscle, which would contain 150 grains of fat .....We refer to fat distributed in and inherent to healthy muscles, and not to masses of adipose tissue.....The chemical use of fat deposited within muscle may be to protect it from the assaults of oxygen during repose. A muscle, when at rest, gives out carbonic acid, which is, no doubt, partly due to the oxidation of its effete particles, but also to the oxidation of fat.... Tissues may, and do, evolve heat by transformation when required to do so" (Dr. Lyon Playfair, *The Food of Man in relation to his Useful Work :* 1865.)

A part of the heat necessary to maintain a constantly elevated temperature

in birds and animals, "depends upon the *direct* combination of certain elements of the food with the oxygen of the air by the combustive process. The quantity of carbonic acid that is generated directly from the elements of the food, seems to vary considerably in different animals and in different states of the same individual....In man and other animals which can sustain considerable variations of climate, and can adapt themselves to a great diversity of habits, the quantity of carbonic acid formed by the *direct* combination of the elements of the food with the oxygen of the air differs extremely under different circumstances. It will serve as the *complement* of that which is formed in other ways." (Carpenter's *Principles of Physiology*, 6th edition, p. 265.)

"The fatty matters of the blood are obviously destined to furnish the contents of the adipose and nervous vesicles, whilst their presence seems also to be required in the early stages of the production of cells generally. One of the principal sources of their expenditure, however, is that combustive process by which the heat of the body is maintained; and the amount deposited in the tissues as fat may be looked upon as the surplus of the quantity ingested that is not thus consumed.....The question of the capability of animals to produce fat has been much disputed.. There seems, however, no reason for doubting that it may be generated, not only from the hydro-carbonaceous constituents of the food (sugar and starch), but also from the splitting up of the albuminous compounds." (Carpenter, *op. cit.*, p. 341.)

"It is clear that fat exerts a protective influence over the albuminous tissues, sparing their consumption or oxidation by its own greater affinity for oxygen.....When fat is added to the food, its direct combustion takes up the oxygen, and prevents its action on the nitrogenous tissues." (Carpenter, *op. cit.*, p. 332.)

In the second place, I must point out that, although reliable observations on the blood in phthisis are very scarce, those which we have are consistent with my hypothesis. "In this disease, the saponified fatty matters of the blood decrease perhaps more than in any other." (Becquerel and Rodier's *Animal Chemistry*, Speers's translation, 1857.)

In the next place, all reliable analyses and observations of the nature of tubercle itself give results consistent with my statement, that it is albuminoid tissue disintegrated by the combustion of some of its fat elements.

From the analyses of Scherer, "tubercle may be regarded as protein from which five atoms of carbon, one of hydrogen, and one of oxygen, have been removed." (Simon's *Chemistry*, translated by Dr. Day, p. 479.)

"Chemistry, equally with the microscope, has failed to point out any essential difference between 'grey miliary' and 'crude yellow' tubercle. Chemists are agreed in regarding tubercle as consisting of some modified protein compound or compounds with fat and earthy salts." (Dr. Fuller, *Diseases of the Chest*, etc., p. 355.)

"It exhibits no tendency to form perfect cells, but rather abortive corpuscles, which form slowly, and slowly break down. Elementary molecules are not formed capable of further development.....The tissue which is occasionally seen mixed with it in expectorated matter, is disintegrated lung (Andrew Clark), and only means that so much pulmonary structure has been destroyed." (Dr. Pollock, *Prognosis in Consumption*, p. 111.)

"The destructive change which takes place in tubercle is not a vital act, but is determined by a conversion of the chemical components arising out of an interchange of the elements." (Rokitansky, Sydenham Society's edition, vol. i, p. 299.)

In the fourth place, it must observed that not only all the symptoms of confirmed phthisis, but the earliest appreciable symptoms of impending tuberculisation, upon which physicians are generally agreed, are entirely consistent with my hypothesis. These latter have been briefly and pointedly stated by Dr. Pollock in his recent work. "A pre-existing disorder of health is invariably present before there is any evidence of deposit in the lungs..... These are symptoms not referable to any disease of the lung, so far as the function of respiration is concerned, but clearly indicative of a general dis-

order of the system.....The earliest of all this group is emaciation." (*Op. cit.*, p. 39.)

The next point to which I would direct attention is the occurrence of hemoptysis in the early stage of phthisis. It so often happens that the first occurrence of hemoptysis is followed by a steady descent of the patient, that, *popularly*, the hemoptysis is looked upon as the cause of consumption. Although this is proved to be erroneous, it is a fact that, in a large proportion of cases, the first attack of hemoptysis, even though slight in amount, is followed by marked symptoms of phthisis, and especially by loss of weight. The first medical report of the Brompton Hospital (1849) shows that hemoptysis occurs in 63 per cent. of all cases of phthisis, and is more frequent in the first stage, as 3 to 1. Dr. Pollock says: "I have observed 351 cases of profuse hemoptysis, of which 204, or more than one-half, occurred in the first three months of the illness. "An early profuse hemoptysis is to be regarded as a bad prognostic; the softening being likely to take place early, and the remission in the progress of the disease to occur only at a period when already much structural mischief has taken place." "The prognostics to be derived from slight repeated hemoptysis are, on the other hand, not favourable, this being the commencement of the ordinary form of phthisis." (*Op. cit.*, p. 139.)

This is entirely in accord with my hypothesis. For, so long as there is a sufficient supply of fat in the system to protect the albuminoid tissues, there is no reason why tissue should be invaded; but, as soon as this ceases to be the case, the first breaking up of organic structure will occur; and this is the moment at which hemoptysis may be reasonably expected to take place for the first time. The hemoptysis, therefore, although in no sense the cause of the tubercle or of the emaciation, may, in truth, often mark the point in the case at which the formation of tubercle begins, and also the point from which emaciation and other general symptoms of disease must advance in a marked degree, until the progress of tuberculosis is stopped.

A sixth and still more interesting point is the relation of tuberculosis to the menstrual function, with respect to the effect upon the carbon in the system. Menstruation in health has a definite relation to the quantity of carbonic acid discharged from the system by other means. From the occurrence of puberty to the cessation of menstruation, so long as healthy nutrition is maintained, everything which arrests menstruation increases the discharge of carbonic acid from the lungs; and the recurrence of menstruation reduces that discharge. It is evident, therefore, that the menstrual flux is a means of eliminating carbon. Now, it is well known that, previous to the appearance of local symptoms of tubercle, the catamenia usually become irregular, and gradually diminish in quantity until, as the disease goes on, they entirely cease; that permanent arrest of menstruation is a usual occurrence in confirmed and progressing tuberculosis; that any prolonged arrest in the disease is usually attended with a restoration of menstruation; that, when menstruation coincides with confirmed and progressing tuberculosis, all the symptoms of the disease are usually increased at the monthly period; that the cessation of menstruation during pregnancy is commonly accompanied by quiescence in the symptoms of tuberculosis; that a large number of cases of phthisis commence at the age of puberty; and (which appears at first very curious) that at this age, among those attacked there is a large preponderance of males over females—"123 to 90." (Pollock, p. 295.)

As I read these facts, they have the following meaning. In females, the balance of their nutritive functions is so arranged that, at the age of puberty and during the whole generative period, there shall be, normally, a surplus of carbon in the system. At the age of puberty, this superfluous carbon protects them from some of the dangers of tuberculosis to which boys, who have no such surplus to fall back upon, are exposed. Thus, a girl at the age of puberty, deprived of some of the normal supply of properly prepared fats by a temporary arrest of the function of the pancreas, simply suffers a delay in the appearance of menstruation, appropriating her surplus carbon for the protection of the tissues; whereas a boy, similarly placed, has no such means of escape.

The disappearance of the catamenia when tuberculosis has set in is explained by the deficient supply of fat to the blood cutting off the surplussage of carbon for excretion. The latency of tuberculosis, when the catamenia are retained by pregnancy, is explained by the saving thus temporarily effected in the expenditure of carbon. The restoration of the catamenia during periods of arrested tuberculosis is the expression of a return to the normal proportion of carbon in the system. With this clue many other analogous phenomena may be easily explained. It is important to remember that, although the number of reliable experiments on the quantities of carbonic acid exhaled by the lungs in tuberculosis, as compared with the quantity in health, is not large; and that such experiments are open to many sources of fallacy; yet, so far as they go, they are to the effect that the exhalation of carbonic acid from the lungs is diminished in tuberculosis, which is, like the above facts, in accordance with my hypothesis.

[The order in which primarily tuberculisation usually takes place in the principal organs, is as follows:]

In adults and children, the lungs are by far the most frequent seat of tubercle. In adults, the small intestines and mesenteric glands are the most frequent seat of tubercle except the lungs. In children, the bronchial glands are affected with tubercle more frequently than any other part except the lungs, and they may be tuberculous without the lungs being so. After the bronchial glands, the small intestines and mesenteric glands are the most frequent seat, as in adults.

The lungs are the organs, in adult life, in which, *par excellence*, the required combination of conditions for precedence in tuberculisation exists. The small intestine and mesenteric system are the parts most nearly resembling the lungs in this respect.

It is hardly necessary to point out that there is no other part of the body traversed by fluids essentially the same as those which traverse the lacteals, mesenteric glands, thoracic duct, and pulmonary artery; and that there are no other parts of the body at which the relations between fats acted upon by the pancreas, albuminoid tissue, and oxygen, are so similar as in the small intestine and mesenteric system, and in the lungs.

In the lungs, the oxygen is in the air and in the nutrient capillaries; in the intestine and mesenteric system, it is in the blood of the capillaries.

[Dr. Horace Dobell's hypothesis also affords a satisfactory explanation of the variable course of tuberculosis in different cases.]

1. A sudden, almost complete, or total suspension of normal pancreatic secretion, accounts for acute tuberculosis.

2. A less complete suspension or perversion of the function of the pancreas accounts for chronic tuberculosis.

3. Either of the above changes of pancreatic secretion occurring intermittently, accounts for recurrent tuberculosis.

I have next to speak of that portion of the hypothesis which assumes a change in the absorbing power of the small intestine, by which the passage of fats into the blood may be prevented after the function of the pancreas has been restored.

That the continual passage, through the delicate absorbent apparatus of the intestinal mucous membrane, of a depraved pancreatic secretion, and of food that has not undergone those changes which fit it to be brought into contact with the absorbing surface, must damage the apparatus, is a conclusion which analogy justifies us in assuming to be correct. That such damage does occur in the alimentary canal of the tuberculous, is amply proved by a pathological research; and the extreme difficulty of identifying, after death, delicate changes in the condition of mucous membranes, renders it certain that such changes may continually occur without being recognised in a *post mortem* examination.

Bernard found that, in dogs who had been deprived of their pancreas, the fæces commonly became streaked with blood, as from ulceration of the intestines. (*Mémoire sur le Pancréas*, 1850.)

Bidder and Schmidt observed, in their experiments on the lower animals,

that inflammation of the mucous membrane of the intestine at once prevented the absorption of fatty chyle by the lacteals.

To say the least, it is in the highest degree probable that perverted pancreatic secretion, and food improperly prepared for absorption, passing over the mucous membrane of the intestine from day to day, do, sooner or later, so damage its absorbing function as to add an additional obstruction to nutrition by the lacteal system; and, as Bernard has shown that the general perturbations of the economy, and the slightest inflammation in the neighbourhood of the pancreas, especially in the intestine, pervert the properties of the pancreatic juice, any damage done to the mucous membrane of the neighbouring intestine by imperfect pancreatic action must react upon its cause, and throw a double difficulty in the way of restoring either the pancreas or the mucous membrane to a healthy condition.

Treatment.—Assuming my hypothesis to be correct in its chief parts, the following are the principles of treatment which it indicates.

1. If the case be so far advanced that it is considered hopeless to aim at restoring the function of the pancreas and absorbing power of the intestine, two principles must guide our treatment.

A. To supply the greatest amount of fat to the blood that is possible by other means than through the action of the pancreas.

a. By giving pancreatic emulsions of solid fat, or, if these be rejected, pancreatic emulsions of oil, in the hope that some absorbing powers for such matters may yet remain in the digestive tract.

b. Chiefly by giving olein, with a view to its absorption by the portal system of vessels; and by rubbing it into the surface of the body and limbs, with a view to its absorption by the skin.

c. By giving highly albuminous food in conditions in which it may be most easily utilised, so as to supply an excess of material as a source of carbon by disintegration.

B. To save the albuminoid tissues from disintegration to the greatest possible extent.

a. By supplying a surplus of carbohydrates, so as to economise as much as possible the consumption of hydrocarbons.

b. By diminishing the proportion of oxygen in the air presented to the lungs.

c. By diminishing the demand for the generation of animal heat, by supplying it artificially.

d. By reducing respiratory and cutaneous action to the lowest point consistent with maintaining what remains of appetite and digestive power.

It is obvious that the persistence in these means must be considered utterly at variance with curative intentions, and simply as the expedients best fitted to prolong the process of gradual death.

2. If the symptoms be only what is commonly called "premonitory"—that is, if they be those of commencing tuberculosis, and no reason or sign be discoverable which justifies the suspicion that tuberculisation has commenced; if a sufficiency of fat-elements remain, without calling upon the albuminoid tissues—the principles of treatment are quite opposite to those last detailed. They are now entirely curative in their intention.

A. To restore the function of the pancreas as quickly as possible, by placing the patient under those conditions which call for pancreatic action.

a. An atmosphere rich in oxygen.

b. A climate at once cold and dry.

c. Exercise of the respiratory and cutaneous functions.

d. Cheerfulness of the mind and quiet to the emotions; exhilaration of the animal spirits.

e. A good mixed diet, easily digested, and free from substances irritating to the mucous tract.

f. Pancreatic emulsions of solid fat and pancreatic juice in quantities sufficient to ensure protection to the albuminoid tissues while the process of restoration is going on in the pancreas, but not in such quantity as to

entirely supplant the natural pancreatic function and thus to counteract the measures adopted for calling it into action.

These two classes of cases represent the two extremes in the history of tuberculosis—the greatest and the least hope of cure.

3. A third class of cases, intermediate between these extreme points, represent the bulk of all those brought under the care of the physician. They require a combination of the two principles of treatment already stated. The nice adjustment of these two plans of treatment—the hopeful and the hopeless, the palliative and the curative—calls for all the intelligence and judgment that a medical man can possess.

If tuberculisation, even in the smallest degree, be going on, the first and most urgent need is to supply fats. Olein should be given for absorption by the portal system, and thus to afford, so far as it is able, materials for combustion. But above all, fats acted upon by pancreatic juice are called for, and must be supplied until the deficiency is removed, the just balance restored, the process of tuberculisation stopped. Until this point is arrived at—until the balance is turned in favour of the albuminoid tissues—everything which favours the reception of oxygen into the blood, everything which increases the wear and tear of the body, everything which calls for the generation of animal heat, directly favours tuberculisation, and precipitates the patient into the very catastrophe we wish to avert.

On the other hand, so soon as the balance has been turned in favour of the albuminoid tissues, and tuberculisation has been artificially arrested, everything which postpones the restoration of the function of the pancreas directly favours the conversion of a temporary into a permanent disease. But moderation is essential to success. Too great and too sudden a demand upon a weak and defective pancreas may only paralyse the organism in its attempts at restoration of function. It is, however, impossible to magnify the importance of periods of arrested tuberculisation, whether artificially or naturally produced. They are the bright opportunities for the permanent restoration of pancreatic function. Too often these opportunities are wasted. The patient and his friends, pleased with the marked improvement that has taken place under some plan of treatment, either continue this treatment long after its proper time and place have passed by, or give up treatment altogether, content to wait till signs of active disease return. In either case, an opportunity for establishing a permanent recovery is thrown away, and perhaps it may never return.

It must not be forgotten that, when tuberculisation and tuberculosis have been stopped, when the function of the pancreas has been restored and the intestinal mucous membrane brought back to a normal condition, there may yet remain tuberculæmia. From time to time, fresh blood-poisoning may occur from the absorption of deposited tubercle; and, if there be any considerable quantity deposited, this chronic blood-poisoning may become a tedious and dangerous disease. The absorbed tubercle may be again deposited and again absorbed, and thus keep up a ceaseless repetition of morbid changes somewhat analogous to those witnessed in pyæmia. Treatment directed to the blood-poisoning will, therefore, be imperatively called for in most cases of tuberculosis which have advanced to the stage of tuberculisation.

I have long been aiming at testing my views regarding the action of the pancreas in tuberculosis by a crucial experiment; but the difficulties in the way of artificially supplying pancreatic juice to a number of patients were so numerous that they were not finally overcome till the summer of 1863. With the valuable assistance of my friend Mr. Heathorn, who had long been working with me at the subject, I succeeded in obtaining a pancreatic emulsion of fat sufficiently palatable and in sufficient quantity to permit of my beginning my crucial experiments by administering it to patients suffering from tuberculosis at the Royal Infirmary for Diseases of the Chest. For some time our supply was too fluctuating to allow of the experiments being carried out in a manner that I thought justified me in making the results public; but in the summer of 1864 I was able to draw up the first of the

three reports which have since appeared in the *Lancet*. This was published September 10th, 1864; the emulsion used in the reported cases been made of beef-fat and the pancreatic juice of the pig. The next report was published June 10th, 1865; the emulsions used in those cases being made of lard-oil, and the pancreatic juice of the pig. The last report was published November 11th and 18th, 1865; some of the cases having been treated with the lard-oil emulsion, and some with the emulsion of suet and pancreatic juice of the pig. The number of cases in the three reports is eighty-nine; but, out of these, only twenty were in the first stage, and in these the deposit of tubercle was considerable, and the general symptoms very marked; in fact, they were, as a rule, just passing into the second stage, before applying for treatment at the hospital. It is obvious, therefore, that, although the results were remarkably satisfactory, there was not in one of the cases the opportunity of trying the treatment in the most hopeful stage. In addition to this, I must remark, that for a long time, until I was satisfied of the good effects of the emulsion, I did not feel justified in giving it unless for some reason the patient refused to take cod-liver oil; so that this added another unfavourable feature to the cases selected for experiment. And, again, it is important to bear in mind that the emulsion, as we first prepared it, was necessarily in a much less satisfactory condition than the emulsions used in later experiments. From time to time Mr. Heathorn introduced great improvements in the mode of manufacture, and these have been still further carried out by Mr. Schweitzer; so that the emulsions, as now sold by Messrs. Savory and Moore, are far more satisfactory in every respect than those which we employed for the hospital cases included in my reports.

Those accustomed to hospital practice will know well that the chances of treating tuberculosis in its simple and early stage, before the occurrence of tuberculisation, are very rare. Patients do not apply at hospitals for relief until they have some marked symptoms of disease, such as hemoptysis or purulent expectoration. When by chance they are seen in an earlier stage, they discontinue treatment directly the first feeling of debility is allayed. It is in private practice that the opportunity arises for detecting commencing tuberculosis; and it is especially within the province of the family doctor, frequenting the houses of his patients for other maladies, to keep a watch, especially in consumptive families, for the earliest dawn of impending tuberculosis.

In conclusion, I wish particularly to impress that, if the views here brought forward are correct, the importance of pancreatic emulsions of solid fat over cod-liver oil, in the treatment of tuberculosis, must be as great as that of supplying a plant with good soil instead of putting it into water. In the one case, a provision is made for the maintenance of normal life; in the other, death is only postponed by a temporary expedient.—*British Medical Journal, Jan.* 20, *and Feb.* 10, 1856, *pp.* 64, 139.

30.—NEW BI-VALVE SPECULUM FOR EXAMINING THE LARYNX, INVENTED AND PATENTED BY DR. LABORDETTE, OF LISIEUX.

[This instrument combines the functions of mirror and depressor of the tongue in one instrument, and a great deal of light is economised by reflection from the polished interior of the speculum tube.]

The description of the instrument given by the inventor is as follows:—

"The posterior blade, which is curved to fit closely the back of the mouth, descends more or less deeply into the pharynx; the inferior blade, which is short, penetrates as far as the base of the tongue, which it depresses by means of the lever-spring fixed to the handle.

"The instrument, introduced into the mouth, is pushed as far back as possible; the posterior blade being in the pharynx serves as a point d'appui; the tongue is then immediately depressed in the manner before described, by

means of the lever-spring acting upon the anterior blade, and exposes the orifice of the larynx reflected in the mirror.

"There are two sizes, one for children and one for adults, which may be obtained, price 31s. each, through most chemists, or direct from S. Maw and Son, 11, Aldersgate-street, E. C."—*Medical Times and Gazette, Feb.* 24, 1866, *p.* 213.

31.—ON THE THERMOMETER AS A MEANS OF DIAGNOSIS BETWEEN TUBERCULAR AND NON-TUBERCULAR DISEASES.

By the Editor of the MEDICAL TIMES AND GAZETTE.

We again impress on our readers the fact that the temperature is always preternaturally raised when the tubercle is being deposited in any of the organs of the body. This elevation often continues for many months. The temperature reached in tuberculosis varies from any point above that of health to 105° or 106° Fah. The usual maximum temperature of the day varies between 102° and 103° Fah.

Not unfrequently when tubercle is deposited in the meninges, or in the substance of the brain, the symptoms that result are almost identical with those that occur from other diseases of that organ. In such cases by an appeal to the thermometer we often gain information by which we can learn the nature of the disease. The correctness of this statement is shown by the following case:—

The patient was a boy of 4 years of age; previous to his admission into Hospital he had suffered from severe pains in the head, frequent sickness, and obstinate constipation of the bowels. There had been no delirium. He was ill rather more than five weeks, and gradually became comatose, from which he could only be partially roused. His head was thrown back, and was fixed in this position by the muscles of the back of the neck. The muscles at this part were rigid and contracted. There was ptosis with intermitting and fluctuating starbismus. The pupils were widely and irregularly dilated, and they acted but little under the influence of light; the skin flushed up to a very unusual degree on irritation. The respiration and the pulse were very slow and irregular for a great part of the time. There was, however, no paralysis, nor rigidity, nor convulsive movements of the limbs. The coma deepened so that he could not be roused, and continued so till the time of his death. The lungs tested by physical signs were healthy. Such symptoms, occurring in a child 4 years of age, were sufficient to lead to the idea that the disease was tubercular meningitis. If the symptoms, however, be more carefully examined they will be found to be insufficient to establish such a diagnosis.

The head of the child was thrown back, and it remained in this position. To what was this position of the head due? In *Children* the three conditions that may cause this are:—1. Angular curvature of the spine. 2. Rickets. 3. Effusion into the ventricles of the brain. With rickets there is great muscular weakness, and the child has much difficulty in retaining the head in the erect position. The head being most weighted behind, therefore falls backwards; also on account of muscular weakness the back is curved and the shoulders raised; this is the common position of ricketty children. They are said to "sit all in a lump." If the position of the head mentioned above be due to this disease, this can be at once detected by the absence of the symptoms that accompany the other diseases producing it, and by the absence of any rigidity of the muscles of the back of the neck. If, moreover, the position of the child be altered, the head will roll forward, or to either side, and will assume the most dependent position. If it be due to angular curvature of the spine, this cause can be at once detected by inspection and by running the fingers down the spinal column.

If these two diseases can be excluded, and if the head be fixed back by the muscles of the back of the neck so that it cannot be pushed forward except with difficulty, and often not at all, and if the muscles of the back of the neck

be rigid and firm, we are justified in diagnosing in a child effusion into the ventricles of the brain. This conclusion is based on many cases on which post-mortem examinations were made. In no case have we known the sign to prove incorrect.

In this case the head was firmly fixed backwards, and the muscle of the back of the neck were contracted and hard. There was no angular curvature of the spine. Hence we were justified in concluding that the child suffered from effusion into the ventricles of the brain.

Effusion into the ventricles is, moreover, sufficient to explain most of the remaining symptoms of the case. From this there results loss of consciousness, loss of sight, drooping of the upper eyelids, strabismus with irregularity of the pupils, flushing of the skin or irritation, slow and irregular pulse, the irregular breathing, and curving of the head backwards.

This case, and that which will shortly follow, prove that these symptoms are not significant of tubercle deposited in the meninges or in the brain. That they may result simply from effusion into the ventricles we conclude from a case in which these symptoms were present. At the post-mortem examination there was found considerable effusion into the ventricles produced by inflammation of the straight and one of the lateral sinuses, and the occlusion of these vessels by a firm, old, and adherent clot. This is the simplest form of the disease that can be met with, as it can hardly be maintained that the symptoms were caused by the inflammation of the sinuses, apart from their producing effusion into the ventricles. The brain itself, with the exception of the effusion into the ventricles, was healthy.

Nor can the remaining symptoms present in the case just given—namely, pain in the head, vomiting, constipation of the bowels—be accepted as proof of the tubercular or inflammatory nature of the affection of the brain; for such symptoms occur in other diseases as this case proves. The utmost, therefore, that could be arrived at from the symptoms present was the certainty that the child had suffered from effusion into the ventricle of the brain.

What diseases may produce such an effusion into the ventricles of a child of this age? 1. Inflammation, simple or tubercular; 2. Tumours of the brain; 3. Coagulation of blood from inflammation of the straight or lateral sinuses, with occlusion, complete or partial, of these vessels. If other diseases cause the effusion, they are very rare. Of those mentioned, the former two are by far the most common. How are we to learn to which of these causes the effusion is due? The thermometer often gives us the information we require. We have seen that the deposition of tubercle causes an elevation of the temperature of the body. If, therefore, the temperature be normal, the disease producing the symptom is not tubercular. It may be objected that tubercle may be deposited in the brain, and the further deposition may cease, and that in such cases the temperature will be normal while the effusion remains, the symptoms resulting from this will also continue. It is true that such cases occur, but they are infinitely rare, as the deposition of tubercle very generally continues till the patient dies. This is rare of tubercle in the brain substance; it is still rarer of tubercle in the meninges of the brain. How can we learn if the effusion is caused by non-tubercular inflammation of the meninges of the brain? If the temperature be normal at the commencement of the attack, this disease is excluded from the diagnosis. But children much more commonly survive the simple acute than the tubercle inflammation of the meninges of the brain. The non-tubercular inflammation of the meninges either destroys life in five or ten days, or the inflammation subsides and the temperature becomes normal; but in such cases the effusion into the ventricle continues and may increase in quantity. How are we to learn that in such a case the effusion is due to a previous inflammation? The two chief points to be relied on are, the age of the patient and the method of invasion of the attack. The former of these is of the most value. Simple inflammation of the meninges in children almost always occurs during the first few months of life. (Tubercle in any of the organs is uncommon under two years and very rare under one year of age.) Non-

tubercular inflammation of the meninges most commonly begins with an attack of convulsions—tubercular inflammation rarely so. The other cause also of ventricular effusion, namely, tumours, are very rare in children under two years of age. Thus the most common cause of such effusion in very young children is simple inflammation of the meninges.

But while the temperature of the body is raised both by tubercular and non-tubercular inflammation of the meninges in some part of their course, *the temperature of the body is normal in cases of non-tubercular tumours of the brain.*

In the case just given the temperature of the child was *normal*, and thus the nature of the disease was proved, and as the child came under observation early in the disease, we excluded simple inflammation of the meninges, and moreover the child was not of the age when this most frequently occurs, nor did it begin in the manner usual with this disease.

The diagnosis, therefore, was that the child suffered from effusion into the ventricles, and that this was most probably produced by a non-tubercular tumour (inflammation of the straight sinus and its occlusion by a clot is so extremely rare that this could with probability be excluded from the diagnosis). This conclusion proved correct, for on the post-mortem examination there was found a non-tubercular tumour of the size of a Maltese orange, situated in the anterior and inferior part of the cerebellum. This had pressed on the right half of the medulla oblongata, and had caused much atrophy of this part.

We venture to add another case to exemplify the use of the thermometer in similar cases to the one just given. A boy, aged 11, had suffered from severe pain in the head, with vomiting and obstinate constipation of the bowels. When first seen these symptoms still continued. His head was slightly thrown back, and the muscles at the back of the neck were rigid. His face was flushed, and he was partially unconscious. The pupils were dilated, and failed to act under the influence of light. There was slight strabismus, but no paralysis nor rigidity of any of the limbs. These symptoms are those most commonly met with in tubercular meningitis. *His temperature, however, was always normal.* Thus the disease could not be of a tubercular or inflammatory nature. It was considered probable that he suffered from a non-tubercular tumour of the brain. The symptoms described above disappeared in a few days. He was frequently seized with similar attacks, from each of which he recovered. He died suddenly from some unexplained cause. There was found a large fibro-plastic tumour, situated in the cerebellum. The ventricles were dilated, and contained about 3 oz. of fluid.

These cases serve to illustrate the assistance we may derive from the thermometer in cases similar to those described.

If, in such cases, the temperature be normal, the disease is not tubercular; if, on the other hand, the temperature be elevated in a child over two years of age, it is in the highest degree probable that tubercle is being deposited in the meninges or in the brain of the patient. For if the elevation of the temperature should be caused by some intercurrent disease, such disease can mostly be detected.

Before concluding this article we add a few words concerning effusion into the ventricles of the brain, and the symptoms that result from this.

If the effusion occur quickly, the consciousness is greatly lessened; if it occurs more slowly, the consciousness is but little impaired. The loss of consciousness is caused by the pressure of the grey matter resulting from the effusion. The brain, however, accommodates itself to slow pressure, and thus the consciousness is but little altered when the effusion occurs gradually. Again, when the effusion has occurred quickly, and the consciousness has been quite lost, if no further effusion take place, or if its further increase be slow, the intelligence of the child to a great extent returns, and after some time has elapsed, it may be chiefly restored. In most of the cases similar to those described, it is the rapid effusion by producing loss of consciousness that quickly destroys life. When the effusion is slowly produced, the children may survive for years, and in some cases they live the number of years usually allotted to man.

The rigidity of the muscles at the back of the neck, and the degree to which the head is drawn back, is proportionate to the amount and rapidity of the effusion. If this displacement of the head is but slight, and but a short time has elapsed, the amount of the effusion is small; if, on the other hand, the effusion has occurred slowly, and the head has had time to enlarge, then this rigidity of the muscles, and this position of the head, is but slightly marked. This symptom also disappears after the effusion has lasted some time, if no further increase occurs. It disappears much earlier in young people, in whom the bones of the head are ununited, than in those children whose skull is firmly closed. In the latter it lasts often for a considerable time; it continues long after the intelligence of the child has greatly returned.

The ptosis, strabismus, and inequality of the pupils are but slightly marked when the effusion is slowly produced, and this soon disappears when the further effusion of fluid into the ventricle ceases. Loss of sight, however, is mostly present whether the effusion occurs quickly or slowly, and is one of the last and rarest symptoms to disappear.

The shape of the head, when the fontanelles are open, is so peculiar and characteristic that we need not dwell on it here. The shape, however, somewhat differs when the bones are firmly closed. In such a case, when the effusion occurs slowly, and consequently the patient lives some months, the enlargement is most marked at that part of the skull occupied by the parietal bones. The frontal bone is comparatively little affected. Thus there is formed a very prominent and acutely formed ridge, corresponding to the junction between the parietal and frontal bones. The sudden elevation in some cases amounts to half an inch in extent. It occurs earliest, and is most marked, in the temporal regions.—*Med. Times and Gaz., March* 24, 1866, *p.* 311.

32.—ON A CERTAIN FORM OF HEMOPTYSIS UNASSOCIATED WITH PULMONARY TUBERCULOSIS.

By Dr. Richard Payne Cotton, Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

[That hemoptysis is met with in a very considerable number of non-tubercular cases is well known; its occurrence however always suggests the presence of tubercle in the lung. There are many conditions which may give rise to hemoptysis.]

In the present instance I am anxious only to draw attention to a not unfrequent, but, so far as I know, little recognised form of non-tubercular hemoptysis, met with chiefly in the female sex, but sometimes also amongst males, generally in the early period of life.

It may simplify my description of this variety of hemoptysis if I give a brief account of two or three cases in point.

A young lady, aged 18, recently arrived from a residence in one of the West India islands, was supposed to be phthisical. I was requested to see her, and report upon the nature of her disease, about which several very conflicting opinions had already been given. She was anæmic, nervous, and out of health; had a dry cough, but had not become thinner; her catamenia were regular, but scanty; her appetite was capricious; and she had had frequent hemoptysis, which there was every reason to believe did not proceed from either the mouth or fauces. The blood, upon examination was found to be thin and watery, of a dark colour, free from coagulum, unassociated with either bronchial or salivary secretion, and in general appearance much resembling a mixture of *red-currant jelly and water.* I was informed that this was its general character. Sometimes it had been considerable—as much as half a pint in twenty-four hours; at others, it would not exceed a teaspoonful or two during the same period; sometimes it would be scarcely enough to tinge a pocket-handkerchief, and often it would disappear for days together. This state of things had existed for nearly two years, causing great anxiety

to the patient and her friends, from a belief that it was indicative of pulmonary disease. Careful examination of the chest, however, failed to elicit any evidence that such was the case. Rest, change of air, and tincture of sesquichloride of iron, entirely restored this patient to health. It is now more than three years since I was consulted, and I heard a short time back that the young lady was in perfect health.

A case very similar to this came under my notice two years ago in consultation with Mr. Humpage, of Upper Seymour-street. A young lady, aged 24, had long been delicate, and was supposed by her family to be consumptive. She had become thinner; had had a dry cough for some length of time; and had spat blood. Upon examination, this was found to present an appearance very similar to that described in the preceding case; it looked, in fact, more like watery red-currant jelly than anything else. As in the other patient, there were no decided physical signs of tubercular disease; and we came to the conclusion that the patient was not phthisical. Time has justified our diagnosis; Mr. Humpage having lately informed me that the young lady had, for some months, lost the symptom which had caused so much alarm, and was in good health.

Another case, which I shall even more briefly relate, came under my notice about eighteen months back. It was that of a young lady, 12 years of age, of slender and somewhat delicate appearance, but free from every symptom of tubercular affection. Mr. J. N. Winter, of Montpelier-road, Brighton, frequently saw this patient, and quite agreed with me as to the nature of her disease. She first alarmed her parents by spitting up, just after going to bed one night, a considerable quantity of blood. This was found upon examination to be watery and dark-coloured—in fact, of the *thin red-currant jelly character* already described. This symptom has recurred at various intervals with more or less intensity, and the child still remains delicate, but without any indications of tubercular disease. At the time of her attacks her tonsils and pharynx are somewhat congested, her gums spongy, and her fingers even have sometimes exuded a little watery blood, just as one sometimes sees in extreme cases of purpura. It is evident that, in this instance, the blood escapes not only from the mucous membrane of the respiratory passages, but also from that of the throat, tonsils, and gums.

Other cases of this form of hemoptysis have fallen under my observation; but I shall not specially refer to them in consequence of not knowing their sequel. At least twelve or thirteen have happened in my own wards in the Hospital for Consumption. Two only of these were males; the rest were females, generally of delicate and nervous appearance, and under the age of 30. Several had very suspicious symptoms of phthisis; but the physical signs failed to exhibit any evidence of pulmonary tuberculosis, and most of them improved in health under appropriate treatment. In every case the expectoration was of the same general character; and sometimes it was mixed more or less with bronchial mucus, slightly tinged perhaps with blood, and sometimes with salivary secretion; but more frequently it was simply watery blood, resembling, as I have described, a mixture of red-currant jelly with water.

The following are the conclusions at which I have arrived from a consideration of the preceding notes:—

1st. There is a form of true hemoptysis in which the expectoration is of a dark colour, and of a more or less watery consistence, bearing a close resemblance to a mixture of red-currant jelly and water.

2nd. That such hemoptysis is of non-tubercular origin, and may proceed from any part of the gastro-pulmonary mucous membrane.

3d. That it is attributable, to a morbid and fluid condition of the blood, allied, at least in appearance, to that which is met with in purpura and scurvy. —*Lancet, Dec.* 2, 1865, *p.* 6,18.

33.—ON THE USE OF PHENIC (CARBOLIC) ACID FOR THE CURE OF PHTHISIS.

By Dr. J. R. Wolfe, Aberdeen.

Some two years ago the eminent physiologist, Prof. Longet, who had an attack of hemoptysis with tuberculosis, made some experiments upon himself with phenic (carbolic) acid, by which he was greatly benefited. It is owing to the favourable report of that distinguished patient that this substance is now largely employed by some of the French physicians. Dr. Labori, of the Convalescent Hospital (L'Asile Impériale de Vincennes), has told me that he administered it to between 200 and 300 patients in different stages of phthisis, with most favourable results. The mode of administration is as follows : fifteen drops of the pure acid are dissolved in 3ij. of spirits, and the solution mixed with ℥xxxij. of water. This quantity is administered daily, partly by the stomach and partly by the inhalation of the fluid in a pulverised state.

There are several instruments now in use for reducing fluid to fine molecules.

The internal administration of creosote and the inhalation of tar vapour have long been recognised as useful remedies in phthisis ; but, considering the *modus administrandi* just referred to, it is not quite as if we wanted to introduce an old friend with a new face. The great objection to the inhalation of tar and terebinthinate vapour was, that in order to be effectual it must be persevered with, sometimes for years. Even in the case of that valuable and standard remedy, cod-liver oil, when first introduced for the cure of phthisis, some questioned its remedial effects simply for want of perseverance to give it a long enough trial. Now, by means of the pulverising method, the larynx, trachea, and, as has been proved upon animals, the lung tissue, are brought into direct contact with the remedial agent, which is also absorbed into the system, and it has besides the advantage that the patient can regulate the quantity to be inhaled.

As an adjuvant to cod-liver oil, it is beneficial in all stages of phthisis ; but it is eminently useful in arresting hemoptysis, in allaying irritation, and arresting the profuse secretion in cases of chronic bronchitis and of cavities in the lungs, of laryngeal phthisis, and of colliquative sweats, and I think it deserves a trial in cases of diphtheria. I would here take the opportunity of strongly recommending the use of the pulveriser. From what I have seen of its efficacy for the last eight years in laryngeal, bronchial, and lung affections, in which various medicines have been administered in that form, I am confident that it needs only to be known to the profession in order to be generally employed. It opens a new field of therapeutics, and enables us to cope with disease which the gargle, probang, and even the ordinary inhaler could not reach.

The pure phenic acid directly applied is used in bites of the charbon fly or any poisoned wounds. Charpie dipped in a solution of ʒss. of the acid, in ʒj. of spirits, diluted with ℥xv. of water, is used by Maissonneuve and Gosselin for dressing indolent ulcers and suppurating surfaces. A similar dressing answers excellently in open cancer and fœtid discharges of all kinds. The only objection to its use, is its strong and rather disagreeable smell. To obviate this, I have made some experiments, and find that cotton-wool saturated in the pure acid, and then pressed to get rid of the excess of acid, and dried, leaves enough of the acid to act upon the wound without leaving so much as to make the smell disagreeable. The cotton so prepared is kept in a tin box to prevent volatilisation of the acid.

As far as my experiments have gone, I am satisfied with its beneficial results. It causes a healthy granulating surface, counteracting the tendency to hyperplasie and suppuration. But my study of the subject is not sufficiently advanced to enable me to enter into particulars in this communication. I may express it, however, as my opinion, that its employment for dressing wounds will greatly lessen the risk of purulent and erysipelatous infection in our Surgical wards.—*Med. Times and Gazette, Nov.* 25, 1865, *p.* 572.

DISEASES OF THE ORGANS OF DIGESTION.

34.—CONCERNING THE TREATMENT OF CONSTIPATION AND OF STOPPAGE OF THE BOWELS—WITH SPECIAL REFERENCE TO THE USE OF ATROPIA AND OF GALVANISM.

By Dr. ALEXANDER FLEMING, Senior Physician to the Queen's Hospital, Birmingham.

[Dr. Fleming has now for many years employed atropia in cases of epilepsy, asthma, and other disorders; and about eight years since his attention was forcibly directed to its effect in promoting the action of the bowels.]

In the course of my practice, when I had occasion to prescribe atropia, I noticed frequently that, in from one to four days, a slight relaxation of the bowels took place. The stools were but little altered in character, and the intestinal secretion but slightly increased; still, the action of the bowels was decidedly easier, and, if constipation had existed, it was removed. Occasionally the purging was more marked. I believe that this effect is brought about by *increased* peristaltic action. The cause of this increased action may be direct stimulation of the muscular coat by the atropia carried to it with the blood; but other causes have been suggested to my mind from close observation of the effects of atropia on other parts of the body, more especially on the throat, stomach, and bladder. When this drug is exhibited in small and medicinal doses, it causes remarkable dryness of the throat and tongue, difficulty in, yet constant efforts at, swallowing. The changes in the act of micturition are remarkable and noteworthy. This is often hurried and frequent, sometimes interrupted, and occasionally there is slight strangury. I have seen a patient compelled to make water every five minutes. In the throat, the mucous secretion is obviously checked, the membrane is seen to be dry, and the surface is thus rendered more susceptible of irritation; hence the constant efforts of deglutition. I believe the effect of the drug on other mucous membranes to be of the same nature; and in the bladder, this arrest of the mucous secretion results in irregular and frequent micturition. According to this view, its action on the bowels is easily explained. The mucous secretion being checked, the irritation caused by the contents of the intestinal canal, when its surface is thus unprotected, provokes more prompt and vigorous contractile action.

Secondly, atropia constricts the smaller arteries; and we can understand that a gut, dormant and paralysed by distension, is the subject of passive congestion, the continuance of which will contribute to maintain its state of inertia. Atropia, acting on the arteries, checks the supply blood to the bowel, relieves the congested muscle, and thus facilitates its return to healthy action. This *modus operandi* is analogous to the well-known effect of blood-letting or leeches, in relieving the congestion of, and unloading, an inflamed intestine.

Accepting these views of the *mode of action* of atropia on the bowels, we at once perceive its advantage over the ordinary irritant purges in the treatment, not only of simple constipation, but especially of the more serious and alarming cases of intestinal obstruction from impacted fæces. The ordinary irritant purges provoke increased secretion and peristaltic action of the gut *above* the obstruction; this may succeed in propelling the accumulation forward, but should it fail in doing so, we have inverted action and vomiting, with the further risks of enteritis, and general, and it may be fatal, exhaustion of the patient. Atropia, on the contrary, operates through the blood on the entire canal; acts directly on that part of the gut which is distended by the accumulation, and so paralysed. Deprived, by the drying qualities of the drug, of its natural coating of mucus, the mass more readily provokes irritation; the natural contractile action is re-established; and the bowel is more or less quickly relieved of its contents.

There is another circumstance connected with atropia, which distinguishes its operation from that of common purgatives; its action is not followed by reaction; its relaxing power is not succeeded by a disposition to constipation.

On the contrary, the improved action of the bowels is, comparatively speaking, sustained.

The powder and extract of the crude drug belladonna have been employed successfully in constipation by Bretonneau, Trousseau, Fleury, Drs. Brinton, Routh, Fuller, Leared, and others; and a most interesting paper on the Use of Belladonna in Intestinal Obstruction was read at the Bristol meeting of our Association in 1863.

The causes of constipation, and the conditions under which it occurs, vary infinitely; each case requiring separate consideration, especially as regards the hygienic and dietetic treatment.

It is not my intention to enter now into the question of the regimen and diet of constipation; but, to prevent any misconception as to my view of their value, let me state distinctly, that I assign to them the first place in importance as curative means, and regard medicinal agents in the light only of valuable auxiliaries. At the same time, the error should be avoided of underrating the value of medicines. In former times, they were relied on too exclusively; of late, under the influence of hydropathy and homœopathy, their importance has been most unwisely neglected. It is the function of the philosophical therapeutist to recognise the respective value of all remedial agents, whether hygienic, dietetic, or medicinal, and to assign to each their relative importance.

This truth is forcibly illustrated in the disease now under consideration; for while, in the more simple forms of constipation, regimen and diet are often equal to the cure, many examples of a more obstinate nature have occurred to me, where these means alone have signally failed to give relief, but where recovery has ensued when the treatment I shall now describe has been strictly followed.

In cases of *simple constipation* I have exhibited atropia in various forms, both in pill and in solution; but my later experience has led me to the adoption of a plan of treatment, of which the following is an outline.

The subjoined draught is given the first thing in the morning and the last thing at night on an empty stomach.

℞. Magnesiæ sulphatis ʒj; acidi sulphurici aromat. ♏x; tinct. aurantii ʒj; aquæ ad ℥j. M.

Ten minims (containing one-sixtieth of a grain) of a solution* of atropia are added to the draught at bed time; and the dose is increased nightly by two minims, until a very slight degree of the earlier physiological effects of the drug—dry throat, wide pupil, and dim sight—is produced. This is attained with much precision and safety; but it may be necessary to give thirty, forty, or even fifty minims, according to the strength of the patient, before this result is attained. The dose should then be somewhat diminished, and continued at the reduced quantity for two or three weeks, as circumstances may indicate. I then discontinue the drug gradually; and finally replace it with strychnia, giving five minims of a solution† in both morning and evening draughts for a week or two; or the strychnia may be given alone as soon as the saline draught can be dispensed with. This commonly suffices to restore the normal tone of the bowel, and completes the medicinal treatment.

When constipation is neglected, the fæces accumulating gradually distend the bowel, and finally deprive the muscular coat of its irritability and con-

* The solution of atropia which I use is made thus:—Atropia, 1 grain; distilled water, 5 drachms. Dissolve *thoroughly* with the aid of a few drops of diluted muriatic acid, and add of rectified spirit sufficient to make ten drachms. This solution keeps well, and is of uniform strength. The tincture and extract of belladonna, however carefully prepared, vary much in power. I have found the tincture of one chemist seven times the strength of the same preparation from another and equally respectable chemist; and the extract is even more uncertain. The internal, and at the same time efficient, use of these preparations is for this reason very unsafe. The solution is so proportioned, that ten minims, containing one-sixtieth of a grain of atropia, is the commencing dose for the adult.

† The solution of strychnia which I use is made thus:—Strychnia, 2 grains; distilled water, 5 drachms. Dissolve the strychnia *thoroughly* with the help of a little diluted muriatic acid, and add of rectified spirit sufficient to make ten drachms. This solution has the same advantages over the powder, extract, and tincture of nux vomica, that the solution of atropia has over the tincture and extract of belladonna. It is uniform in strength, passes readily into the circulation, and the dose can be apportioned with accuracy. The ordinary commencing daily dose is ten minims, and contains one-thirtieth of a grain of strychnia.

tractility, and we have established one of the most frequent forms of *obstruction* of the bowels. (The observations in the present paper refer to this form only.)

If, after a moderate use of the ordinary purgatives by the mouth and in the form of enemata, the obstruction shows no disposition to yield, and the patient suffers from pain and distension of the belly, with (it may be) nausea and vomiting, I prohibit entirely the use of the more powerful cathartics, the exhibition of which increases the vomiting and irritation, and may provoke inflammation. I desire the patient to be confined to liquid food; viz., milk and beef-tea alternately every four hours. If there be much vomiting, I direct the milk to be mixed with one-third of Carrara water, and the quantity of food at each meal to be very small, until the irritability of the stomach has subsided. The following draught is prescribed every four hours before each meal.

℞. Magnesiæ sulphatis ʒj.; solutionis atropiæ (author) ♏ iv; acidi sulphurici diluti ♏ x; aquæ, ad ℥j. M. Fiat haustus.

Should there be much spasmodic pain, I add half a drachm of chloric ether, prepared by distillation, to each dose. This draught is, for the most part, readily borne by the stomach; promotes gently the action of the bowels; and softens their contents. The atropia favours, in the manner already indicated, the contractile power of the gut. In using atropia in the manner specified, the patient *must* be seen twice daily; for, as a slight degree of the physiological action of the drug should be induced, the dose should be increased, diminished, or omitted, according to the effect observed. If pain and indications of approaching inflammation be present, warm fomentations to the belly are demanded; on the other hand, if these symptoms be absent, the general purpose of the medication is promoted by frictions two or three times a day with warm liniments; the rubbing to be so applied as to promote the normal course of the intestinal contents.

In a considerable proportion of cases, this treatment alone affords the desired relief; in other and more obstinate examples, we have to conjoin the use of aperient enemata. These should be used two or three times daily; and be introduced by means of the rectum-tube as high as possible into the bowel. When ordinary injections fail, ice-cold water sometimes succeeds; and it is well to bear this in mind.

In inserting the tube for any distance into the rectum, much annoyance is often experienced by its doubling upon itself. I have overcome this difficulty by the use of a stilette within the tube. Should any impediment, such as a fold of membrane, obstruct the passage, the withdrawal of the stilette for an inch or two allows the flexible tube to adapt itself to sinuosities of the intestine, and facilitates its further introduction. I have had the tube marked off in inches, to indicate precisely the extent of insertion. The higher it can be passed with safety, the more efficient is the injection.

Between the enemata, galvanism should be applied to the bowels. This agent should be conducted through the rectum, and passed, as nearly as may be possible, through the paralysed gut; care being taken to employ it gently, but repeatedly, our object being rather to restore the action of the bowel by small but successive doses of the stimulus, instead of attempting to dislodge the impacted contents by one powerful application of the galvanic current.

Employed in this manner, the galvanism is infinitely more efficient than when passed in the ordinary mode from the back to the belly. It gives rise, however, to acute suffering; and, unless it be used gently, as I have directed, tends to exhaust the patient.

The following is the apparatus I employ. Through a rectum-tube, twenty inches long, a copper wire is passed with a brass button at the distal extremity; a hole being drilled at the opposite end for the reception of one of the wires of the galvano-magnetic machine. The tube being passed into the rectum, the current is completed by applying to the abdominal walls the excitor of the other wire. This excitor—which is sometimes a sponge, sometimes a cylinder of metal, and at other times an olive-shaped metallic button covered with leather—should be moved along the course of the distended

bowel, and its position constantly shifted; the skin of the belly being kept moist with salt water.

When the current is gentle, its application may be continued for half an hour at a time, and from time to time a sharper shock may be transmitted; the force of the current, and its duration, being carefully adapted to the strength of the patient.

I use Duchenne's galvano-magnetic machine, as with it the current can be usefully modified in various ways.

I have employed galvanism by the rectum in the manner indicated, in intestinal obstruction, since 1857.

Such is the outline of the treatment of intestinal obstruction from impacted fæces, which I can say with confidence has furnished very satisfactory results.

In conclusion, it may not be superfluous to add that, in the diagnosis of the cause of stoppage of the bowels, we may often be at fault and treat as a case of simple accumulation, some one or other of the many forms of insuperable obstruction. It is satisfactory to know, that the medicines here recommended may do good and can do no harm in such a case; but the suspicion of error in diagnosis obviously enjoins much caution in the use of galvanism. This was well shown in a case of complete obstruction in an elderly female, which I saw in consultation with Mr. Hoskins of this town. The usual treatment had been judiciously employed. Atropia was then administered, and most efficiently, so far as regards its physiological action. Galvanism was also carefully used. The obstruction was insuperable, and finally proved fatal. The patient, however, lived for a week after the use of these measures, in comparative ease; showing that no harm had resulted from their employment. A *post mortem* examination was not obtained.

For the better illustration of the nature of the results furnished by the treatment described above, I subjoin histories in detail of five cases.

Case 1. *Severe Constipation of three years' standing; Colic, Vomiting, and serious Exhaustion; Failure of ordinary Methods of Treatment; Use of Atropia; Recovery.*—The patient, a young lady, aged 18, came under my care in September, 1861. She was then suffering from almost constant spasms of the bowels, causing intense agony, aggravated after every attempt to take the simplest food; the vomiting and retching were incessant. There was no fever nor tenderness of the belly. She was very weak, confined to bed; the countenance was pale; the pulse was feeble, but not frequent; and the case wore an aspect of much anxiety.

History.—Three years previously to the time of my seeing this patient, she had suffered from prolonged and most obstinate constipation, brought on by gross neglect of the bowels at school. The stomach became irritable, rejecting many articles of food; and the least imprudence in diet brought on spasmodic attacks of pain in the belly and vomiting. After repeated, but unsuccessful attempts to obtain relief under medical treatment in various places, her friends placed her at Malvern under the water-cure. She derived no permanent benefit; though, from the strict attention to diet, her general health was improved, and the attacks for a time were not quite so frequent. They returned, however, shortly with redoubled violence, accompanied by excessive flatulence, twisting of the bowels, and severe suffering. The irritation of the stomach increased, until the simplest food produced so much pain that she was, as she expressed it, "terrified to eat."

After a careful examination of the case, in which I had the valuable assistance of Professor Simpson of Edinburgh, it was concluded that there was no inflammation, but that the peristaltic action of the bowels was perverted; in the smaller intestines it was abnormally active, leading to severe spasmodic contortions and heaving of the belly; while the colon was unduly distended and more or less completely paralysed. There was then no absolute obstruction, but a constant tendency to constipation.

Acting on this diagnosis, the treatment of constipation described in this paper was adopted. The patient was restricted to a milk-diet. Atropism was fully induced; blisters were repeatedly applied to the belly; and aperient enemata were administered from time to time. Under this medi-

cation, which occupied about a fortnight, the colon was gradually unloaded of its accumulated contents, and the vomiting and spasms quickly subsided. The stomach became tolerant of food; the normal appetite returned; and the patient rapidly regained flesh and strength.

The cure was absolutely permanent. The stomach was delicate for some months; but, with a moderate attention to diet, the patient continued well. There was no return of constipation or spasms of the bowels. The young lady is now in the enjoyment of excellent health in all respects.

I have no hesitation in describing this as a remarkable case. The relation between the physiological action of atropia and the improvement of the symptoms was unequivocal; lastly, the short duration of the treatment, and the completeness of the cure, contrasted very strikingly with the lengthened and unsuccessful efforts at relief which had been made at previous times in the earlier history of the case.

Case 2.—Stoppage of the Bowels from Fæcal Accumulation; Colic; Stercoraceous Vomiting; Failure of Ordinary Treatment; Atropism on the thirteenth day; Recovery.—(This case occurred in the practice of Dr. Spencer Thomson of Burton-on-Trent, to whom I am indebted for the following notes.) L. R., aged 45, married, a thin, spare, delicate woman, was seized on the night of October 20th, 1863, with severe twisting pain in the bowels, incessant vomiting, and complete constipation. The bowels had been opened on the previous day. Within twenty-four hours the vomiting became stercoraceous; and continued so, with intermissions, for thirteen days of the sixteen during which the obstruction lasted. During the latter half of the period of the attack, there was much tympanitic distension and tenderness, but there never were any positive signs of peritoneal inflammation.

The remedies resorted to were at first bismuth, prussic acid, opium, and chloroform, to subdue the irritable condition of the stomach; counter-irritants of the belly; and a persevering use of every variety of purgative injection.

No relief was obtained. On the thirteenth day of the attack, one drachm of the following mixture was directed to be taken every hour.

℞. Solutionis atropiæ (Fleming) ♏xv; acidi hydrocyanici diluti ♏xv; aquæ ℥ij. M.

Soon after the above was commenced, the stomach became quiet, and retained the medicine, as well as small quantities of beef-tea and brandy and water. In forty-eight hours, the atropism being now manifest, the obstruction showed signs of yielding, and the bowels were gradually completely relieved; and, on the sixteenth day from the commencement of the attack, all signs of obstruction were removed. Mrs. R. gradually recovered, and is now (April, 1864) in her usual health, having had no return or even threatening of the disease since. The catamenia, which were irregular for some months previous to the attack, have since been quite normal.

Case 3.—Obstruction from Fæcal Masses; Constant Sickness and Vomiting; Use of Salines, Atropia, and Galvanism; Gradual breaking down and Removal of the Accumulations; Recovery.—S. L., aged 50, when placed under my care, was suffering from severe tormina, constant retching and vomiting, and obstruction of seven days' standing. The pulse was frequent and feeble, the countenance anxious, and the skin covered with perspiration. Three distinct tumours were to be felt in the belly; one in the upper part of the ascending colon; one at the corner, between the ascending and transverse colons; and the third in the transverse colon itself. These were moveable, and presented the other features of fæcal masses. They differed in hardness, the central one being much the hardest of the three. The diet, saline draughts, atropia, aperient injections with the rectum-tube, and after several days galvanism, were employed as indicated in my paper. The vomiting rapidly ceased, and the patient was soon able to take food well. In three days, the bowels began to act, and continued to do so two or three times daily; the evacuations were small in quantity and fluid. The tumours in the belly gradually decreased in size, and finally were dispersed.

In the discharges, the gritty and insoluble components of the fæculent concretions were readily distinguished; they were composed of small pieces of bone, undigested tendon, &c. The patient was an old dyspeptic, through whose stomach the harder portions of the food were apt to pass undissolved.

Galvanism in this case was used daily, but very gently, for a fortnight, and was passed through the colon in the neighbourhood of the swellings. When the galvanic current was powerful, the suffering was acute, and could not have been maintained without risk of undue exhaustion. Therefore the utmost care was required in its administration; and no attempt was made to secure the immediate expulsion of the fæcal masses.

In the third week of the treatment, the intestinal canal was clear of obstruction; and the patient, though very weak, was convalescent. He made a good recovery.

Case 4.—*Simple Constipation of long standing, in a Healthy Subject; Failure of Dietetic and Ordinary Modes of Cure; Use of Atropia; Recovery.*—J. J., aged 27, a strong healthy young man, living in the country, applied to me in April, 1862, on account of constipation. His bowels had for years been obstinate, and for eighteen months he was obliged to take aperient medicines daily; otherwise he would pass three or four days without going to the closet, and would then suffer much pain at stool. So long as he continued taking aperients, his appetite and general health were good. He had several times been under medical care, with temporary advantage; and had endeavoured to rectify the evil by diet and exercise, but unsuccessfully.

I directed him to sponge with salt water once daily, in the morning; to rub the belly vigorously: to take abundant exercise (without fatigue); to omit from the diet, tea, coffee, and stimulants, with the exception of a glass and a half claret mixed with water to dinner; to take cocoa to breakfast, porridge to supper, and vegetables and fruit in moderation. The medicinal part of the treatment consisted of the saline draughts and atropia, as indicated in this paper. A moderate degree of atropism was induced. The improvement was slow, but very marked. In three weeks he was able to discontinue his medicines, but has ever since persevered more or less closely with the regimen and diet. It is now eighteen months ago; and he has not during that time, except at rare intervals, been troubled with constipation.

Case 5.—*Obstruction; Slight Stricture; Fæcal Accumulation; Severe Tormina, Sickness, and Vomiting; Salines, Atropia, and Enemata; Recovery.*—(I saw this patient, whose case I will relate very briefly, in October of last year, in consultation with Mr. Ross Jordan.) W. C., aged 35, had suffered for five days from obstruction, severe tormina, and incessant retching and vomiting. There were tenderness, distension, with dull percussion in the left iliac region. There was obvious accumulation in the descending colon. Salines, atropia, and enemata, were employed, with the result of gradually unloading the bowel, and restoring the healthy character of the evacuations, with the exception that the stools continued to be slightly flattened.

It appears that a year ago he had sought advice, on account of indigestion, from a quack, who gave him four doses of some drug (probably lobelia), which purged him very severely, causing much pain, and discharge of blood and mucus. Since then, the bowels have continued to trouble him; and the symptoms point to a slight constriction—the consequence, probably, of inflammation—about the sigmoid flexure.—*British Medical Journal, Dec.*, 1865, *p.* 651.

35.—ON THE TREATMENT OF DIPHTHERIA WITH HYPOSULPHITE OF SODA.

By J. Clarkson Maynard, Esq., Wisbeach.

[The plan of treatment pursued in a case of diphtheria by Mr. Maynard, and Dr. Tubbs, of Upwell (to whom the idea of using hyposulphites was first suggested by his gardener arresting vine disease by means of sulphur), is as follows.]

On first visiting a case, if not very far advanced, and in which only a few spots are visible, the throat is dressed twice a-day out with a strong solution of the hyposulphite of soda—viz., 3iij. of the hyposulphite, glycerine 3ij., with 3vj. of water. This generally removes the incipient exudation in forty-eight hours, sometimes in less. But if the case is an advanced one and the parasitic plant is making rapid strides, we wash the throat well out with warm water, by means of one of Maw's flexible syringes. This is alike agreeable and most beneficial to the patient. The affected parts are then dressed with the strong solution, and a gargle of 3ss. of the hyposulphite to half a pint of water, with ℥ss. of glycerine, is given, to be used every hour.

The effect of the solution upon the exudation is most marked. It appears to solidify and dry up the false membrane, and when the syringe is again used, which is to be frequently done, the force of water will, if not completely, nearly entirely wash it away. The exudation in this way seldom or ever reforms, and the patient makes comparatively a rapid recovery. In cases of a graver character, and where there is a larger collection than usual of inspissated mucus, we clear out the posterior nares by means of a powerful curved leaden syringe which is introduced into the nostril. In the putrid stage, and when the unpleasant odour from the throat is very offensive, a small quantity of Condy's disinfecting fluid added to the water with which we syringe the part has proved of great advantage. I may add that from half a gallon to a gallon of warm water ought, certainly in bad cases, to be thrown into the throat three or four times a day. The ext. belladonnæ applied externally has proved very useful where there has been much swelling.

In cases of very young children where it is difficult to dress and get at the throat, we give the hyposulphite internally, from gr. j. to gr. iij. every four hours, and allow them to swallow the gargle, which, by the way, they very frequently do without permission. Dr. Tubbs informs me he is now giving to adults gr. viij. every four hours. Port wine, beef-tea, brandy, and bark are, of course, given in suitable quantities, and in cases where there was much prostration we have occasionally thrown up, with very satisfactory results, an enema of port wine, beef-tea, and isinglass.—*Medical Times and Gazette, Dec.* 30, 1865, *p.* 714.

36.—TRACHEOTOMY IN CROUP AND DIPHTHERIA.

By Dr. George Buchanan, A.M., Surgeon to the Glasgow Royal Infirmary.

[Dr. Buchanan considers that tracheotomy is a most important means of saving life in the later stages of croup and diphtheria, if performed before the symptoms of diphtheria have been allowed to wear out the patient.]

The following five cases bring the number of my operations up to twenty-six, with the result of nine cures, a proportion sufficiently encouraging, as compared with the published statistics of other operators.

Case 22.—John G., aged six years, had been ailing three days before Dr. M'Gill was called to see him, on the 27th January, 1865. At that time he had symptoms of croup, for which suitable treatment was ordered. He was relieved next morning, but at night got worse, and continued to do so till the evening of the 30th, when I was called to see him. It was quite evident that the tracheal effusion was considerable, as the dyspnœa was urgent; and medical treatment having failed, I proceeded to operate. As usual, the immediate effect of the operation was complete relief to the breathing. He passed a quiet night and next morning was breathing easily. I was, however, uneasy to find that but little secretion passed by the tube, and there was a tendency to dryness in the tracheal membrane, as evinced by a whistling sound. This is a most unfavourable symptom, indicating an extension of the inflammation. A copious secretion, even becoming muco-purulent, necessitating removal of the inner tube very frequently, is a far more favourable condition than that described above. I always feel anxious when the dry whistling sound is present. The tube was frequently cleaned out with a moistened feather; steam was made to pass freely over the patient's bed, but to no purpose. I was not

surprised to find breathing becoming more laborious, the strength sinking, and death taking place on the 1st of February.

Case 23.—On the 22nd of June, Dr. Gray requested me to visit —— Fitch, aged five years, who had been suffering from symptoms of diphtheric croup for a week. On the morning of my visit dyspnœa had become urgent, and was increasing in severity. Dr. Gray's previous experience of the disease had led him to conclude that it had now passed beyond the influence of medicine, and as soon as he formed that opinion he called for my aid. The child was in great distress, and I recommended that the operation should be performed without delay. In this the parents at once acquiesced. The opening of the trachea and insertion of the tube afforded immediate relief, and the progress towards recovery was rapid and uninterrupted. Messrs. Hollings and Gray, two of my students, took charge of the tube for the first two nights, and their reports of the child's condition from the time of operation were most satisfactory. The tube was allowed to remain for ten days, as it caused no annoyance, and the secretion soon became so thin that the effort to cough cleared the tube without much manipulation after the second day. In this, as in other cases, it was observed that milk sometimes ran out of the wound when the patient began to drink; but this was evidently from some of the fluid getting through the glottis, owing to the loss of sensibility or power which often accompanies this affection. A few days after the tube was removed the wound was covered with granulations, and soon completely healed.

Case 24.—This was one of the most interesting and instructive cases I have had. The little patient lived for thirteen days, and the cause of death was inflammation and effusion setting in further down the air passages, after he had recovered from very critical symptoms, which occurred a few days after the operation.

Richard H., aged eight years, began to complain of sore-throat on Friday, the 13th of October. Dr. M'Kinlay of Barrhead, near which place he resided, was sent for, and discovered a diphtheritic effusion on the tonsil, and prescribed chlorate of potash. On Sunday the disease had extended considerably, and that evening Dr. Gairdner saw him in consultation with Dr. M'Kinlay. They both considered it a very severe case of diphtheria. Iodide of potassium was added to the former prescription, and it was agreed that if the larynx became affected surgical aid should be called. At one o'clock on the morning of the 17th a messenger was sent for me, and I drove out to Barrhead, where I met Dr. M'Kinlay, and along with his son we went to see the patient. The progress of the disease was but too evident. The little fellow was in urgent distress, tossing about for breath, and though the pulse was fair, much worn out with restlessness and want of sleep. The steady advance of the symptoms clearly showed that the end was not far off, unless he could be relieved. Dr. M'Kinlay had already explained matters to Mr. and Mrs. H., so little time was lost in preparation. For the first time I gave chloroform in this case, and I was so much pleased with its effect that I would not hesitate to use it in future, although I have hitherto had some doubts of its applicability to tracheotomy. The operation was rather tedious, owing to the great depth of the trachea and some bleeding from small vessels, but I adhered to my rule not to open the trachea till I exposed at least half an inch, which I could see clearly at the bottom of the wound. The chloroform was a great help in this careful dissection. When the opening was made a great quantity of tough substance was expelled, and when the canula was introduced the little patient lay breathing quite quietly, and in a few minutes was fast asleep. I have observed this in almost every case. The patients are so worn out with tossing about in the struggle for breath, that they fall asleep as soon as respiration is rendered tranquil by the opening of the air tube. It was a great reward to see the satisfaction with which the parents regarded even this temporary measure of relief.

Next day I visited the patient along with Dr. M'Kinlay, who had remained almost constantly in the house since the operation. Matters were going on favourably, and breathing was tranquil. I took with me Mr. M'Farlane, one of my hospital dressers, who took immediate charge of the patient, and I was

assisted by Messrs. M'Kinlay, Colquhoun, and Moffat, other students. Their assistance was of the utmost service, as the tube required to be frequently cleansed of the tenacious effusion which repeatedly almost obstructed it. The following day the edges of the wound became erysipelatous, and some emphysema of the neck took place, air escaping into the cellular tissue from the sides of the tube. The swelling partially dislodged the canula, and it could not be replaced in an entirely satisfactory position, so that a good deal of annoyance occurred from this untoward circumstance.

On the fourth day milk and other fluids were observed to come out of the wound along the tube, and I could not determine whether this was owing to the insensibility of the glottis, or whether the displaced canula had made an opening into the œsophagus from the pressure of its point against the back of the trachea. There was much inflammation and swelling of the neck, and consequent increased difficulty of breathing; still I hesitated to remove the tube, as it was frequently obstructed with tough mucus. Iodide of potassium was again ordered, and wine was to be taken at short intervals.

Oct. 23rd. This morning, during a fit of coughing, the tube was expelled, and the patient, though exhausted by the effort, was relieved of much of his distress. When I saw him at mid-day, he was breathing very quietly, had slept a good deal, could swallow easily, and had taken a fair amount of nourishment, the swelling of the neck had much diminished, and he was altogether more comfortable. He could take minced meat, soup, grapes, and wine, and was very willing to do all in his power to help himself.

Oct. 27th. Up till this date the improvement was almost uninterrupted. He slept a good deal, could take nourishment, had himself regularly shifted, and enjoyed the company of his brothers, and could whisper his wants. The swelling in neck almost gone, and breathing took place both through nose and through wound, which still remained open and could easily be kept free of discharge by a sponge pressed against it. His parents, as well as Dr. M'Kinlay, were sanguine of his complete recovery; indeed all the appearances gave ground for their opinion.

On Saturday the 28th, unaccountably, his breathing began to be rapid and laboured, his pulse got weaker, and Dr. M'Kinlay suspected the occurrence of effusion in the smaller bronchial tubes. The opening was still patent and not obstructed by any tough matter, but respiration became more and more feeble. There was no paroxysms of dyspnœa or coughing, but his strength began to sink, he was unable to take wine or other nourishment, and he gradually sank exhausted on Sunday the 29th, thirteen days after the operation.

Case 25.—On the 18th of October Dr. Miller, of Castle-street, asked me to see a child aged 18 months, which he was called to attend two days previously. There were few signs either of diphtheria or croup, but very rapidly symptoms of obstruction in the glottis set in, and increased in severity up till the time I was sent for. When I saw it, the poor little thing was in great distress, but the pulse was fair. Although I formed a very unfavourable opinion of the case, I undertook the operation on the bare chance of saving the child from impending death. It was a very troublesome proceeding, and the introduction of the tube into such a small trachea was accomplished with considerable difficulty, but as usual I left the child asleep and breathing quite quietly. Respiration was free during the night, but next morning the secretion became viscid, and the peculiar whistling sound, so indicative of danger, came on. Pellets of hardened mucus were from time to time extracted from the tube, but soon the removal of these failed to afford relief. Breathing became weak and impeded, the child was soon exhausted, and died the same afternoon.

Case 26.—William K., aged 5 years, had symptoms of croup, a week before he came under my care. He was attended by Dr. Mather, of Bellgrove, and was also seen by Mr. Turner. Medical treatment was persevered with up till the 4th Nov., when, finding the disease still progressing, the above-named medical gentlemen determined to place him under my charge, and sent him to the Infirmary, where he could get more constant attention than

in his own house. I saw him at 8 p.m., and finding well-marked signs of tracheal obstruction, I at once performed tracheotomy. The operation was unusually easy, the trachea being rapidly exposed, and the tube introduced without difficulty. I gave chloroform before operation, and was greatly satisfied with the result, which confirms the experience obtained in Case 24. The little patient fell asleep immediately after the operation, and continued to sleep for several hours. During the night the tube did not need much attention, the inner one being taken out and cleaned every two hours.

No untoward symptom occurred during recovery. From the date of operation, patient continued to breathe easily through the tube, which was very easily kept clean. Several of the students volunteered their services, and each night till the tube was removed two were in attendance, taking watch alternately.

On the ninth day, that is, the 13th of September, I removed the tube, and the child breathed both by the wound and mouth. He could now take plenty of nourishment and was in every way favourable. He was dismissed with the wound completely cicatrized on the 28th November, quite restored to health.

I have only to add that in every case the temperature of the room was kept equable, about 65°, and a free supply of fresh air admitted by occasionally opening the door and window. The atmosphere was kept humid by allowing steam from the spout of a kettle to escape into the room. In most instances it was led by a tin tube to the vicinity of the patient's face, so that the air respired was both warm and moist. I still think this is a safe precaution, and I am sure I have seen good result from it. At all events, when there is much tendency for the secretion to become dry, the moisture contained in the inhaled air softens it and allows of the more easy cleansing of the tube.

I again take the opportunity, which this paper affords, of impressing on my professional brethren the importance of having recourse to tracheotomy in a comparatively early stage of the disease. Whatever may be the primary affection, whether sthenic croup, diphtheritic croup, or genuine diphtheria, death occurs in fatal cases from one of two causes, viz., exhaustion or suffocation. At a comparatively early period in each case, the medical attendant can observe to what type the disease tends; and what I would still urge is that in those cases in which the tendency is to laryngeal or tracheal obstruction, the medical man should advise the surgical operation, when he has found medicine unavailing and before the energy of the patient has been wasted by the progress of the disease. I am the more anxious to press this view on the profession, because I have no opportunity of seeing these diseases in practice at their outset, and consequently have no right to make any suggestions as to their medical treatment. As I restrict myself rigidly to the practice of surgery, it is only where the physician or general practitioner has deemed surgical interference called for, that I have seen much of these fatal affections. I therefore feel justified in calling the attention of those into whose hands is committed the treatment of croup and diphtheria in their early progress, to certain conclusions which have been forced on me while viewing them in what I may call the terminal or surgical stage.—*Glasgow Med. Journal, Jan.*, 1866, *p.* 399.

37.—ON HEPATIC DISEASE AMONGST EUROPEANS IN THE EAST INDIES.

By Inspector-General Sir Ranald Martin, C.B., F.R.S., Physician to the Council of India.

[Sir Ranald Martin states that very early in his course of service in India he became aware of the inutility of mercury in the treatment of chronic enlargement of the liver, and of its baneful effects in the splenic cachexia with hypertrophy of that organ. He therefore employed the nitro-muriatic acid in the form of bath, as follows:]

Directions for Preparing and Using the Nitro-Muriatic Acid Bath.—Take of pure concentrated hydrochloric acid, by measure, three parts; strong nitric acid, two parts; mix the acids very slowly and carefully, so as to avoid any evolution of heat or steam; after half an hour add the distilled water, five parts. Mix the whole carefully.

The Sponging Foot Bath.—1. Of this—the dilute nitro-muriatic acid—three ounces by measure are to be added to each gallon of water, to form a bath.

2. Two gallons of water may suffice for an ordinary foot-bath.

3. The bath thus prepared may be made to keep in use for a few days by adding to it, once each day, half an ounce of the dilute acid and a pint of water, in order to make up for waste in evaporation.

4. A portion only of the bath—say one-fourth—is to be well heated for use, and added to the remainder, so as to make the whole up to 96° or 98°.

5. Glazed earthen or wooden vessels should be used for baths; and the sponges and towels kept in cold water, lest the acid corrode them.

Manner of Using.—6. Let both feet be placed in the bath, while the inside of the legs and thighs, the right side, over the liver, and the inside of both arms, are sponged alternately; or let the abdomen be swathed in flannel soaked in the bath fluid. The process should be continued for half an hour morning and evening.

7. While using the bath, a gentle saline aperient, as Cheltenham or Epsom salts in some bitter infusion, or else Pullna water, should be taken every other morning; and should there be dryness or harshness of skin, a vapour bath at 100° or more, used twice a week, will be found of much service in stimulating and opening the pores, and in purifying the surface of the body.

The General Bath.—8. In urgent cases, a general bath to envelope the whole body should be used, the proportions of the dilute acid and water being continued as above stated, adding one ounce of the dilute acid and two pints of water every day to make up for waste in evaporation.

9. The acid mixture forming the bath should be heated in earthen vessels, such as large pipkins; and the temperature of the bath should be measured by the thermometer at from 96° to 98°, as the body will be chilled by a degree of warmth which feels comfortable to the hand.

10. When the general bath is used, the patient before going into it should be covered over with blankets until a gentle perspiration is induced.

11. When in the bath, a covering-blanket should be drawn over the head and shoulders to confine the steam, and enable the patient to inspire it. A nightcap should be worn to protect the head from damp.

12. Before quitting the bath, the bedclothes, under-vests and drawers, and the towels should be ready warmed; the body to be dried while standing in the bath, and the dressing to be performed immediately, in a room well warmed.

13. Where the acid-bath excites much irritation of the skin the quantity of the dilute acid may be diminished; and where irritation of the gums with general malaise occurs, the use of the bath may be relinquished for a time, resuming its application if necessary, when the above symptoms have subsided.

14. The bath should be made of well-seasoned wood, the pieces dovetailed, and, if nails or screws be used, they should be well covered, and the crevices be made water-tight by putty, a layer of paint being placed on the outside.

15. The bath should be no larger than is absolutely needed to contain the person, with so much fluid as will cover it up to the neck. The height and breadth of the person should be carefully measured.

The following dimensions were used by a person 5 ft. 4 in. in height:—Inside length at the top, 5 ft. 4 in.; inside length at the bottom, 4 ft. 1 in.; inside breadth at the head, 1 ft. 4½ in.; inside breadth at the foot, 1 ft.

The physiological actions and uses of the aqua regia, in the form of bath, were, towards the end of the last century, brought to the notice of the profession, in the treatment of chronic diseases of the liver, by the late Dr. Helenus Scott, of Bombay, who believed that there was "a correspondence in

the effects of the two remedies"—namely, the acids and mercury. Early in the present century, Dr. Scott made trial of his remedy on the person of Gen. Wellesley, then arrived at Bombay, so far on his way to England, the medical officers having declared such a measure necessary to the restoration of his health. His disease was enlargement of the liver; and as he stated to his friends that he had then no desire to go home, they recommended their physician and his new means of cure to his attention. General Wellesley followed the advice of his friends, and in less than two months he returned to his command in complete health, and afterwards fought the battle of Assaye. This event was, in fact, the turning point of his fate.

Dr. Scott, who had for years employed this remedy in his own person and in those of friends suffering from hepatic enlargement, at length published the results of his experience. He stated that in a constitution broken down by disease, by the use of powerful remedies, such as mercury, or by the long continued action of the poison of syphilis, the acid treatment was quite as beneficial in the climate of England as in that of India, and the observation has been confirmed in our own time by other authorities.

"I know of no other means," he says, "that are capable of producing effects at once so salutary and so considerable, so free from injury, with so little inconvenience or disturbance.....As with mercury, the system should be charged with it for a longer or shorter time, according to circumstances. In short, and as a general rule, I have found the acid bath advantageous and salutary in all cases where mercury is useful, and with the additional advantage that the acid treatment is attended by neither injury nor disadvantage. *By the harmless remedies that I now recommend, much good may be done in some diseases that are acknowledged to be beyond the ordinary means of relief.*"

"Even in irremediable cases of chronic enlargement of the liver," says Dr. Christison, "it proves useful in cleaning the tongue, improving the appetite, abating thirst, and sometimes in retarding the progress of disease."

"The rule," says Dr. Bence Jones, "is, sulphuric acid to astringe, hydrochloric acid to promote digestion, nitric acid to promote secretion."

The external application of the acids in the form of bath is, I repeat, by far the most effective mode of using this most valuable remedy; but I have observed that where the internal exhibition is preferred, a far longer time is necessary to the cure. For the dispersion of the products of inflammatory effusion into the viscera, whether the subject be anæmic or otherwise, the use of the bath proves of most excellent effect, and its resolvent power is often accelerated and increased by alternating its use with the alkaline renal depurants, or with taraxacum and bicarbonate of soda, or eliminants, and as means to saponify the ducts. Dr. Jephson, of Leamington, has assured me that in diseases of the mesenteric glands, and in swellings of the cervical glands also, he has found the swathing of the abdomen and the neck with muslin soaked in the warm solution of the acids, as used by me for the bath, of surprising efficacy, only covering the damped muslin with oiled cloth.

Dr. C. J. B. Williams considers nitric acid to be the best medicine he knows for the state of convalescence from inflammation, and in the various cachectic states following on acute disease or on habits of intemperance; and offers the suggestion, that its beneficial influences may be referred to its possessing a large proportion of oxygen in loose combination, and that it thus promotes a more free circulation through diseased parts, or through effused solids, by further oxygenating them. This distinguished physician, speaking of the nitro-hydrochloric acid and chlorate of potash, says; "It seems most probable that these agents are chiefly useful in supplying to the blood the oxygen necessary for the formation of fibrin or deutoxyde of protein, the respiration in its weakened state being unable to furnish a due amount."

But, however the physiological actions of the acids may be interpreted, the fact has long been established in my experience, that chronic diseases of the liver, with or without general cachexia, is absolutely curable by the

remedies in question, while they have resisted all and every other means in ordinary use.

The powerful curative effects of the nitric and muriatic acids, in certain forms of stomach disorders, have often been commented on by medical writers and practitioners; and it is probable that many of the cases thus noted have been complicated with hepatic disorders. The general fact, Dr. Budd says, has been fully established by Dr. Prout. "Prout found them of especial efficacy in the gastric disorder that occurs in what is termed oxalic diathesis; and that is marked chiefly by distressing flatulence, and palpitation or irregular action of the heart, occurring some time after meals, and by the presence of oxalate of lime in the urine. The mineral acids are often useful to persons in whom digestion is habitually slow and feeble from a scanty secretion of gastric juice, and who have a sense of weight or oppression of the stomach after meals. They are often useful, as Pemberton showed, in the indigestion, attended with excessive formation of lactic acid, that occurs in weak and nervous persons, and where the stomach has been for some time disordered and weakened by a source of irritation elsewhere."

M. Trousseau has for years exhibited the hydrochloric acid after meals, he says, with much advantage. In an anæmic patient afflicted with obstinate chronic diarrhœa, he gave chalk at the commencement of the meals, and hydrochloric acid after them, with the result of a complete cure. He adds: "I do not wish to go beyond the fact, and only repeat that in the different forms of dyspepsia connected with chronic affections, whether of the thorax or abdomen, hydrochloric acid, taken after meals, may lead to therapeutic results deserving attention."

When the bowels are inactive, and the elimination by the acids unaccomplished, I recommend a mild aperient every other day, aided sometimes by colchicum. In moderate doses, the preparations of colchicum, added to taraxacum, when used with the acids or the alkalies, form a valuable combination in either case. In persons suffering from hepatic disease, and who are of a gouty or rheumatic habit, the advantages of the addition need not be insisted on.

Referring to the use of local and counter-irritant means, it is here deserving of mention that the first Dr. Jenner employed strong tartar-emetic ointment to the right hypochondrium. This he used perseveringly in chronic enlargement of the liver, and with marked benefit, as he declared.

The disuse of so powerful a means as the nitro-muriatic acid must be mainly ascribed to the brief, imperfect, and desultory manner in which it has generally been employed, whether internally or externally prescribed. It is only by those who have attentively observed the effects of this remedy during months together, in many cases, that its salutary influences in bringing about the removal of disease and in maturing convalescence can be justly appreciated. Certainly, had mercury been used in the irregular and careless manner spoken of, without regard to rational persistence or curative result, it never could have attained to any repute in the practice of medicine, in the treatment of acute or chronic disease.

The nitro-muriatic acid will occasionally, like mercury, irritate the mucous surfaces; and, like mercury, it acts on all the secreting organs; irritation of the gums and fauces, of a very harmless nature, occurring only after a protracted use. It is unlike mercury in that, after an observation of its actions and uses during more than thirty years, I am unable to recollect any one instance of its injuriousness.

When the patient is in so weakened a condition as to be unable to bear the immersion or the sponging, I have recourse to a swathe damped in the solution, worn round the body, and covered with oiled silk. This may be continued for any length of time, and great advantage is often the result.—*Lancet, Dec.* 9, 1865, *p.* 641.

38.—ON WHEAT PHOSPHATES.

By Dr. Tilbury Fox, London.

[Several years ago Dr. Hake suggested the production of a preparation containing the organised chemical products residing in the outer layers of the wheat grain, believing they would be found to possess peculiar nutritive qualities. During the last few years Dr. Fox has used these *wheat phosphates*, as the preparation is named, with great success.]

Whilst the subject of infants' food has, on account of prolific inventions and suggestions, become rather a bore, the tendency of refinement has been in the wrong direction, and very nice looking products, pleasing to the eye, but useless for nutrition, have been obtained. The various forms of infants' food are in a great majority of instances simply and purely starch, the central portions of the cereals consisting entirely of the latter products; these foods do not deserve the name of, and *are not, flours;* to call them flours is nothing more or less than a *fraud*. Now, inasmuch as the starchy element is not the assimilative nor the flesh forming, but the heat and fat producing principle, all our past efforts to secure a nice white flour have been antagonistic to the possession of nutritive material, and actually the very desirable part of the grain contained in the bran—viz., the organised phosphates and other principles, have been deliberately rejected. Seconds flour makes a much more wholesome bread than that of the first quality.

The importance of a due supply of phosphates in health and disease has been fully recognised at the present day, and as a result we now possess various pyro-, super-, and hypo phosphates and phites.

My experience teaches me that there is something essentially special in the organised phosphates—those in fact *which have been formed by passing through a living organism* (in Nature's own laboratory)—as compared with artificially prepared phosphates. It is not the amount, but the kind exhibited that produces the good result. No simple mixture is in any way a substitute. It is not at all unlikely that the cerealin, of which little is known, but which is associated with the phosphates in the bran, and has an action similar to pepsin, may conduce to the beneficial result. A similar kind of action is observed in those cases where quinine fails, but decoction of bark instantly succeeds. The organised phosphates I speak of aid the assimilative function, and I find that ordinary foods are digested, and even medicines—for example, iron when given in combination—act most efficiently after they have previously failed alone. The same is the case with ordinary food in weak digestions. Many of the "foods" recommended are really animal in character—such are the extracts of meat. Nature distinctly points to the agent now under notice as a preferable aid, in virtue of its vegetable origin. The wheat phosphates, too, contain the desirable properties of brown bread without the objectionable ones—viz., the cuticle and husk.

The mode of preparation is simply to make a decoction of well-selected bran, carefully evaporate in a water bath, mix the residue with sugar, and reduce to powder. It may be used in the place of sugar, a teaspoonful or less being added two or three times a-day to the child's food. The cases in which its use is chiefly indicated are those amongst the young, in whom the assimilative function is at fault. I can speak very strongly in cases which belong to my own particular specialty—diseases of the skin. Eruptive diseases of the scalp in infants are most frequently associated with faulty assimilation. Here the wheat phosphates act marvellously well. But in rickets, in marasmus, chronic diarrhœa, and impaired nutrition of all kinds, I believe them to be most valuable adjuncts. Pallid children pick up tone, colour, and flesh; worms disappear; intestinal irritation subsides; the secretions become healthy; and disease goes. I lay great stress on the phosphates under notice in their character as *organised products* as greatly helping assimilation of food and medicinal agents, and believe them to be the

most preferable form of phosphates, especially for the young.—*Med. Times and Gazette, March* 17, 1866, *p.* 278.

DISEASES OF THE URINARY ORGANS.

39.—ON THE SYMPTOMS ARISING FROM ALKALINE URINE.

By Dr. H. Bence Jones, F.R.S. &c.

The bladder in health contains a feebly acid saline fluid. If the acid or salts vary greatly on either side of the usual amount the bladder is irritated by the unaccustomed stimulus of pure water or excess of salt. Thus a very strong solution of salts in the urine, or very dilute or almost watery urine, cause irritation. Highly acid urine and, above all, ammoniacal urine have the same effect. Increased irritation causes increased secretion, and the mucus cells, when thrown out in quantity acted on by the carbonate of ammonia, become ropy and adhesive; like the ordinary alkaline mucus from other mucous membranes, as from the nostrils. If the irritation is continued pus is formed, and these cells also when acted on by carbonate of ammonia become ropy, and this has also been included under the term of ropy mucus. According as more or less earthly phosphates are present—that is, according as more or less magnesia or lime is taken into the system, more or less phosphatic precipitate is mixed with the ropy mucus or ropy pus, and this chemical error mounts up to the formation of mechanical masses of mortar-like or stony substances, which cause excessive pain either in passing through the irritated urethra or in scraping the inflamed surface of the bladder, or even by precipitation within the mucous membrane of the bladder itself.

The constant muscular contraction to expel the irritant is more disturbing to sleep than the cough of bronchitis; and hence loss of rest becomes sometimes the most serious consequence of the complaint. Next to this is the extension of the inflammation upwards to the pelvis of the kidney. The ureters may show or not show that they have been inflamed; sometimes no trace can be found of the passage of the inflammation upwards, but pyelitis and tubular nephritis producing pus throughout the kidneys and uræmia throughout the body may put an end to the progress of the disease by destroying life.

The inflammation may spread downwards, and then the urethra suffers, and often violent orchitis is set up.

If the neck of the bladder and the prostrate get thickened by interstitial deposit of fibrin, the bladder is unable to empty itself, and the fluid that remains is in a variable state of more or less complete putrefaction. If the fundus is chiefly thickened by deposit and increase of the muscular coat, it will contain less and less fluid as the thickening increases; from either cause the frequency of the calls increase, and when ulceration takes place, the amount of pain with each contraction becomes excessive.—*Med. Times and Gazette, March* 3, 1866, *p.* 220.

40.—ON THE TREATMENT OF AMMONIACAL URINE.

By Dr. H. Bence Jones, F.R.S., &c.

The treatment of ammoniacal urine resolves itself into keeping the urine acid and stopping the inflammatory action in the mucous membrane of the bladder; and as the alkalescence by its chemical action keeps up the inflammation, the stoppage of the formation of carbonate of ammonia in the bladder becomes the main object to be attained.

Usually, as the urine comes into the bladder, it is acid; as soon as it gets there it finds some ammoniacal urine there already; this not only neutralises the acid that comes from the kidney, but sets up decomposition of the urea,

and forms fresh carbonate of ammonia; so that the removal of the ammoniacal urine that remains in the bladder is absolutely necessary, unless sufficient acid can be made to come from the kidney to neutralise it.

Vegetable acids far more quickly run off in the urine than mineral acids, of which sufficient cannot be taken to pass in quantity through the kidneys. Of all the acids lemon juice is perhaps the best which can be used. Many years since Mr. Witt made the following analysis of lemon juice for me (see *Quarterly Journal of Chemical Society*, vol. vii., p. 44):—One ounce (480 grs.) of lemon juice contains only 1·728 grs. of inorganic constituents; of which potash, sulphuric and carbonic acids constitute three-fourths; phosphoric acid, soda, and lime, with traces of silica, and iron and magnesia, constitute the other fourth. Hence water and citric acid constitute by far the largest part of the lemon juice, the citric acid being, on an average, about 40 grains to the ounce of juice.

It is highly probable that citric acid and other vegetable acids have an anti-inflammatory action (lessening the oxidation that is going on) wherever they are carried, and they not only pass to the kidneys, but probably diffuse into every part of the mucous membrane of the bladder; to a small degree lessening the alkalescence of the textures in which the peroxidation is going on.

The citric acid must not be taken in such quantity that it irritates the stomach and bowels, nor should it set free uric acid in the urine. So that the highest limit can be easily recognized by the bowel symptoms. The lowest limit is so much as will just make the urine acid. This cannot always be reached. Three, four, five, ten lemons or more daily will not neutralise the alkali in the urine, and will not so stop oxidation and nutrition that the body will become feeble and wasted. Before this effect is produced some other way of removing the ammonia must be tried. Taking away the alkaline urine by the catheter so frequently that a smaller quantity of lemon-juice will keep the water acid is easy when the passage of the catheter does not cause mechanical irritation to take the place of the chemical irritant. The catheter must be used more or less frequently, according to the reaction of the urine. If drawing off the water at night whilst taking lemon-juice or other acid keeps the urine feebly acid, this is all the mechanical means required; if not, then, twice, thrice, four times daily the water must be drawn off; and even, in extreme cases, the catheter must be kept in the bladder, so as to free the urine continually without the irritation of the passage of the instrument; but the presence of the foreign body in the bladder, except when perfect rest is observed, often causes so much irritation that it is an aggravation of the disease. Moreover, by washing the bladder with small quantities of hydrochloric or acetic acid and water, after the urine is drawn off with the catheter, the ammonia may be easily neutralised, and a very small quantity of feebly acid fluid may be left in the bladder to counteract the decomposition of the urea, and to make the surface of the bladder itself less foul. How frequently the washing should be repeated, and what the strength of the acid injected should be, ought to be determined by the reaction of test-paper or the smell of the urine. Whatever you do, remember that the object is to get rid of a chemical irritant, and that if by rough handling of the catheter, by its too frequent use, or by too strong an injection you mechanically or chemically irritate the bladder more than it is irritated chemically by the ammoniacal urine, no good will come from your treatment.

When you have removed or can prevent the ammonia of the urine, then balsamic remedies, as in bronchitis, are often useful. These pass from the kidneys and also directly from the blood to the mucous membrane. Buchu, cubebs, copaibæ, turpentine, eau de goudron (purified tar water), all these may be useful; remembering here, also, that if the stimulant be too great, harm may be done by setting up fresh action. Astringents, as any preparation of gallic acid, tannic acid, uva ursi, rhatany, catechu, matico, alum, lessen the secretion of pus and mucus by contracting the textures.

Tonics also often are useful when the acute symptoms subside—as quinine, biberine, pereira brava, and other bitters.—*Med. Times and Gazette, March 17, 1866, p. 275.*

41.—ON THE SYMPTOMS AND CONSEQUENCES OF INTERSTITIAL NEPHRITIS.

By Dr. H. Bence Jones, F.R.S., &c.

If the disease could be limited to the interstitial texture, and to that alone, it is possible that no appearance of albumen in the urine might occur. No fibrinous casts might be seen. The kidney might gradually, by its contraction, cease to secrete urinous matter, and uræmia would be the final result. But the affection spreads to the vessels and tubes, and then the pressure in the tufts is altered and albumen is effused, the production of epithelium is increased, and some fibrinous casts appear in the urine.

Usually the specific weight of the urine becomes less and less, and generally this weight may be taken to be directly proportional to the amount of cortical structure that exists; so that when the specific weight of the urine is least the cortical structure is least also. Even when hardly any cortical structure is left, an excess of water of the lowest specific gravity may be thrown out, most probably from increased circulation in the mammary part of the kidney.

As long as this excessive flow continues no dropsy occurs, and hence throughout the whole course of the disease no dropsy may occur. The altered blood and the altered lymph may give rise to all kinds of hemorrhages, thickenings, and effusions, and chemical actions on the nerves and muscles, which may be summed up in the word poisoning, and which I have already brought before you when speaking of uræmia.

At any period during the progress of the disease more acute inflammation may suddenly come on, and in the cellular tissue, more readily even than in the tubes, pus-cells are readily formed, suppuration extends throughout the altered cortical structure, and rapidly that assemblage of low feverish general symptoms which now appears to us to be so inaccurately designated as a typhoid state, is produced.

With regard to treatment, medicine avails little for stopping the effect of alcoholic poison or for removing the thickening of the interstitial structure. No appreciable alteration of structure occurs until the alcohol has acted for months; but as each day produces its infinitesimal effect, some good may be done by recognising the disease early and by stopping as far as possible the further action of the poison. Usually the second nature cannot be changed, and the treatment consists then only in alleviating symptoms and in warding off complications as they arise.

Of all the symptoms that of general debility is the one which will most frequently present itself to you, and of all the complications uræmia is the one which is most serious.

The loss of general mechanical power consequent on wrong chemistry of the blood and lymph begins from the very commencement of the disease, and lasts until its final close.

One of the first effects in the blood is the insufficient production of blood globules, on which the passage of oxygen into the blood depends. The gradual diminution of albumen in the blood is a very much more remote cause of symptoms, and as long as food can be taken this admits of an easy remedy; whilst the reproduction of blood globules requires a far more complex chemistry, and necessitates an increased supply of iron for the formation of hematosine.

The accumulation of descending substances in the blood, and still more in the lymph which surrounds and permeates each particle of every structure, must begin from the moment that the kidney begins to perform the function of oxidation and excretion imperfectly. Still it is evident from the experiments in which one kidney is removed and poisoning occurs that even half the structure may be destroyed by disease without the occurrence of bad symptoms. This is a large margin for safety, but as soon as it is removed the lymph begins to cause wrong chemistry in the textures. Products of

downward change from urea upwards keep circulating in the blood and lymph, and these substances produce their chemical actions everywhere. Thus on each mucous and other membrane urea exudes, and in the mouth even it is converted into carbonate of ammonia, and this, with some urinous smelling substance, gives the foul breath that is so frequently observed. The same substances effused in the stomach gives rise to sickness, and in the bowels often to diarrhœa. The higher uric acid compounds in the joints produce chronic gout, and the excess of kreatin in the muscles may stop the changes in the syntonin, whilst the still higher compounds as yet uninsulated may in the nerves and brain produce irritant and narcotic symptoms from the slightest cramp or subsultus to the most profound coma. That there is in every case some distinct local action is clear from the amendment or arrest of the symptoms which is occasionally observed. The sickness and the diarrhœa may be stopped, the convulsions may cease, the coma may disappear.

Let us look at the removal of the poisonous substances a little closer. The vomiting and diarrhœa are not to be checked by strong remedies when the ammoniacal and urinous odour tells you of the accumulation of poisons within. You must, whilst endeavouring to obviate the extreme prostration which these actions cause, remember that more serious muscular and nervous actions are at hand, and that except by acting on the skin, you have no other gate through which the poisonous substances can pass out. Warm baths and hot air baths give no immediate relief like vomiting and purging. I have been asked by a patient for an emetic when he was within a few moments of his death from exhaustion. Almost whilst speaking to me another violently relaxed action of the bowels came on, and from the effects of this he died.

By the stomach, by the bowels, and by the skin, all poisonous substances can be thrown out of the blood and lymph, and, according to circumstances, by this or that road a passage must be obtained.

Irritative actions of the nervous system seem to mark a less degree of affection of the nerves than narcotic actions, and cramps, subsultus, and epileptiform convulsions pass away or yield to treatment far oftener than oppression and coma.

To any who have witnessed the stoppage of convulsions in Bright's disease by the inhalation of chloroform, the thought must have occurred that the chloroform must have so acted directly on the nerve as to render it incapable of being irritated by the urinous poison. A volatile coma, so to say, is produced. Perhaps the local action of the urinous poison is checked by the substance on which it was acting being altered in composition by the chloroform. But when the urinous poisons are causing both convulsions and coma, then this fixed coma is more dangerous than the convulsions; and all your efforts must be directed to stop the local action on the nerves, and whilst doing your utmost to evacuate the poison, you must try, by counter-irritation to determine by blisters the largest possible effusion of serum and lymph as near as possible to the spot where the wrong action is taking place.—*Med. Times and Gazette, Jan.* 6, 1866, *p.* 1.

42.—ON THE TREATMENT OF TUBULAR NEPHRITIS.

By Dr. H. Bence Jones, F.R.S., &c.

[Tubular nephritis is to the kidney what bronchitis is to the lung. In its comparative mildness, in its occasional intensity, in its long duration, and in its rapid and perfect cure it resembles slight, severe, chronic, and rapidly curable cases of that disease. Cold is the most frequent exciting cause, then scarlet fever. In the treatment of the disease,]

The first indication is, as far as possible, to remove the causes of the attack. When cold is the cause, warm baths, vapour baths, and warm clothing may help to restore the action of the skin. Tartarised antimony has no strong action on the kidneys, and has a very decided action on the skin; and when

it acts on the bowels or on the stomach, causing vomiting, it removes urinous substances from the blood. Hence it is a most important remedy in the acute dropsy from cold. Even when poisonous substances are passing out of the blood, as after scarlet fever, cantharides, turpentine, and in extreme cases tartarised antimony may sometimes be used. In slight cases simple dilution with the purest water washes the impurity out of the blood.

The second indication is to stop the thickening of the tube, and to relieve the obstructed vessels. Abstraction of the blood by cupping glasses on the loins can hardly be expected to effect the circulation in the tubes of the cortical structure of the kidney; whilst general bleeding, even to a few ounces, has a distinct effect on the pressure of the blood in the malpighian tufts. Hence, if blood at all is taken, it should be by venesection. Digitaline in small doses acts on the nerves that regulate the circulation rather like a stimulant, but in large doses or long continued the pressure on the arteries is diminished; and of all diuretics this is the only one which is admissible in acute tubular nephritis. As the disease becomes more chronic iodide of potassium may be used in diuretic doses. In ten minutes after the first dose it is present in every part of the kidney, and probably may be found there for many days after the last dose has been taken.

The last indication is to relieve the symptoms and complications that occur.

Of these, the dropsy is the most important. In the acute stage you have to contend with the urinous dropsy. Strong vapor baths or hot-air baths, used to the greatest extent that the strength will bear, are most efficacious. Strong action on the bowels by those watery cathartics that have the least action on the kidneys—as, for example, jalap, gamboge, and elaterium. If the strength admits of it, emetics, tartarised antimony, and ipecacuan may be given.

When the acute stage is over, the tendency to anæmic dropsy begins. To prevent this, and after the anæmic dropsy is set up, small quantities of iron should be given. Nitre, cream of tartar, broom tea, and other diuretics may be used, sometimes in very large doses. Even tincture of cantharides may, in the more chronic state, be prescribed. In a very short time the whole of the anæmic dropsy may be removed, and then iron in larger quantity should be given to prevent a return of the effusion.

The treatment of the secondary inflammation should be carried on with allowance for the general debility which will appear when the increased arterial action subsides.

Counter-irritation is to be preferred to local bleeding, because it saves the red blood. Mercury is hardly ever to be used, as in this form of disease its poisonous action is most quickly and violently set up.—*Med. Times and Gazette, Jan.* 13, 1866, *p.* 32.

43.—ON SECONDARY ALBUMINURIA (CAUSE NOT PRIMARILY IN THE KIDNEY).

By Dr. George Harley, F.R.S., Professor in University College, and of University College Hospital.

Albuminuria from Imperfect Digestion.—Every one who has had much experience with diseases of the urinary system is aware that cases of temporary albuminuria are occasionally met with in persons whose only symptoms are referable to disordered digestion. It may be that they are usually dyspeptic, or, what is still more frequently the case, they have been indulging in food that has disagreed with them. Thus, cheese has been known to cause albuminuria in children, and lobster or crab in grown-up people. In these cases there is nothing essentially wrong with the kidneys, and the albuminuria is simply due to the endosmotic equivalent of the albumen absorbed into the circulation being different from what it ought to be. Certain kinds of albumen, as before said, will not stay in the blood unless they have previously

undergone modification by the digestive process. Hence, if the necessary modification be interrupted, either on account of the digestive organs being out of order, or by reason of the indigestible nature of the food itself, a temporary atttack of albuminuria is the immediate consequence.

The effect of food on albuminurias of all kinds is much more important than most persons imagine; for, as I shall afterwards show when on the subject of treatment, by diet alone we can at pleasure augment or diminish the amount of albumen eliminated with the urine. Nothing further need therefore at present be said on the subject.

Albuminuria from Nerve Lesion or Reflex Irritation.—This, like the last form of the affection, is characterised by the absence of organic change in the structure of the kidneys. But the analogy does not proceed further, for although the albuminuria is due to no structural change, it can, nevertheless, be clearly traced to a disturbance of the renal circulation. It may be remembered that while on the physiology of the subject I mentioned that in the lower animals albuminuria can be produced at will by a variety of operative procedures—such, for example, as section of the renal nerves, division of the spinal cord, lesion of the cerebral peduncles, &c. So we cannot be in the least surprised at the detection of albumen in the urine of patients labouring under similar nerve lesions, the result of disease. As the cause of the albuminuria is apparent enough in the lower animals—namely, renal congestion—there need be, I think, but little doubt that in man the pathology of the morbid condition is precisely similar. Cases of albuminuria in connection with spinal paraplegia, hemiplegia, apoplexy of the base of the brain, &c., have been so frequently reported, that nothing further need be said upon the subject, but we may at once pass on to the consideration of that form which is least understood—viz., the albuminuria of pregnancy.

Albuminuria of Pregnancy.—Many years ago, when House-Surgeon to the Edinburgh Royal Maternity Hospital, I was led, at the suggestion of Professor Simpson, to examine the urine of every patient that entered the charity during three months. The urine of those who came in after the pains of labour had already begun was immediately drawn off by the catheter, so that even in their cases all risk of error, from the accidental admixture of vaginal secretions, was avoided and I was surprised to find how much less frequent the presence of albumen in the urine of pregnancy is than I had been led to expect from the opinions expressed by previous observers. It did not amount to 4 per cent., which is actually less than a third the percentage of the cases of temporary albuminuria found to exist among female patients admitted into a general hospital. As regards the frequency of puerperal convulsions, an almost similar conclusion was arrived at—namely, that they are not so exceedingly common as generally supposed, for although I have seen them both in private and public practice, not one case of the kind occurred among the 450 women who were at that time delivered under my superintendence, and at least ten of whom were passing albumen in the urine. Moreover, even in those cases where the albuminuria is most severe, accompanied by dropsy, and apparently entirely due to the pregnancy, there may still be no convulsions, as the following case will show:—On May 30, 1851, a servant-girl, aged 20, was sent into the Edinburgh Infirmary by a medical man who thought her case was one of Bright's disease. Her urine was highly albuminous, her face puffy, her legs swollen, and her abdomen greatly distended. On passing my hand over the abdomen I imagined that I felt an enlarged uterus, but on suggesting pregnancy, the girl indignantly denied its possibility. It was true that she had seen nothing for the last nine or ten months, but to that she attached no importance, as she had often before been irregular. She had never had morning sickness, never felt any movement, and besides, it was impossible she could be pregnant. Facts, however, are stubborn things, and fœtal hearts being not usually heard in cases of simple ascites, I quietly, but firmly, told her my opinion that the albuminuria was associated with, if not the direct result of pregnancy; at the same time adding that she might remain in the hospital a few days while we tried to relieve the dropsy. In four days she was taken in labour, and within an hour after I

was called to her bedside she was delivered of a full-grown healthy child, without a single convulsion or any head symptom whatever. From this time the albumen began to diminish in the urine, the dropsy to disappear, and on June 16, twelve days after delivery, not a trace of matter coagulable by heat and nitric acid was to be found in the urine. On July 14 she was dismissed in apparently perfect health.

[Dr. Harley then relates two other cases, and after detailing the results of various investigations, draws from them the following general conclusions.]

1st. That in the albuminuria of Bright's disease there is less than the normal amount of albumen in the blood.

2nd. As the amount of the albumen in the urine increases, the quantity in the circulation proportionately diminishes; and as the albuminuria decreases, the amount in the circulation gradually reapproaches the normal standard.

3rdly. The quantity of albumen and urea in the urine are in inverse proportion to each other—that is to say, where there is much albumen there is little urea, and where there is much urea there is but little albumen.

Lastly. The condition of the urine affords us a pretty correct idea of the probable condition of that of the blood.

I have now to point out an error into which people are very liable to fall—namely, to look upon the albuminuria of pregnancy, and pregnancy with albuminuria, as one and the same thing, while, in reality, they are perfectly distinct. When we speak of the albuminuria of pregnancy, we mean that the pregnant state induced the albuminuria, as was the case in the example first cited; whereas when we speak of pregnancy with albuminuria we simply mean that a woman during the period of her pregnancy has been attacked with kidney disease.

I have now to call attention to a third form of albuminuria and pregnancy—namely that in which a patient already the subject of kidney disease becomes pregnant. This is not only a graver complication than the "albuminuria of pregnancy," but, as a rule, even more dangerous, both to the life of mother and child, than a case of pregnancy complicated with kidney disease. The following case, of which I can only give a very brief abstract, will illustrate this remark:—

The subject was a lady, aged 30, whom I saw in consultation with Dr. Magrath, of Teignmouth, and to whom I am indebted for the patient's history. Her mother died of diseased kidneys at the age of 45. The patient herself was healthy until she suffered from frequent attacks of intermittent fever whilst abroad. During her first pregnancy her legs were noticed to be œdematous, but, as albuminuria was not suspected, the urine remained unexamined. Her delivery was followed by a convulsion, for which she was bled to sixteen ounces. During the second pregnancy and confinement, nothing remarkable was observed, but after the third delivery she was again seized with convulsions, which were in this occasion arrested with chloroform. In the early part of 1863 she again conceived, and almost immediately before or afterwards, was, while recovering from a catarrhal fever seized with hemiplegia. In the following month she was seen by Dr. Brown-Sequard, and, under the free administration of tonics, with change of air, she gradually regained the power of her arm and leg. In August of the same year, shortly before I saw her, her face and hands were for the first time observed to be œdematous. The urine was of a specific gravity of 1020, and was not noticed to contain albumen. About this time she suffered from headache, vomiting with stertorous breathing, and muscular twitchings during sleep. At the end of August, when I examined the patient, the specific gravity of the urine was 1014, contained a large quantity of albumen, coagulating to one-third of its volume, and with the microscope both blood corpuscles, and renal tube-casts were detected. As the symptoms of uræmic poisoning continued to increase until the breath had a urinous odour, the induction of premature labour was had recourse to, during which she had a convulsion, which was arrested with chloroform. After this the patient made a slow recovery until October 24, when she was considered convalescent. The urine at this time

was abundant, of a specific gravity of 1014, contained very little albumen, and only a few hyaline tube-casts.

On November 25 the quantity of urine was forty ounces and the specific gravity 1021. Waxy tube-casts were still detectable.

In this case, as in the preceding, the union of pregnancy and kidney disease must be looked upon as accidental. In the former the albuminuria supervened during the course of the pregnancy; in the latter, the pregnancy in the course the kidney disease, each being quite independent of the other. The distinction to be drawn between these cases and that first cited—namely, the albuminuria of pregnancy—is of the utmost importance both as regards prognosis and treatment; for while in the albuminuria of pregnancy it is to the pregnancy, and not to the kidneys, that we must look for relief, in the case of kidney disease associated with pregnancy, as well as in that of the pregnancy associated with kidney disease, it is to the condition of the kidneys that we must specially direct our attention. The reason of this is easily understood when we remember that in the albuminuria of pregnancy the albumen rapidly disappears from the urine after delivery, whereas in the others the mere circumstance of delivery influences the albuminuria only in a secondary degree. The patient must be as carefully treated for diseased kidneys after the expulsion of the child as during its sojourn in the uterus.

As regards puerperal convulsions in cases of albuminuria, as far as my own experience goes—and I have nothing else to guide me—they are far more frequently the result of diseased kidneys than of the simple albuminuria of pregnancy. Convulsions, as we well know, frequently occur without pregnancy, and we cannot be surprised at their occurrence when pregnancy is superadded to the albuminuria. In the latter case the convulsive attack may be delayed until the time of delivery, or it may occur at any period of the pregnancy. There can be no doubt that albuminuria, from whatever cause, predisposes to epileptic convulsions, just as any other disease which impoverishes the blood, and thereby leads to malnutrition of the nervous system; for malnutrition of the nervous system is of all things the most fruitful source of epilepsy. In the second case cited we had ample proof of the impoverishing influence exerted upon the blood by albuminuria; and the absence of convulsions in it might probably be attributed to the circumstance that the blood had almost entirely regained its normal condition at the time of delivery.

That epileptic convulsions may occur in complicated albuminuria in the predisposed I see no reason to doubt; and a well-marked case of this kind, and which is briefly as follows, fell under my notice in 1851:—

A dark-complexioned woman, of modern development, who had been for nine years a strumpet—she was then 28—was admitted in April 28 into one of the wards under the charge of Dr. Halliday Douglas. She had a dull, languid, and waxy appearance. Her urine was albuminous, of a specific gravity of 1008, and for several days after admission averaged 100 ounces per diem. She said her illness began with dyspeptic symptoms eight months before, and that shortly afterwards she had a fit, which had since recurred about once in every three or four weeks. On May 11—that is to say, thirteen days after her admission—she had a fit which lasted an hour, and from then until she died—seven days later—she had one, two, or more every day. She died immediately after coming out of one. From the 11th to the 18th she was in a state of stupor, and was not easily roused out of it; but when awake she answered questions correctly. The urine fell from 100 to 50 ounces a-day during the last fortnight of her life. On post-mortem examination, the kidneys were found enlarged; one weighed six, the other five ounces; they were in a state of waxy degeneration. The woman had had syphilis. The capsule came off with difficulty; the texture of the organ was exceedingly firm, and of a pale colour. The separation between cortical and medullary substance was ill-defined. In the pyramids were several opaque points which yielded a puriform fluid on pressure. When examined with the microscope, similar opaque points in the cortical substance were found to contain renal epithelium in all stages of degeneration, and numerous oleo-albuminoid granules.

Supposing this woman had chanced to become pregnant a month before she took ill, she would have been delivered just about the time she died, and her death would in that case have been most probably attributed to puerperal convulsions from albuminuria, whereas, as we here see, the pregnancy might in reality have had nothing at all to do with it. I do not wish it for a moment to be supposed that I ignore the influence of pregnancy, and more especially of the effects of delivery, in inducing convulsions in those otherwise predisposed to them.

All I desire is, to utter a strong protest against the oft-repeated statement that puerperal convulsions are always the result of the albuminuria of pregnancy, while in reality they are much more frequently the concomitants of true kidney disease, assisted by the effects of the puerperal condition, the chief cause of the convulsions being the retention in the circulation of the excrementitious urinary products.

It is of vast importance to both mother and child to be able to diagnose these cases; for while in the albuminuria of pregnancy, unless the symptoms be severe, the case may almost be left to nature, in pregnancy, associated with true kidney disease, energetic treatment is demanded, even, as we have seen, to the induction of premature labour; for even when the pregnancy does not end in convulsions it never fails to act prejudicially on the renal affection.

I would even go further, and say that pregnancy, in many cases, is one of the exciting causes, not alone of albuminuria, but of true kidney disease; just in the same way as cardiac and hepatic affections are—by keeping up renal congestion.—*Med. Times and Gazette, Dec.* 16, 1865, *p.* 651.

44.—ON THE TREATMENT OF BRIGHT'S DISEASE.

By Dr. George Harley, F.R.S., Professor in University College, and of University College Hospital.

[It is in the power of the enlightened practitioner not only to check the milder forms of renal disease, but even to control and mitigate the symptoms arising from the severer renal affections. To be successful in treatment, however, we must be correct in diagnosis. The first principle upon which we must act is to give rest to the diseased organs, reducing their labour to a minimum by promoting the vicarious action of other organs.]

There are three channels by which we can draw off from the system the excrementitious materials which normally fall to the lot of the kidneys to eliminate—namely, the bowels, skin, and lungs. By keeping up an excessive action in these, we can not only remove for a time a portion of the burden from the over-taxed kidneys, but in those cases where dropsy has already become a distressing symptom give great relief to the patient.

The value, indeed, of pressing into our service the vicarious action of the bowels, skin, and lungs in the treatment of renal disease it is scarcely possible to over-rate, for we can not only by such means give rest to the kidneys and diminish dropsy, but even mitigate the more distressing symptoms which are ordinarily included under the term uræmic intoxication, but which, more properly speaking, are the direct result of the combined effects of the retention in the circulation of all the excrementitious products, organic as well as inorganic, which normally fall to the lot of the kidneys to excrete. The skin and lungs are more powerful auxiliaries in the elimination of urinary products than is generally supposed, for, as shown in our first lecture, the cutaneous perspiration does not only carry off water, but many of the organic as well as inorganic urinary salts. Thus it has been found that the sweat even in health contains urea, uric acid, phosphates, and chlorides, while in disease, in addition to these, it contains many abnormal compounds, such even as the insoluble oxalate of lime. Pulmonary exhalation, too, as was then shown, may be almost of equal service, for in the expired air of even healthy man have been detected urea, uric acid, urate of soda, and urate of ammonia.

The vicarious action of the bowels is to be induced by the internal administration of mild or drastic purgatives, according to the constitution and condition of the patient. When there is much dropsy, elaterium is a favourite form of purgative, but in cases of kidney disease it is usually advisable to administer it along with hyoscyamus, as it not unfrequently brings on an exhausting diarrhœa, especially if given after the symptoms of uræmic poisoning have already set in.

The vicarious action of the skin may be induced either by the internal administration of diaphoretics or the external use of the warm bath, vapour-bath, or hot-air bath. The two latter not only increase the cutaneous, but also augment the pulmonary elimination of urinary products. This is especially the case with the hot air-bath.

I may here remark that the usual practice of trying to diminish rather than to increase the urinous odour of the sweat and breath in cases of advanced kidney disease is greatly to be reprehended; for instead of trying to check, we ought, on the contrary, to assist Nature in her laudable efforts to rid the circulation of the deleterious agents that are gradually extinguishing the life of the patient.

In the next place, the employment of antiphlogistics is in many of the inflammatory forms of kidney disease of essential service. The most powerful of these is, of course, the local abstraction of blood, either by leeches or the cupping-glasses. But just as in many cases of inflamed lung the general condition of the patient prevents the employment of such active means, so also in the case of the kidney we must occasionally abandon this line of treatment, and content ourselves by merely diverting for a time the course of the circulation, either by the application of dry cupping-glasses, counter-irritation, or of hot fomentations to the loins.

I ought not to omit to mention that the unloading of the portal circulation by a smart calomel purge will often prove an important adjunct to other antiphlogistic measures.

As regards the employment of diuretics in the treatment of kidney disease, a few words are here necessary. In the first place, it ought never to be forgotten that in acute Bright's disease, as well as in the first stage of all inflammatory and congestive attacks occurring in the course of chronic kidney affections, diuretics are inadmissible. In the second place, it must be borne in mind that great care should always be observed in their selection; for a diuretic which will prove beneficial in one form and at one particular stage of renal disease will often not only do no good, but actual harm, when administered in another form or at another stage of the same attack. Thus, whenever the albuminuria is the result of active congestion, the antiphlogistic variety of diuretic—such, for example, as a combination of bitartrate of potash and digitalis—is to be selected; whereas in the absence of active congestion, and more especially when the vital powers of the patient are low, the stimulating variety of diuretic may not only be used with impunity, but with actual advantage. The reason why the employment of diuretics often does harm in acute kidney affections, is readily understood when we recollect that they have always the tendency rather to increase than diminish the flow of blood to the already engorged organ.—*Medical Times and Gazette, Dec.* 30, 1865, *p.* 702.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

45.—ON THE USE OF CHLORIDE OF ZINC IN SURGICAL OPERATIONS AND INJURIES.

By CAMPBELL DE MORGAN, Esq., F.R.S., Surgeon to the Middlesex Hospital.

[The use of chloride of zinc was at first adopted only after removal of cancerous tumours by the knife, but the results obtained led to its application in all wounds, whether made in operations or accidentally.]

The cancer wards in the Middlesex Hospital are constantly forcing on the minds of the surgeons attached to it the questions, can nothing be done to remove this fearful disease? Is, in the majority of cases, its recurrence after operation an inevitable necessity? May it not be that the apparent return of the disease is but the continued development of germs which were not included in the extirpation of the tumour? My colleague, Mr. Moore, has recently given expression to the conclusions at which he has arrived. They are so important as to demand the most careful consideration on the part of the profession, and, if correct, will reverse the decision on the question of removal, at which a large number seem to have arrived. I think that the observations and the reasonings upon them are such as to a great extent to justify his conclusions and to encourage the hope that we may yet, in cases admitting the operation, get a mastery over this hitherto unconquerable disease. But take what view they please, it is quite clear that surgeons should endeavour in their operations to remove *thoroughly* every trace of the disease. Probably the fact first shown by Schroeder van der Kolk has not been sufficiently considered, viz., that the germs of disease lie scattered far beyond the apparent limits of the tumour. But in addition to this mode of dissemination of cancer, it frequently occurs that in operations parts of the tumor are cut through, and are left to be removed when the principal mass has been taken away. In such a case it is probable that of the innumerable living cells which are set free and deposited in the wound some will find a nidus in which they will go on developing—they will be scattered like seed, in fact, and grow wherever they find a favourable soil. That this is very often the case has seemed to me shown by the rapid appearance of points of disseminated cancer in and around the cicatrix after operation. Were the return of the disease dependent solely on a constitutional tendency or peculiar condition of the blood, it is probable that the disease when it returned in loco would do so much as it appeared at first, in a single tumour, or that it would return in a similar part to that in which it first appeared, as in the opposite breast. But both are rare occurrences. I know not how far this view may be capable of proof, but every surgeon has seen cases which would be easily explained on such a supposition, and with difficulty on any other. It appears to me that it may be to this mode of dissemination and implantation of cancer-germs that we may attribute the wider and more rapid return of the disease in situations where there is much fat, and in the young, than in elderly and spare persons. In the young, of course, the general activity of natural growth is shared by any morbid structure, and we should expect to find a wider area of disease radiating from the principal mass, in the manner described by Van der Kolk. But something may also be due after operation to the laxity of tissue, and the freedom with which imbibition and

infiltration may occur at this age. In the case of malignant tumours forming in the neighbourhood of fat, it is probable that the wide-spread and rapid recurrence of the disease may be owing to the laxity of tissue alone, and its consequent capability of ready imbibition.

A case occurred at the Middlesex Hospital which, at the time, impressed me strongly with the notion that cancer could be thus propagated by implantation. A stout, otherwise very healthy woman, aged fifty-six, was admitted in December, 1863. She had been operated on by my colleague, Mr. Lawson, in the previous September, for a large encephaloid tumour on the upper and anterior part of the thigh, which she had noticed for the first time only five weeks previously. The mass was connected to the fascia lata, above which it was embedded in fat; part of it had to be dissected away from the fascia after the principal mass had been removed. The wound healed rapidly, and she went out apparently well on the 13th October. On the 23rd November she noticed a return of the disease. On her admission the second time in December there were large nodules lying below the skin in the course of the cicatrix, and several smaller ones scattered about above and below. She had had no pain; had gained flesh since she went out; had a quiet pulse, a good appetite, and showed no appearance whatever of constitutional disturbance. The growth was removed the day after her admission, with a large quantity of the great mass of soft fat in which it was embedded. The outgrowth had again to be traced piecemeal, but it had now penetrated through the fascia and between the muscles. On examining the removed part I was struck by observing that extending widely through the fat were little deposits of the morbid structure in great number, quite distinct from one another, and varying in size from the smallest pin's head to a large grain of wheat; some of them were at least 3½ inches distant from the central masses, and they may possibly have extended still further. But the point of interest was, that while the masses which occupied the situation of the original tumour were large and spreading, these little scattered deposits were small and of comparatively uniform size. The impression conveyed was, that while the larger portions had grown from palpable masses which had not been reached by the knife in the first operation, the smaller ones had originated in minute germs, which had been scattered broadcast, as it were, in the soft structures around, and had permeated them to various distances through the delicate lax connective tissue. Of, course, the disease rapidly returned, but still the parts took on a healthy action after the operation, and there was no indication of cancerous cachexia for some months.

I remember, too, a case which occurred very many years ago in the practice of Mr. Mayo, at the Middlesex Hospital. He removed a small tumour from the sole of the foot. At that time there was no appearance of disease elsewhere. In the course of three weeks little nodules of cancer appeared over the whole leg and thigh up to the groin; there were at least fifty of them. The glands in the groin enlarged at the same time; the man soon became cachectic, and died. Now, which of the two explanations is admissible? That which maintains that we had merely evidence of a cancerous diathesis, intensified by the irritation of an operation? Or that which presumes that in the operation germs were set free, were carried by the lymphatics or veins, or in the areolar tissue, and becoming arrested here and there, took root, as it were, and were developed into new tumours? The patient speedily died from the effects of the disease.

It is a fact, too, which is worthy of note, that while primary cancer is uniformly single, secondary cancer is most frequently multiple. It is not a little strange that Rokitansky cites this as a proof of the existence of a cancer "dyscrasia." He admits that "carcinomata originate and subsist not rarely as local evils;" but he adds: "Far more commonly, however, they are associated with a dyscrasis, which, in point of fact, often precedes and engenders the cancer. *Hence* the multiple appearance of carcinoma, as a sequel to a single one—as the sequel to the extirpation of a voluminous and hitherto solitary one. Hence in other cases, the original appearance of cancer in several organs simultaneously, or in rapid succession."

This *original* appearance of cancer in several organs simultaneously can never be proved. But surely the multiple appearance of the disease after the development or the extirpation of a single tumour points to the setting free and dissemination of germs rather than to a "dyscrasia." And a little further on he states himself that "the highest grades of cancer crasis originate through inflection—that is through the reception into the lymphatics, or more especially into the bloodvessels of cancer cells, or of cancer blastema, of a lax, soft, semifluid character." And these cancer-cells and blastema are derived from an original tumour, which has ulcerated, or has penetrated into the canals of the bloodvessels.

If the highest grades of cancer crasis arise from infection from an original tumour, and are indicated by the production of multiple growths, why assume a dyscrasis which precedes and engenders the original tumour?

Nothing, in fact, can more resemble the secondary occurrence of cancer than the deposits and consequent secondary formations in purulent infection. The mode of distribution of the formations in the lungs and liver are at times so like that they might, at first view, be mistaken for one another, dispersed here and there in equal size nodules; in one or two places, perhaps, larger deposits formed. And when we find the same tendency to multiple reproduction in the neighbourhood of an original growth, whether after operation or not, why, in the face of the fact which Van der Kolf pointed out, and which is now established, refer it to any pre-existing constitutional cause, which even a *constitutionalist* would hardly admit in the case of these lung and liver deposits, and which he would certainly deny in similar local deposits from purulent infection? My own conviction is, that in most cases of recurrence, where the tumour has even been to all appearance entirely removed, the recurrence has been due to the presence of minute cancer elements which have escaped the knife, or, as is perhaps as frequently the case, to germs being set free by section of the tumour or of diseased tissue around it during the operation, and their implantation into the newly-cut structures. That this is the case is, I think, shown by the fact that where a voluminous breast with only a small tumour embedded in it is removed, and the incision is consequently very long in proportion to the size of the tumour, the recurrence frequently takes place along the whole line of cicatrix, not especially in that part which corresponds to the site of the tumour. If, then, the view which has been so ably advocated by Mr. Moore be correct, that cancer is a local disease which becomes disseminated from the point of its first invasion, the practice of early extirpation cannot be too much insisted on. A tumour, as soon as it is recognized as cancerous, should be removed, if practicable, at once. A few days, even, may make a difference; not, perhaps, to any extent in the size of the tumour, but in the extension of its germs beyond its apparent limits. It would be better even to extirpate doubtful tumours, some of which might turn out on after examination not to be cancerous, than to leave them to develope themselves, and then to find, too late, perhaps, for help, the characters of malignancy becoming marked. These views which had often been discussed amongst the members of the surgical staff at the Middlesex, encouraged Mr. Moore, in a severe case of breast cancer, to apply the solid chloride of zinc to a large portion of the surface of the wound made in its extirpation, as he had effectually done in a case of extensive epithelioma of the face, which he showed at one of the meetings of the British Medical Association in London. This was done in April, 1864.

The patient was a spare, healthy woman, forty-three years of age. The right breast was the seat of a small cancer, with adherent skin, cherry coloured over the centre of the tumour. In the axilla was a cluster of enlarged glands, which formed a tumour a little larger than that in the breast. The mammary disease was only known to have existed for four months. Mr. Moore first detached all the axillary glands, and tying them above, cut them off. He immediately touched the stump with the solid chloride of zinc. He then dissected out the diseased mammary gland, and before closing the wound he lightly touched nearly all the exposed axillary tissue with the chloride. The case ultimately did well, but what was remarkable was, while the skin

over the breast sloughed to a slight extent, and was inflamed for some distance, due in great measure, according to Mr. Moore's notes, to its being somewhat tightly held together by the sutures, there "was no tension or great discharge from the axilla," and after a fortnight there is a report that the axilla is well filled up. It was clear that, at any rate, the touch with the caustic did not retard the cure.

To me it occurred that one might obtain the benefits which were sought by using the caustic in a less active form, and that a strong lotion of the chloride of zinc applied freely over the whole exposed surface, after an operation for the removal of cancer, would penetrate to some little extent beyond the limits of the section, and would at least destroy any floating particles of the disease which might adhere to it without endangering the vitality of the whole thickness of the flap.

The first case in which I tried this plan was that of a lady, forty-one years of age, who had a cancerous tumour in the right breast. She was well nourished and healthy, and had noticed the tumour about a twelvemonth. In the axilla was a gland slightly enlarged, but not hard, and in all respects the case was a favourable one for operation. The operation was performed in March, 1865. The strength of the solution employed was twenty grains of the chloride of zinc to the ounce of water; the whole surface of the wound was well sponged with it. The blood which still oozed was, as usual, rendered of a bright pink colour, and the contact of the solution at once caused a more free oozing from the exposed surfaces; otherwise no effect was perceptible. The lotion was thoroughly pressed in with the sponge, and in a little time the surface became soft and creamy in feel, and this softness extended to a little depth—a line, perhaps. Here I stopped, not knowing how far I might venture without causing sloughing of the flaps of skin. My impression was that a superficial slough would form, and would be thrown off by degrees during the suppurating stage, which, I assumed, must of necessity ensue, and which, in fact, I rather desired. The edges of the wound were, nevertheless, put together with sutures, save at the outer part, which was left quite open, to allow of the free passage of the supposed inevitable pus. A compress was put over the wound to check the tendency to any further bleeding into the cavity. After recovering from the effects of the chloroform, she complained for two or three hours of smarting pain; not more, I think, than is usually felt, and from that time she was entirely free from any pain at all. I thought it probable that on removing the compress, about eighteen hours after, the parts would be found swollen and angry, although the pain had been so slight; but instead of that, the circumstance most noticeable, was the absence of even the usual amount of fulness. It was evident that action, instead of being increased, had been diminished, one might almost say arrested, by the application. The skin, even up to the cut edge, looked and felt exactly like the skin of the other breast. Blood, in much about the same quantity as is usually found after such an operation had oozed from the wound; but it was pink and creamy in character, and what was especially remarkable was the entire absence of the peculiar odour which is generally found in blood which has been pent up beneath a compress for some hours. There was, in fact, no animal smell at all. But what most struck me in the progress of the case was the absence of suppuration. The whole line of incision united in the course of forty-eight hours, except just at the outer angle, which discharged a *very* small quantity of the same pink, creamy-looking fluid for a day or two more, and then healed. The same absence of animal odour was noticed to the end. I certainly never saw a wound which did not heal absolutely by the first intention go through its process of cure so speedily or so quietly. The patient remains perfectly well to the present time.

In cases of cancer on which I have subsequently operated I have used stronger lotions. In this first case the strength was 20 grains to the ounce. I next tried 30 grains, and then 40 to the ounce. With the stronger lotion a more rapid effect is produced, and the blood exudes more abundantly, but this is only for a few seconds, otherwise much the same course of events has been seen as in the first case during the early period. Some have healed in the

same rapid manner, in some there has occurred an after suppuration, but in none have any bad effects been seen.

What the effect of this treatment may be in limiting the tendency to return after extirpation of cancer, can only be determined by time and numbers. If the views of the diffusion and transplantation of cancer germs before expressed have any truth, it cannot but be beneficial; for to some extent it certainly must alter the character of the exuded matters and of the remaining tissues. My own impression is, that in cancer it would be well to go beyond the point hitherto reached, and that this may be safely done, for the effect of the chloride of zinc seems to be limited to the point with which it is brought into contact. It appears to produce very little irritation beyond that point, so that it may be worked into the inner surface of the flap till the tissue is softened to within a few lines of the surface without risk to the vitality of the remainder. In the course of the last summer I removed a large cystic tumour from the breast of a lady. The skin was stretched over it, so that when the tumour was away, there remained two extensive flaps of thinned skin with scarce a trace of adipose tissue. I was almost afraid to use the lotion to these flaps, but I did so until the inner surface was softened. The whole was healed in about twelve days, without anything that could be called suppuration, and with the same quiet inactive state of the skin as I have before described. So far as I have observed, I am well content with the *special* effect of the treatment, though the cases on which I have tried it are too few and too recent to justify any opinion being formed. In three cases of breast cancer which have been operated on for more than six months there is as yet no appearance of return. In one—a case in which the disease made such rapid progress that it had increased by at least a quarter of its size in a fortnight—where it extended beneath the pectoral muscle and ran close upon the axillary vessels, it reappeared in the axilla in six weeks. In this case, however, the disease could not be fairly removed, and I was obliged to content myself with detaching as towards the depth of the axilla, throwing a ligature tightly round the neck and cutting it off. The operation was done in June last, and the improvement in general health and alleviation of pain which followed the operation fully justified its performance.

Another case was one of recurrent cancer in the breast a year and a half after removal. At the sternal part the disease was fixed to that bone. Towards the axilla there was a subcutaneous mass of the size of a small walnut. There were small subcutaneous tumours in the line of the cicatrix, but no deposits in the skin itself. The patient's general health was perfectly good; the axilla was free. I removed the whole of the disease, using the strong chloride of zinc lotion to all the soft parts, and destroying the sternal part, after cutting the disease away, with the actual cautery, and laying over this chloride of zinc paste; I treated the part, in fact, precisely in the same way as has been so successfully adopted at the Middlesex Hospital, since Mr. Moore tried it in the case of cancer of the face before referred to. All the parts healed rapidly and with scarcely any suppuration, except, of course, at the sternal part. She was at the end of six weeks seized with symptoms of pyæmia and died. The origin of this was clearly in the cancellous structure of the sternum; but on the most careful examination of all the tissues about the seat of operation, no trace of cancer could be found. The final result of this case may seem to invalidate the statement which has been just made, that no bad result has attended the use of the chloride; but it does not affect the question. The pyæmia took place long after all direct effect of the chloride had passed off. The bone was intentionally exposed to some extent below its surface to ensure the removal of all cancer germs which might have developed in it, and was liable ultimately to any accident which such an exposure might subject it to. My mistake here was in not using from time to time a weak solution of the chloride, so as to keep in an undecomposed and healthy state the matters which lodged in the cancelli.

From data so limited it would be absurd to form conclusions. I am myself so satisfied with the results at present obtained, that I trust the plan will be far more extensively tried.

The singularly favourable way in which the wound healed in the first case on which I tried this plan, satisfied me that it might be beneficially adopted in other than cancerous cases. There was one point which especially struck me, as giving great value in hospital practice; the perfect purity of the discharges from the wound during the first few days after an operation. It is well known that the presence of decomposing animal matter tends to bring any dead animal matter with which it may be in contact into a rapid state of decomposition; and if this take place in a wound, it will certainly interfere for a time with the natural and healthy processes of cure, and may induce erysipelas or pyæmia.

That this decomposition does usually occur is evidenced by the peculiar sickly animal smell which is perceived whenever a wound which has been covered for a few hours is opened. When, on the contrary, a wound has been fairly impregnated with the chloride lotion, there is invariably an absence of any animal smell whatever for two or three days; and, unless some diseased tissue remain in the wound, there may be none throughout the healing. Were this the only advantage, it would be a great one; I believe that in our hospital it has saved many a patient from erysipelas: certainly we have been for the last eight months very free from it after operations, while just before it was very prevalent. But this may be an accidental coincidence merely, and time and experience can alone determine how much is due to the treatment. It is not, however, the only advantage. One of the most striking consequences of the application is the quiescence of the wound. The action which one would imagine must of necessity follow the application of an escharotic so powerful as the chloride of zinc, is never to be seen. The parts, up to the very edges of the wound, retain their natural colour during the early periods after an operation. I can state this confidently after the use of the lotion in varieties of operations, the removal of tumour, amputations, even with extensive and thin flaps, as Syme's and Mackenzie's amputation operations about the rectum, involving the mucous membrane, and in the perineum, and in many others, as well as after accidental wounds. In many cases the wounds have healed in twenty-four hours, without the least fulness or swelling, and leaving a line of cicatrix which after a short time could hardly be seen or felt.

Some cases are at this moment under my observation. A week ago, my colleague, Mr. Shaw, performed two operations. One of these was on a strumous subject who had sinuses in the neighbourhood of the heel, running towards one spot. No bone could be felt, but it was thought that there must be some caries keeping up these sinuses. The heel was laid open to its full extent from before backwards through the track of one of these sinuses, but no disease of the bone could be found. The wound was well bathed with a solution of twenty grains to the ounce, and healed by the first intention without pain, swelling, or discoloration.

Another case was one of necrosis of the lower end of the tibia, with sinuses on the outer and inner side of the ankle. On laying open the outer sinus the joint was found to be opened, and the astragalus in part rough and denuded of cartilage, which still covered some portion of the articular surface. In the situation of the lower end of the tibia was a large cavity, communicating with the medullary cavity, in which lay several pieces of necrosed bone. The articular surface was gone. A portion of the shell of this cavity was removed, and the necrosed bone taken away. Here, by Mr. Shaw's permission, I used the forty grain solution, sponging out thoroughly and repeatedly with it the cavity of the bone and all the tissues surrounding the ankle-joint: up to the present time the parts have been perfectly free from swelling, pain, or redness. There has been scarcely any discharge, and what there is is entirely free from smell. The tissues about the inner ankle, which were very much infiltrated, and more than an inch thick, were also laid open, and gaped widely. They were put together with sutures; they did not unite, but were found next day to be covered with a thin layer of slough, which speedily peeled off and left a healthy granulating surface below. There has been throughout an entire absence

of all inflammatory action. In such a case as this, there would, I believe, have certainly been a foul discharge for a little time at least after the operation, had the lotion not been used. The tendency to form a superficial layer of slough is seen in every case in which the skin is thus infiltrated from long-continued disease. It is due, probably, to this low state of vitality of the part. The slough separates very quickly, a healthy granulation covers the surface, and there is no inflammation of the adjacent texture. Another and very striking case is now under the care of Mr. Nunn. He removed the head of the femur below the trochanters from a woman with caries of the hip. She is suffering also from considerable disease of the liver, and her general health is much impaired. The acetabulum was perforated, and the finger could be passed into a cavity beyond; but there was no carious bone to be felt. The lotion was very freely used. The operation has only been done three weeks, but the parts are quite covered with healthy granulation, and the discharge has almost entirely ceased. But an unhealthy inflammation has taken place in the opposite groin, over a large resonant swelling, which appears to be connected with some perforation of the bowel. In this case the lotion was injected for some days into the cavity caused by the operation.

This singular absence of action in the wound is, as one might suppose, accompanied by absence of pain. That great pain sometimes attends the immediate application to a sensitive wound need not be mentioned. But this is not lasting. In most cases it subsides in from one to two hours; in some cases it does not occur at all. Where the application is made after an operation done under chloroform, the patient generally remains altogether free from pain. This is particularly the case when morphia has been subcutaneously injected, as Mr. Moore first suggested, immediately the operation is concluded, and before the effects of the chloroform have passed off. After this proceeding the patient often remains calmly asleep for some hours, and wakes entirely free from pain. There seems to be also less tendency to sickness. When, however, the immediate pain of the application has once subsided, the comfort which the patient enjoys is very striking. I have seen cases in which the patient could not tell from their sensations after a couple of hours that any operation had been done. One can explain, perhaps, from observing the action of the chloride, why action and consequent pain should be lessened or altogether prevented. It is quite clear that the chloride of zinc does not act as an irritant beyond the point of contact If its use is carried so far as to produce an eschar, the eschar will act as an irritating body, and there will be inflammation and swelling around it; but if applied short of this, and it requires a continued application of the solid chloride to make an eschar in the natural structures, it produces a peculiar pulpy state of tissue, widely different no doubt from the natural tissue; but certainly not eschar, not a charred mass which *must* be removed by the ordinary process of separation below and around it. Were the surface of a large wound converted into an eschar, it could not heal in twenty-four hours. And yet the tissue appears disintegrated; it is rapidly discharged as a creamy exudation, leaving the parts below perfectly natural in appearance. This can easily be seen when the lotion is applied to an open wound. In this creamy surface must be involved all the sensitive and vasculo-motor nerves, and their function must be arrested. With many escharotics, as the actual cautery, the acids, &c., the irritating effect is propagated along the nerves beyond the point of contact. Although the nerves exposed on the surface may be destroyed, the effect is carried beyond this point, increased nerve action and inflammation is the result. It is not so with the chloride of zinc. From whatever cause it may be, the action terminates at the point of contact. The sensitive surface is destroyed, but no irritation is set up beyond the part directly acted upon. Hence there is no pain, no vascular action, no inflammation. Whether this is or is not the true explanation, the fact certainly is as I have stated it.

Of course I do not mean to assert that all this immunity will be found in every case; but I can safely say that I have never seen it otherwise, and

that, as a rule, I have never seen the general run of cases go through so favourable a course as since I used this application.

The mode of application has been described in connexion with the first case in which I tried the solution. The first effect is always to stimulate the small vessels and cause a general oozing of blood from surfaces which had been previously dry. The blood becomes pink and creamy by contact with the chloride. On further application of the solution the whole surface is softened and assumes the same pink colour. The blood will continue to ooze out as long as the solution is applied, and for a short time after. Every part should be well saturated with the chloride—the edges of the skin, the adipose tissue, the spaces between the muscles, the medullary cavity, or cancellated structure of bone. At first I was afraid to touch bone with it, but I find now that no harm comes of doing so. The surgeon need not hesitate to apply it even to thin and delicate structures. Those cases have done best in which it has been most thoroughly used. For wounded surfaces I generally use a solution of from thirty to forty grains to the ounce of water.

An objection has been raised to the use of the chloride that union by the first intention must be prevented, and that we are therefore throwing away a chance of obtaining the best and safest mode of cure after operation. Entire union by the first intention is, however, rarely, if ever, seen after operations of any magnitude. Parts of the wound may so heal; and were the objection valid at all, it would be simply on a question of more or less. Supposing, moreover, that in one case in twenty entire union by the first intention did take place (I am still speaking, of course, of large operations), surely it would be well to forego the chances of that one, if we could ensure the safer and better healing of the remaining nineteen.

The objection, however, is really not valid. A wound will heal entirely in twenty-four hours when the solution has been freely used. It may do so in less time, possibly, but I have certainly found it healed at the end of that time. And, as a rule, I have found wounds heal more rapidly since I have used the application. After what I have seen of its effects, I should have no hesitation in using it in a plastic operation, in which, of all cases, one most desires kind and early healing.

Surgeons have, as is well known, tried various applications in the hope of preventing or lessening suppuration, or of warding off the evils which produce the large proportion of deaths after operations, such as erysipelas, pyæmia, and osteo-myelitis. Of these applications, the principal are strong alcohol, either pure or holding in solution astringent or aromatic substances, or iodine. Alcohol and some of the aromatic tinctures I have tried; but though they act very beneficially in unhealthy wounds, I have not found that they protect recent ones for any length of time from unhealthy action; nor do they seem to lessen inflammation or promote union. Aloes enjoyed a very high reputation for many centuries for its healing qualities. When applied externally, it was said to hasten cicatrization and repress hemorrhage. I have often used it in the form of the compound tincture, both to recent wounds and to old ulcers. In the latter, and especially in bad sores, it often does great good; but I never found any special benefit from its use in recent wounds. So long as it remains in contact with the wound, it prevents decomposition—as the spirit itself would—and it perhaps has a locally tonic effect on the tissues, but it requires frequent renewals, which could not be managed in a wound which it is desired to heal rapidly. The application of tincture of iodine in recent wounds was recently proposed by M. Pétrequin. Finding that suppuration was never present when this fluid had been injected into cavities or into tissues, he recommended and practised its application to wounds made in the removal of tumours, especially when situated in the face or neck, where it is very desirable to prevent the formation of scars. Whether it has been tried by others I do not know; but for many years I have been in the habit of adopting a treatment which was suggested to me by Mr. Hoffman, late of Margate, the introduction of small tents covered with iodide of starch into strumous glands in the neck, even

before suppuration has occurred in them, the effect has been very satisfactory; but considerable pain and inflammatory action have followed the treatment. Mr. Higginbottom has long recommended the application of a solution of nitrate of silver in recent wounds as well as in erysipelas, as a means of checking any tendency to suppuration. I have not tried it, nor do I know whether it has been generally useful. I doubt, from what I have seen of its action in slight wounds, whether nitrate of silver could be applied as freely as the chloride of zinc; or whether, if so used, it would preserve the parts in a healthy state for so long a time.

We find, too, that Mr. Butcher advises the use of the actual cautery in some operations. He writes: "I have found its application in this way"—quickly brushed over the surface—"most serviceable after excision of the upper jaw, and other severe measures, in arresting unhealthy forms of inflamation, diffuse or erysipelatous, and arousing at once a reparative inflammation."

I have met with nothing which acts in so unmistakably beneficial a manner as the chloride of zinc.

But there is nothing new under the sun. My colleague, Mr. Shaw, was speaking recently to Dr. Brown, of Sloane-street, on the subject of the use at the Middlesex of the chloride lotion, who told him that the subject was not new to him, as he had in early life seen the use of Burnett's fluid in recent wounds. Dr. Brown has kindly favoured me with the following account:

"When I was a young disciple of that most worthy chief of the medical department of Portsmouth Dockyard, James Henderson, a tank was erected in the ropery for saturating yarns, &c., in Sir William Burnett's fluid. Now, it was the duty of every man in the whole establishment to come and report at the surgery the slightest hurt he might receive in the course of his work; but soon after this tank was set up we found on several occasions that men in the ropery, meeting with slight wounds and abrasions did not so report themselves, and upon being remonstrated with, they said they had gone directly to the tank and applied some 'solution' to the part, and it always got well very quickly. Practically, the men employed at the tank found if they had cuts about their fingers that they healed surely and speedily through dabbling in the solution, and the fact soon spread amongst their fellows. This presently so attracted the attention of my excellent and observant friend, that we had Sir W. Burnett's solution brought into the surgery to be used in surgical treatment, and I am almost sure that at some time between 1842 and 1844 it was specially referred to in a Report to the Director-General of the Navy."

The Director-General of the Navy was so good as to have the reports of these years inspected; but he informs me that no mention of the external use of Burnett's fluid is to be found in them.

[The value of chloride of zinc is further confirmed by Mr. Paget, in the following cases at St. Bartholomew's Hospital.]

G. S., aged 55, was admitted March 31st, 1866, with an epithelial growth over and adherent to the right malar bone, and involving the structures of the cheek. The disease commenced seven years ago as a wart, which was cut away, and twelve months ago, when it had attained a considerable size, it was again cut away, and nitric acid applied. On admission the growth was as large as a five-shilling piece, irregular in form, ulcerating, very hard and immovable. The skin around was involved in the growth, and tuberculated. But little pain or inconvenience was experienced by the patient.

On April 7th, Mr. Paget removed the growth, together with a portion of the malar bone. There was considerable hemorrhage. A solution of chloride of zinc (thirty grains to the ounce) was mopped freely into the wound, and produced the characteristic creamy pink colour. A piece of dry lint was placed over the surface. The man suffered no pain in the wound as a result of this application.

10th. Wound was dressed. On removing the lint there was a moderate amount of suppuration, and not the slightest odour was perceptible. From this period the progress of the case was uninterruptedly favourable.

We saw the man on April 18th. The wound presented a healthy-looking granulating surface, with moderate suppuration, and a few portions of sloughing tendinous fibre. There was not the slightest trace of any smell.

C. C., an Irishwoman, aged 65, presented herself at the hospital on March 3d, with a compound dislocation of the lower end of the right ulna and fracture of the lower end of the radius. Reduction was accomplished, and the limb placed upon a splint. The patient was advised to come into the hospital, but this she refused. She went away, and did not show herself for a week.

On March 10th she was admitted. Phlegmonous erysipelas had taken place, and her arm was in a very unsatisfactory condition. Sloughing of the tendinous structures ensued, the wrist-joint became the seat of abscesses, and such general disorganization of tissues with depression of vital powers took place that, about a fortnight afterwards, Mr. Paget judged it expedient to remove the limb. To this the old woman would not consent.

Her condition daily became more and more unsatisfactory, and on April 7th she at last gave permission for the operation to be performed. The arm was then amputated just above the elbow-joint. The structures were very œdematous and indurated. A solution of chloride of zinc (thirty grains to the ounce) was applied somewhat sparingly to the wound. The change in colour, Mr. Bloxam informed us, was not so well marked in this as in the preceding case, where a freer application was made. Dry lint and a loose bandage were applied to the stump. She suffered no pain from the solution.

Next day she was doing very satisfactorily. There was fair reaction; a soft skin; pulse 90. No odour was perceptible about the wound. On the following day a slight odour was observed.

April 10th. The dressing was removed. There was a fair amount of discharge somewhat offensive in smell, but the stump otherwise was looking well.

12th. There was a good deal of discharge from the wound. The sutures, which were beginning to cut their way out, were removed. The wound was syringed with water. The entire surface was apparently undergoing sloughing.

We saw this patient on the 18th. The sloughs were then separating, and the lower portion of the wound was granulating. The stump was shrinking. There was still some odour about it. The general condition of the patient was satisfactory.

On the 28th, we learnt that the wound was nearly healed.

The occurrence of sloughing in the latter of these cases was to be expected under the very unfavourable condition which the patient presented. Mr. De Morgan remarks in his paper: "The tendency to form a superficial layer of slough is seen in every case in which the skin is infiltrated from long-continued disease. It is due probably to the low state of vitality of the part. The slough separates very quickly, a healthy granulation covers the surface, and there is no inflammation of the adjacent textures." From our observation of cases in the Middlesex Hospital, we think that it might have been better, notwithstanding the woman's low state, to have used the lotion a little more freely. The fœtor would in all probability have been longer delayed, and there would have been no greater amount of sloughing.—*British and Foreign Medico-Chirurgical Review, Jan.*, 1866, *p.* 201, *and Lancet, May* 5, 1866, *p.* 483.

46.—FORCIBLE FLEXION OF THE KNEE FOR ANCHYLOSIS IN THE STRAIGHT POSITION.

By Dr. Philip Crampton Smyly, Surgeon to the Meath Hospital and County of Dublin Infirmary, &c.

[The patient had suffered from a violent attack of rheumatic arthritis some years before. The limb had been fixed in the straight position, and had be-

come perfectly anchylosed. There was not the slightest motion in any direction, and the patella was firmly attached. There was complete absence of pain and of any trace of inflammatory action.]

Miss W. complained bitterly of the constant source of vexation her stiff knee was to her. She could not walk any distance on account of the fatigue caused by swinging the limb round to place it in front of the other; she could not sit down with comfort except on a very high seat; she could not kneel; and she could get up stairs only by "putting her best foot foremost," and dragging the stiff one after her.

Under these circumstances I advised her to have the joint forcibly flexed, and then to employ passive motion.

I first bandaged two strong iron splints, well padded with cotton wool, to the thigh—one in front and one behind—extending from about half-an-inch above the point I wished to effect the fracture to the groin in front, and as far behind. This was to avoid the danger of breaking the thigh bone instead of the knee. Miss W. was fully chloroformed and drawn down on the table, so that the end of the posterior iron splint rested exactly on the edge. Mr. Collis with both hands propped firmly on the anterior splint.

I grasped the foot with my right and placed my left hand about the middle of the tibia. By throwing the whole weight of my body on my left hand I succeeded in breaking through some of the adhesions. This process had to be repeated three times before the knee could be bent to a right angle. Each time the adhesion gave way with a loud crunching sound. The thigh splints were then removed, and the limb placed in a long jointed splint, with a screw behind, and lightly bandaged—the leg and thigh being left at an angle of about forty-five degrees.

The knee was kept constantly iced for forty-eight hours, and the foot kept warm.

Every day the position was slightly changed, so that towards the end of the week the limb was again straight. The screw was then slowly tightened, several times a day, until, in about three days, the leg and thigh were nearly at a right angle. For about six weeks this gradual process of flexion and extension was persevered in; then the splint was removed, and Miss W. was allowed to get up. She was desired, always, to sit on a low chair and to try and strain the foot backwards so as to touch the leg of the chair.

Miss W. then returned to the country and has enjoyed perfect health ever since.

[Two years after the operation the report of the case is as follows:]

"She has as good use of that knee as of the other—can take long walks, dance, run, or exercise in every way as freely as before the joint became affected."—*Dublin Quarterly Journal, Feb.* 1866, *p.* 18.

47.—SUBACUTE SYNOVITIS AND CONTRACTED JOINTS.

Mr. W. Adams has been employing for some time past at the Great Northern Hospital a mode of treatment for effusion into joints which he finds very satisfactory. In subacute synovitis, especially of the knee-joint, where there is much fluid, a large blister is applied to the skin, and when this has risen, the cuticle is cut away completely, leaving a raw surface, which is then dressed with strong mercurial ointment. The dressing is continued so long as the blistered surface remains unhealed. Occasionally healing takes place very rapidly. In such cases a dressing of equal parts of mercurial and of savin ointment is applied in order to keep the blister open. No constitutional disturbance or salivation, Mr. Adams tells us, results from this treatment; but he finds the fluid in a joint absorbed with unusual rapidity. The best cases for the employment of this method are those where, after an attack of acute synovitis, although all inflammatory symptoms have subsided, the amount of effusion shows no tendency to diminish. In very chronic cases with much thickening, he considers Scott's dressing more useful. As

regards the propriety of employing forcible extension in the contraction which so often follows synovitis, Mr. Adams considers that where this condition ensues upon traumatic or rheumatic affection of the joint, the process is allowable, and may often be employed with great advantage. His plan is to give chloroform, and apply a fair amount of force, not in jerks, but steadily. When the joint is felt to yield, even ever so little, the limb is immediately placed upon a splint, provided with power of extension by screws, which are turned daily. In contraction from strumous disease of joints, he objects even to this amount of violence, having frequently seen grave mischief lighted up by this means. When, however, a joint has been very long quiet, and bony anchylosis has taken place, the strumous taint meanwhile almost disappearing, the objection ceases to apply with the same force, and very careful extension is often found useful.—*Lancet, Jan.* 6, 1866, *p.* 8.

48.—ON AN EASY METHOD OF REDUCING ORDINARY DISLOCATIONS OF THE SHOULDER JOINT.

By Wm. Elliott Porter, Esq., Linfield, Sussex.

While a student at the London Hospital a dislocated shoulder was brought in, which great force failed to reduce. On pausing for a short time before adopting other means, one of my fellow students picked up the arm and gently manipulated it; while doing so the bone slipped into its place—how. none could say, but the dislocation was reduced. I thought of it a great deal, and came to the conclusion that the humerus, without the exhibition of great force, was a sufficiently powerful lever, when used as one to reduce an ordinary dislocation of the shoulder. Not long after I left the Hospital, and had to wait for some time before I had an opportunity of testing my plan.

While acting as House-Surgeon to the Dorset County Hospital in 1856, a dislocated shoulder was brought in; it was a stock case, that is, it came frequently; but it must have been troublesome to reduce, as they always used pulleys for it, and the Hospital porter began getting them in order; I thought, however, I would try my plan first.

The patient was a moderately muscular man, at about the middle period of life, and having undressed him I laid him on a mattress on the uninjured side; kneeling behind him, I placed the palm of one hand firmly on the head of the scapula and the fingers in the axilla under the head of the humerus; with the other hand I rasped the condyles and pressed the arm to the side, then drew it backwards and thus got a better hold with the fingers engaged at the head of the bone; by the next movement I slid the arm forwards and depressed the elbow, at the same time rotating outwards, and lifting the head of the bone it immediately slipped into its place.

Since I have been in practice I have tried the plan three times on healthy, muscular young men, all first dislocations; in two I succeeded quickly and easily, and failed in one which had been done three days; the old way of the heel in the axilla was in this case after some difficulty successful.—*Med. Times and Gazette, March* 10, 1866, *p.* 264.

49.—ON THE TREATMENT OF FRACTURE OF THE RADIUS AT THE STYLOID PROCESS BY MEANS OF GORDON'S SPLINT.

By Lawson Tait, Esq.

[Few fractures have had so many ingenious splints devised for their treatment as that known as Colles's fracture, yet most or all have been given up, and the ordinary palmar and dorsal straight splints generally used, although frequently with unsatisfactory results.]

In spite of the utmost care most cases of this fracture turn out unsatisfactorily, and many are the actions of damages that have been raised on its

In none of the cases did the patients complain of the slightest pain or inconvenience arising from the apparatus.—*Med. Times and Gazette, Feb.* 17, 1866, *p.* 170.

50.—ON SOME INJURIES TO THE HEAD AND NECK OF THE HUMERUS.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital, and Lecturer on Surgery.

We will classify fractures of the humerus near to the shoulder-joint not into those at the surgical neck and those at the anatomical neck, but into those *below the tuberosities* and those *through the tuberosities*. Respecting the latter, I have now to ask your attention to a most important practical point, and one which you will find scarcely mentioned in books. A few months after a fracture through the tuberosities, or, what comes to the same thing, a separation of the epiphysis, it is very likely that all the appearances of a dislocation will be assumed. Owing to the altered form of the neck of the bone, the head is gradually displaced downwards, until a hollow is caused under the acromion, and the aspect of things is exactly like that of a dislocation. You may even feel the head of the bone in the axilla, though not so low down, nor so distinctly as in a true dislocation. It often happens that a patient in this stage of things consults another surgeon, and is told that a dislocation has been overlooked, or, still more frequently, a surgeon discovering this state in a patient whom he has himself treated, is led to the belief that he has made an almost disgraceful mistake. Sometimes this deceptive appearance is present from the first, but more usually it is produced when the patient begins to use his arm, and if present at first it always increases. I have seen this often, and have seen reduction attempted not unfrequently. A boy, aged 14, was brought to me by a surgeon, who believed that he had overlooked a dislocation. The deformity was very considerable—a hollow under the acromion, the head of the bone felt in the axilla, the fibres of the deltoid stretched. Two months had elapsed, and the surgeon who brought him expressed himself as certain that the symptoms now present were not there at first. I detected irregularity and thickening about the line of the epiphysis, and I found also that by pressing the arm straight upwards, I almost filled up the subacromial hollow. I convinced myself and my friend that the accident had been a separation of the epiphysis, and that there was no reason to reflect upon the treatment. The most conclusive example of this condition, however, came under my notice nearly twenty years ago, when I was a student. Being at an autopsy on a lunatic who had died in asylum, I observed deformity in one shoulder, and remarked that it must be dislocated. The medical superintendent remarked that he had treated the man a year ago for fracture of the neck of the humerus, and that he felt confident that there was no dislocation. He willingly gave me permission to remove the bones, although expressing his fears that I should expose some bad surgery. We found on dissection that there was a fracture through the tuberosities, and the head of the bone had travelled down to a new articular facet just below the glenoid cavity. This specimen is, I believe, now in St. Bartholomew's Hospital Museum.

When the appearances of dislocation are present in a case of fracture from the first, they are due to displacement inwards of the upper end of the lower fragment, and the higher the line of fracture the more deceptive they will be. At this stage, however, we have the symptom of crepitus to help us, and it is decisive. The hollow under the acromion is also never so definite as it becomes at a later stage. At the latter, when the bone is consolidated, it is often exceedingly difficult to form a decided opinion.

From this clinical fact two corollaries arise—first, that we should be exceedingly careful in giving opinions respecting old cases which have not been under our own care from the first; and secondly, that when we have to treat such a fracture we should clearly explain to the patient what the result will be. In one case recently in the hospital we had all the appearances of

dislocation, with the most positive crepitus in the neck of the bone. At the end of a month, when the bone was united and the swelling had disappeared, we had yet more deceptive appearances, and explained to the man that, should he ever be examined by another surgeon, he might very likely be told that a dislocation had been overlooked, and I offered him a written statement of what the nature of his injury had been. I may repeat that in the first stage the diagnosis is not difficult; the abnormal mobility, the crepitus, and the ease with which the elbow may be brought down to the side all point to fracture.

It is possible that dislocation and fracture may occur simultaneously. Two or three times I have suspected this complication, but I have never had an opportunity for proving it, or of examining a dissected specimen. It is *à priori* very improbable that such lesions should be produced together. When the neck of the bone snaps by violence there is no longer any agency by which the head of the bone, now discontinuous with the shaft, should be displaced, and on the other hand, if once the bone has been dislocated it would be exceedingly difficult by any continued violence to break it. I suspect strongly, therefore, that most of the cases in which we hear of dislocation and fracture together, are really instances of fractures unusually high up, and in which, as I have just explained, the appearances of dislocation are subsequently simulated.

In any case of separation of the epiphysis in which the two fragments should be thoroughly liberated from each other, the epiphysis itself would in all probability be displaced by the three muscles attached to it (sub-scapularis, supra-spinatus, and infra-spinatus) in such a direction that its under surface, instead of looking downwards, would look directly outwards. There is nothing whatever to counteract these muscles. Should it occur, no surgical art could bring the epiphysis down again, and the shaft of the bone would unite to it at an angle. Thus the neck of the humerus would come to resemble in form the neck of the femur of a young subject, and subsequently the elevation of the arm by the deltoid would push the head of the bone lower and lower in the glenoid cavity, and gradually produce the appearance of dislocation to which I have adverted.

As regards the treatment of injuries to the neck of the humerus, we may note that in the majority of cases there is little or no displacement, and that the fragments are locked or impacted together. The higher up the injury and the thicker the bone at the site of fracture the more likely is this to occur. In such cases it matters little what plan of treatment is adopted. If there be displacement, it will probably be that of the lower fragment inwards, and to counteract this we must employ extension and put a large pad in the axilla. It may be sufficient in some cases to employ the plan of a large axillary pad with the arm bandaged across the chest. On the whole, however, I prefer to use a single long splint, well padded and pressed into the armpit, on which the arm and forearm may be extended with great efficiency. The only plan which would be likely to remedy the displacement which has been just described in reference to separation of the epiphysis would be by carrying the limb directly outwards at the right angles with the patient's trunk. I have never tried this, and it would probably be very inconvenient in practice. In any case in which decided displacement existed despite extension in the straight, I should be inclined, in spite of the inconvenience, to try abduction of the arm, keeping the patient in bed and using plaster of Paris.

You will see that in discussing the treatment of these fractures we have to take notice quite as much of the extent of displacement as of the direction of the fracture. In many cases, whether of fracture through the tuberosities or separation of the epiphysis, there is only incomplete displacement of the fragments one from the other, and they still remain in apposition at parts. In such cases the fragments, by their mutual locking and pressure, prevent the muscles from effecting the characteristic distortion. The thicker any given bone is at the part where broken, the more likely is it that the displacement will be incomplete.

Then we must also recollect that in many cases the displacement will not

be according to the direction of muscular action, but will be influenced rather by the position assumed by the bone at the moment that violence was applied. Very often the original displacement, although contrary to muscular action, is from some wedging of fragments, or more frequently from the intervention of tense bands of periosteum or fascia, never afterwards altered. Thus we know beforehand what the typical displacement is when the fragments are free to move under the influence of the muscles, but we cannot predicate of any given case what it will be, since it may be modified by other conditions. —*Medical Times and Gazette, March* 10, 1866, *p.* 247.

51.—ON A NEW FORM OF LEG-SUSPENDER AND BEDGUARD.

By Henry Greenway, Esq., Plymouth.

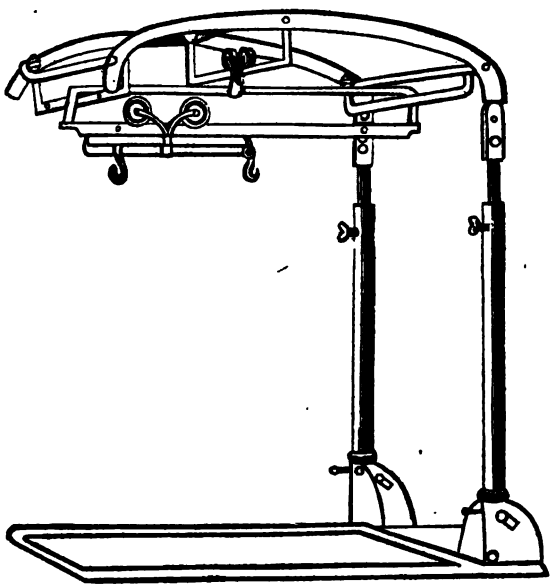
[An instrument which will alleviate the tediousness of confinement to bed, in cases of fracture of the lower extremity, must be received as a valuable addition to our armamentarium.]

In August, 1864. I left instructions at Weiss's for an instrument to be made, whereby I hope to gain every movement that could be desired. These instructions were carried out, but my success was only partial. I have since made a number of expirements in order to attain the desired end, and I am happy in now being able to bring before the profession an invention whereby a patient is enabled to move his injured limb about the bed with nearly the same freedom as he would his sound one.

The accompanying engraving will give some idea of the nature of the apparatus. In general terms it may be described as consisting of a base or foot, two telescope pillars, and an overhanging top, to which is attached travelling gear affording longitudinal, transverse, rotatory, and swinging movements. Also contrivance attached to the base, whereby the pillars are supported, and any tendency to inclination corrected.

The *base*, which is placed on the battens of a bed, underneath the mattress, is a stiff iron frame, nearly square in form, at the outer angles of which are fixed *quadrant-shaped plates*. Attached to these are two *pillar stumps*, their lower, flattened ends being hinged to the plates, and their upper or free ends rounded so as to fit into the lower part of the tubular pillars. These stumps and plates not only support the pillars and parts above, but by means of screws, have the power of correcting any inclination—an occurrence which would happen if the battens of the bed, on which the base lay, dipped in the middle. (Only a very small, intermediate portion of the stumps can be seen in the sketch.) The *telescope pillars* are partly tubular and partly solid. Each tubular portion, at its lower end, fits on to the upper part of the pillar stump; the solid portion of each pillar is attached to the upper framework by a joint which is fixed with a pin when the apparatus is in use. (This joint is placed for the purpose of portability, as by it the pillars can be folded underneath the top.) With the telescope arrangement the height of the apparatus can be regulated at pleasure, by means of a check-pin at the upper end of the tube. The *top*, which is supported by the pillars on its outer side only, is an iron framework of about the same dimensions as the base (nearly two feet square), which it overhangs. Attached to this framework is the *travelling-gear*, by which the various motions are obtained. The first portion of this gear consists of three flat bars placed longitudinally, and fastened to the top by the turned-up ends. The middle one of the three is the longitudinal truck-rail; the other two are bearing-bars. On the truck-rail are placed the trucks which allow the patient to move upwards or downwards in the bed. Passing through the holder of these trucks, underneath the rail, is an arm, the ends of which are turned downwards to support the transverse truck-rail. The ends of the horizontal portion of the arm rest on the bearing-bars, and consequently glide over them when the before-mentioned trucks are in motion. On the transverse rail run a pair of trucks similar to those above; these allow the limb to be moved sideways. Attached to the holder of these trucks,

underneath their rail, is the rotary bar. This works on a pivot, and enables the patient to direct the limb across the bed diagonally. On this bar are placed two hooks, from which suspension of the limb is made, and a swinging movement obtained. In the event of the limb not being properly balanced when the hooks are placed at the ends of the bar, the hook on the depressed end should be moved to the notch nearer the centre of the bar, as shown in the sketch. The equilibrium will be thus obtained, as in a steel-yard.



The mode of connecting the limb with the suspender may be left pretty much to the choice of the surgeon. If the fracture be treated with the ordinary side splints, or the starched bandage, then the limb had better be laid on a slightly curved back-piece, (supplied with the suspender,) and slung with straps from the hooks before-mentioned. Where the limb is treated with a back-splint, as M'Intyre's, or lies in a fracture apparatus having a back-piece, the connexion can be made by the aid of straps without the addition of another back-piece.

I was once asked by a medical man whether I could devise a method by which a patient with a fractured leg could turn over on his side? A very slight addition to this apparatus would enable such patient to accomplish that feat; but as the act of turning would necessitate muscular exertion, and thus displace the fragments, I have not thought it necessary or prudent to carry out the idea.

In the application of this apparatus, the base should be placed *underneath* the mattress in the required position for the limb, and tied on its outer side to the frame or battens of the bed. If possible, this should be done before the patient is placed on the bed. The fracture having been attended to, the remaining portions of the apparatus are placed in position by dropping the pillars on the stumps which appear at the edge of the mattress. Connexion can then be made between the limb and the suspender, and any inclination of the top from the horizontal position can be rectified in the manner before stated.

At what height the limb should be raised above the surface of the bed, is a question on which difference of opinion exists. As a rule, the patient is most

at ease when the limb is only slung sufficiently high to clear the bedclothes underneath.

The advantages embodied in this instrument are (in addition to the longitudinal and swinging movements): a transverse motion, the most frequently required; a rotatory motion, which could only be imperfectly obtained in an ordinary two-sided "cradle"; and, the form of the instrument being unilateral, the sound limb is not thrown aside from its fellow by any intervening framework, thus preventing distress in the hip-joint. This peculiarity of form also enables the surgeon or the nurse to dress any wounds, supposing the fracture to be compound, without removing the suspender from the bed.

Many modifications of this instrument might be suggested. I have one now in use, with a single telescope pillar reaching the floor, and fastened to the frame of the bed with a screwclamp; but I consider the present form most to be depended on for stability.

The chief difficulties I had to contend with in the construction were, to gain free longitudinal movement when the line of suspension was out of the centre—that is to say, when the lower trucks were at one end of the transverse rail (as seen in the sketch); also, as to the mode by which attachment to the bedstead should be made. These have both been overcome. If, however, by any accident, the upper trucks drag when the lower trucks are in the position just named, they will right themselves by the patient merely bringing his leg underneath the centre of the transverse rail before he raises or lowers himself in the bed; but if the travelling-gear be made and put together properly, no dragging will take place *whilst the leg or any weight is suspended*, as the upper trucks are thereby prevented rising off their rail. A little oil should be occasionally smeared over the parts subject to friction.

I have instructed the Messrs. Weiss on certain points of detail, apparently unimportant, but on the carrying out of which the proper working of the instrument depends.

I have also desired them to make a simple and inexpensive unilateral framework, without travelling-gear, for relieving any part of the body from the pressure of bedclothes. I have termed this a *bed-guard*. The apparatus just described can be used for this purpose by merely removing the travelling-gear.—*Lancet, Feb.* 24, 1866, *p.* 202.

52.—LEATHER-FELT SPLINTS.

These very simple and admirable splints are the invention of Mr. J. G. Hides, of Mortimer-street. They consist of stout felt, lined on one side with wash leather. All that is necessary for their application is to place a splint (with the leather side downwards) upon a table, then paint the felt side with a liquid which is supplied with them, apply the splints to the injured limb, and bandage with an ordinary roller, leaving a little interval between each turn of the bandage. If properly used, the splint will harden almost like a piece of wood in about thirty-five minutes, taking the temperature of the limb at 100° F. The advantages over other appliances seem to be their lightness, perfect adaptability to parts, and, as no padding whatever is required, they are more quickly and easily applied, and the bandage uniting with the splint as the liquid dries more support is given.

They can be used again and again by merely painting the felt side afresh with a weaker solution of the liquid. They are cheap, the price for a set of eight arm and hand splints being about the same as that of wooden splints. If additional support be required, the felt has only to be made of thicker quality.

We believe that they have been much approved of by Mr. Partridge, of King's College, and Mr. De Morgan, of Middlesex Hospitals. We have ourselves tried them for a fractured arm, and had every reason to be satisfied with them. The fracture was rapidly put up, and had the appearance of lightness and neatness so delightful to a Surgeon's eye. We fancy they

would be capital splints for field practice in the army.—*Med. Times and Gazette, Feb.* 10, 1866, *p.* 162.

53.—HOW TO MAKE GUTTA-PERCHA SPLINTS.

By Edwin Truman, Esq.

No material presents so many advantages for surgical purposes, with so few drawbacks, as gutta-percha; and a very little experience will enable all surgeons to use it perfectly. In a periodical of this year this passage occurs: "After a good deal of hard work I succeeded in thinning down the substance to the required thickness." The writer is describing the production of a gutta-percha splint for putting up a fractured jaw. It seems strange to hear so kindly a substance so spoken of—dough is not more easily moulded and flattened into sheets in the hands of the cook than gutta-percha might be—and by the same means a paste-board and rolling-pin which would have despatched this "hard work" in a few seconds, and produced a sheet of any thickness or size, perfectly even and ready for the splint. No matter in what form the gutta-percha is, nor in how many small threads or large balls, —place it in a vessel of water, and boil it till it is perfectly softened throughout; now collect it into one ball; dip it in cold water to enable it to be handled without burning, and knead it into a uniform mass with the hands. To do this, if it is very good and tough, will require some strength and also warming two or three times: but eventually it will form a uniform plastic mass not much harder than stiff putty. When in this state, place it on any flat surface which is wet, and with a roller—a common glass rolling-pin dipped in water is the best—roll it into a sheet of the desired thickness, let the roller slide upon it in all directions, and continue this action till the surface is uniform; it will now be fit for cutting into shapes. This can readily be done either when hot or cold with a pair of scissors, and no force is required to mould it into the form of splint required, the only precaution requisite being to hold it steadily in that shape till it is cold; after which it will not alter unless again made hot. It may sometimes be requisite to join two pieces of gutta-percha together. This is readily done by warming the two surfaces to be united by dry heat, with a hot iron, not burning, but merely softening the surface. Two such hot, dry, soft clean surfaces will unite most perfectly when brought into contact with each other, if held together till they are cold.

It may sometimes also be useful to know how two pieces of this material may be moulded on each other without sticking. If one be cold and the other hot, they will not unite, especially if the cold surface be anointed with soap-suds.

It is, I believe, the custom in some of our London hospitals to look upon an old gutta-percha splint or one which has aborted, so to say, in the making, as useless, and only fit to be sold as old material, thus necessitating a considerable loss. If the above described process were employed no small saving might be effected, as the same piece of gutta-percha might be used over and over again.—*Archives of Dentistry.—Edinburgh Med. Journal, May* 1865, *p.* 1036.

54.—ON A CASE OF RECURRING FIBROID DISEASE OF THE SHOULDER.

Under the care of Sir William Fergusson, at King's College Hospital.

[Sir William drew attention to the appearance of the hand and arm which had just been removed from a patient on account of recurring fibroid disease of the shoulder. Two-thirds of the scapula had been previously excised, and in the present operation, the remainder of that bone along with the greater part of the clavicle.]

The disease being confined to its points of junction with the trunk, the

limb itself, when the affected portion was covered with a cloth, showed no sign of the cause which had necessitated so severe a procedure. Round, white, and shaped with more than ordinary beauty of proportion, it reminded one of a marble model in a sculptor's studio. Pathologically, this unimpaired preservation constituted a point of scientific interest. It suggested at once a line of distinction between the disease with which the patient was affected and the malady with which such an affection was formerly classed—cancer. There was here none of that wasting which could scarcely have been absent in a disease of the latter class existent—as this has been—for five years. And this is a point which has been noted by Mr. Paget, who first proposed the name "Recurring Fibroid" as distinctive of this disease. He says (art. Tumours, "Holmes's Surgery"): "Moreover, after repeated recurrences, the patient commonly retains apparently good health, and shows none of that cachexia which would almost certainly exist in a patient who had suffered repeated recurrences of cancer." A short reference to the observed characteristics of recurring fibroid disease, as graphically described by Mr. Paget, will be useful to illustrate the reasons for Sir W. Fergusson's operation.

The great feature of this disease is obviously its liability to recurrence. But this character belongs also to cancerous disease. Recurring fibroid, however, does not affect the lymphatics, and its recurrence is in the situation which it formerly occupied. The system generally does not appear to be affected by the disease. In the few cases where, very late in the disorder, similar growths have occurred in the lungs, it may be suspected that the propagation was by direct transport of germs in the blood from vessels into which a tumour had grown. "The general appearance of recurrent tumours is, in their first growth, often not distinguishable from that of tumours of the same structure that are not likely to recur; they may look like ordinary fibro-cellular, fibrous, or other tumours. More often, however, they differ from these ordinary tumours in that they are softer, more succulent or glutirous, glistening or translucent, brittle, or easily torn with a kind of fibrous grain. Commonly, all these peculiarities become more marked in the successive recurrences of the tumour; and the latter growths are, generally, like masses of yellow or ruddy soft gelatine with bloodvessels. The latter are, also, usually much more rapid in their progress than the earlier growths, and are, generally, less well-defined, penetrating further and more vaguely amongst the interstices of adjacent parts."

There seems scarcely a limit to the number of times that these tumours may recur.

[There was a patient in St. George's Hospital a twelvemonth ago, in whom recurring fibroid disease had been removed forty times and yet recurred, yet the removal should always be effected if possible, for the patient may regain perfect health and the disease cease to recur. There have been a fair number of cases recorded in which part of the clavicle and the scapula have been removed successfully. In January 1865, Sir W. Fergusson removed the lower two-thirds of the scapula in a young woman on account of this disease. The wound healed and left the arm freely movable. Since then a tumour appeared under the pectoral muscle, and increased so rapidly that it was determined to operate at once.]

The patient having been placed under chloroform, a grooved needle was thrust into the upper part of the tumour a little below the clavicle, at a point where it seemed just possible, from an obscure sense of fluctuation, that fluid was present. There was, however, none. A small incision was then made over and along the clavicle about an inch and a half external to the sterno-clavicular joint, through which the bone was divided by the saw and cutting pliers. The object of this, as Mr. Fergusson afterwards explained, was to allow free movement of the shoulder during the ensuing steps of the operation, without causing any strain upon the sterno-clavicular joint. By this step, too, implying the preservation of the inner end of the clavicle, the sterno-mastoid muscle was reserved entire. An assistant (Mr. Wood) then thrust his thumb through this wound and compressed the subclavian artery pon the first rib. Next, the incision was continued along the clavicle,

at first outwards, then backwards over the acromion, and lastly downwards and forwards, so as to terminate in the inner and upper part of the arm below the axilla. From the point where this incision, leaving the clavicle, tended backwards, another was made passing down in front of the shoulder-joint, and meeting the first at an acute angle. By these means two semilunar flaps were formed, one before and the other behind, and the skin of the axilla was preserved. The tumour having been exposed by dissecting the flaps from its surface, the muscular structures which attached it to the trunk were divided. There still remained to be accomplished the section of the subclavian vessels and the accompanying nerves, and this was the most delicate part of the operation. Behind the clavicle the tumour was less distinct than at any other parts, spreading vaguely amongst the tissues, and rendering it doubtful at first how far it might extend amongst the muscles of the neck. A careful dissection succeeded in completely isolating it. The mass was then drawn forwards, and the subclavian artery was compressed. In order to obviate the chance of slipping, a strong forceps, such as is used for removing sequestra, had been prepared by having its teeth covered with wash-leather. The blades of this were pushed from behind forwards so as to enclose the subclavian vessels, and another instrument of the like kind was pushed from before backwards with a similar object. Thanks to these, which admirably answered their purpose, there was no difficulty in retaining and ligaturing the artery, and the operation was completed by finally dividing the remaining tissues, chiefly nerves and vessels, outside of these blades, with the loss of scarcely a table-spoonful of blood. For precaution's sake, ligatures were applied to two or three other vessels, but they were scarcely needed. The flaps were then brought together, sutures applied, and the patient removed.—*Lancet*, *Nov.* 25, 1865, *p.* 592.

ORGANS OF CIRCULATION.

55.—ON THE GALVANO-PUNCTURE OF ANEURISM.

By Dr. John Duncan, M. A.

[The patient in this case was 45 years of age, and complained chiefly of shooting pains in the shoulders and upper part of the chest. In December, 1863, a small pulsating tumour appeared through the sternum.]

The aneurism now, in October, 1864, occupied nearly the upper half of the sternum, stretching beyond it above and to the sides; in shape, a flattened hemisphere, quite without nodules, and with a moderate pulsation. From that time it progressed slowly for a year, extending more upwards than downwards, sometimes stationary, never retrograding Notwithstanding its increase, Mr. A.'s general health remained so good that he was able to follow uninterruptedly his usual vocation, concealing the unsightly deformity by comforters and a loose great coat. He was, however, greatly annoyed by flatulent dyspepsia, and grew thinner and paler and weaker. He had little of his former pain, but his nights became restless, his cough troublesome, his breathing difficult when he lay upon his back, and the necessary use of opiates, increased the stomachic disorder. In October, 1865, the acetate of lead was tried and pushed to plumbism; for about that time an apparently successful case had been recorded by Dr. Owen Rees. It had absolutely no effect. The tumour grew faster than ever, and by the 10th of November had acquired a bulk truly enormous. So high did it reach in the neck, that as he sat, stooping from weakness, his chin rested upon it; and he asked me one day if it might not push back his head by its increase. It covered half of the sternum and of each clavicle, and from side to side over its highest part it measured thirteen inches. It was no longer smoothly hemispherical. Nodulated eminences had arisen here and there. Now one and now another had rapidly increased, reddening the skin, and threatening to burst, but hitherto they had failed to do so. At length, however, there

came at the upper part a small vesicle, the size of a pea, perhaps from friction of the coverings. It was smeared with lard, and the softest lamb's-wool was placed between the clothing and the skin. Thus treated, it shrivelled and disappeared in a day or two; but, on the 15th of November, another of the same size filled with bloody serum arose lower down, where the skin was extremely thin. As this tightened and puckered with the pulsation, it seemed as if every moment it would give vent to a gush of blood. It burst in a day or two, but exposed simply the raw surface of the skin as after a blister. This surface then took on a slow ulcerative action, deepening little but gaining in superficies, while one or two new vesicles formed around it, which either dried up again or added to its extent. Matters progressed thus, while the patient's strength rapidly failed, till, on the 2d of December, a very slight oozing of blood took place, which was stopped by applying the perchloride of iron solution. It was evident, however, that, if nothing were done, the patient could live but a day or two at furthest, for bleeding from some point of the extensive ulceration must speedily recur. What was to be done had of course been anxiously and often thought over and discussed.

My father had already made an essay with galvanism for the cure of an abdominal aneurism which was threatening to point in the posterior lumbar region. Fifteen minutes after the introduction of the needles, pulsation had entirely ceased, and the operation had been performed without the slightest pain or inconvenience to the patient. Unfortunately this person afterwards resumed his occupation as a post-boy. In a fortnight he rode to Roslin, and indulged in a quite unwarrantable amount of exercise. That night he died suddenly, it was supposed by the medical man who saw him, from rupture of the aneurism internally. To the day of his death he felt perfectly well.

The case was encouraging, and it was determined as a last resource to apply galvano-puncture in the present instance. The injection of perchloride of iron was evidently too dangerous, for as each drop formed its coagulum it might easily be carried into the circulation; and the fatal result attending the only attempt to introduce wire into the sac of such an aneurism gave little encouragement for a repetition. On the other hand, galvano-puncture had been attended with a large amount of success, and the needles gave a holding point for the clot during its formation.

It was thought good, therefore, to request the assistance of Dr. Wright, whose knowledge of matters relating to natural science is well known, and who had been conjoined with my father in his former experiment. Everything had been prepared for the evening of 3rd December, when, unfortunately, at four o'clock that day, I was hurriedly summoned to the patient on account of copious hemorrhage. It had already greatly ceased under the perchloride of iron, and a little further pressure arrested it entirely, but not before the patient had been rendered very faint and the pulse very feeble. I at once sent for my father and Dr. Wright, and also for Professor Simpson, who was desirous of witnessing the operation.

By six o'clock everything was ready, and the patient was placed on his back upon the sofa. Two needles were introduced, one on each side of the sac not far from its base, and were so placed that they must have overlapped each other by an inch at the point, while lying about that distance apart. The conducting wires were attached after the needles had been introduced, and a very feeble sensation accompanied the closure of the circle. The patient thought that he experienced a slight shock also on its being opened, but nothing was felt during the whole period of galvanic action. Three-quarters of an hour after the current began to pass, the intensity of the pulsations had decidedly diminished, and the wires were transposed at the battery, so that the positive needle now became negative. In an hour and three-quarters the pulsation had still farther diminished, but without any measurable change of bulk in the tumour. By percussion, air could be mapped out on the surface in little wells, principally to the right front. As it increased in quantity pretty rapidly, and its entrance into the circulation was feared, the needles were removed at five minutes past eight o'clock, or two hours and ten minutes after their insertion. Towards the end the pulse

had become a little more feeble from the long-continued restraint in one position, which Mr. A. bore with difficulty in his exhausted condition, though fortunately he had not experienced that impediment to respiration which was usual when he lay upon his back. Some brandy was administered; he was undressed and put to bed. He was directed not to turn too much upon his face, that all danger from the gas might be avoided.

4th December. He had passed on the whole a comfortable night, sleeping well towards morning. The pulse was 80, rather feeble. The force of pulsation in the tumour, though greater than last night, was decidedly less than before the operation. At eleven o'clock, a very faint tympanitic note was yet elicited by percussion on certain parts of the tumour, but it disappeared in the afternoon.

At half-past seven in the evening, two needles were again introduced a little higher than before. They were more than twice as thick as the first pair, and somewhat longer; so that they must have overlapped nearly two inches. Their action was much more rapid than on the former occasion. In fifteen minutes there was clear percussion at parts, and the poles were changed. In five-and-twenty minutes the tympanitis was very extensive, and it was thought right to remove the needles. The air, as before, was principally on the right side, but much more was formed in this than in the first operation, and some even escaped along the needles. A very notable diminution in the force of the beats was observable.

5th. He was restless during the night. He would often start from a doze, in the belief that blood was trickling down his side, and that he was dying of hemorrhage. In consequence he was feverish a little, and exhausted in the morning. The tumour in parts gave the sensation of dough, in others, of a half empty bladder. A few drops of bloody serum escaped by the puncture of the second pair of needles.

6th. He was extremely well, having passed the night in quiet sleep. His pulse was about 80, stronger than it had been. and he enjoyed taking small quantities of arrow-root, calf's-foot jelly, and beef-tea. In the tumour very little air could be detected, and the pulsation remained moderate.

On the 7th, he suffered a good deal from flatulence, and could take almost no food. A vesicle, dark and containing some air, had arisen during the night. It was higher up than the ulcerated surface, which was still covered by the lint steeped in perchloride of iron. In the evening his morphia draught was increased from 60 to 80 minims.

8th. He had slept well and felt much refreshed. An ulcer now occupied the site of the vesicle, and, as the discharge was a little fœtid, Condy's fluid was applied. He continued lively throughout the day; but next morning was very much worse, and he rapidly grew weaker, suffering greatly from flatulence and colic, till he died on the morning of the 11th December.

Electro-puncture was revived by Petrequin in 1845, and a considerable amount of experience has since been accumulated. Many experiments have been made on the occlusion of arteries in animals, chiefly by M. Abeille, and by commissions of the Milan Scientific Society. Their results were thoroughly satisfactory.

Thus, in eleven experiments on dogs and sheep, M. Abeille invariably found on killing the animal a few days afterwards, a firm and even laminated clot closely adherent to the vessels. M. Asson reports, as the result of the labours of an Italian committee, that galvanism is a mode of producing a firm obstructing clot, which is composed of fibrinous granulations, connected together and adherent to the walls of the vessel. It has been asserted that this is due to inflammation excited by the needle or galvanic current in the walls of the artery; but I have found that, if the animal be killed immediately after making these experiments, a clot already exists at the point operated on; and Phillips observed that it required forty-eight hours to obstruct a vessel by needles through it.

About sixty cases of aneurism or other tumours containing blood have been treated by this method in the human subject.

[In a table given in Ciniselli's Memoir on the subject, out of 50 cases treat-

ed by electro-puncture twenty three were cured, twenty were not cured, and seven died.]

The most common and important accident which follows electro-puncture is inflammation of the sac or of the surrounding tissues. More than half of the total number of patients thus treated have suffered from it in a greater or less degree. In the sac it may be produced by the simple rapid deposition of a clot, whatever be the means employed; but much more often in the cases before us it was excited by the chemical action of the electric current. This, however, is a form of the accident, which, although it may be followed by unpleasant or even fatal results, yet not unfrequently, when slight, tends to a prosperous issue by its own proper action. In fact, a very considerable number of the cures have been consequent to it. A much more serious effect of the current is cauterization of the skin or of the aneurismal wall in the course of the needles. This has occurred only too often, and has led, as might be expected, to troublesome complications in the shape of abscess, sloughing, and hemorrhage. In some a slough has been at once produced, and bleeding has accompanied its separation, or has been restrained only by the coagulum deposited at the same time, or by the use of styptics over the ligature. In others an abscess has formed in the wall after a varying interval, and has opened externally, in many, fortunately with no bad result.

The most intense pain is often experienced during the operation, so great, indeed, as to cause convulsions and alarming syncope. A patient of M. Petrequin utterly refused to allow a second introduction of the needles, though success seemed certain. M. Abeille, in describing his very successful subclavian case, states that the patient, having emerged from the influence of sulphuric ether during the operation, uttered the most piercing shrieks, and was fearfully convulsed. "She poured," he says, "with sweat. No surgical operation can cause such tortures."

This pain, however, is found to be present only when the other accidents I have mentioned are so; and many have experienced nothing further than the shock upon opening and closing the current which was felt by Mr. A. This shock, of course, must generally exist; for, although the blood itself is not sensitive, it conveys the elecricity to the wall, just as, if the hand be immersed in water when an electric current is acting, the influence of the wires will be felt even though they be not touched. Such a sensation, however, is hardly perceptible except with a powerful battery. Pain would, indeed, be felt were the interrupted current used, as it not unfrequently was; but for other reasons that form of galvanism ought not to be employed.

Leaving out of account the accidental puncture of a nerve, pain then depends on cauterization in the aneurismal wall. This, as also the main source of danger, we must aim at avoiding, and I think we are in a position to do so. In most of the operations the needles were coated with varnish, having a certain length at the point uncovered. But this was found to be unsatisfactory. The varnish was imperfect or stripped in introduction, and cauterization still often took place. Restelli complains that in conducting his experiments he could get no varnish to effect perfect insulation. Ciniselli, impressed with the fact that this was almost the sole source of danger, and believing that no insulation could be made perfect, supposed that he could prevent the action on the tissues where it is most serious, viz., at the negative pole, by passing through it a current of positive electricity in the first place. The object may be attained, as was proved in Mr. A.'s case, by coating the needles with glass; although it has also the disadvantage of being liable to strip. One of them did so, and it was withdrawn, covered with sealing-wax, and re-introduced by its original puncture. Dr. Wright had suggested the use of vulcanite, which could be polished to perfect smoothness with the rest of the needle, but, through a mistake of the maker, glass was applied to the second as well as the first set. There is, moreover, a little difficulty in applying vulcanite. It is evident, however, that glass, sealing-wax, caoutchouc, or gutta-percha, will act as perfect insulators, and can be readily enough applied to the metal. I have found them to be so in various experiments and prefer, on the whole, a mixture of shellac and gutta-percha. If care be also taken to avoid touching

with the unprotected point any portion of the inner surface of the sac, there is no danger from chemical decomposition; while thermic action is easily prevented by merely keeping the electrodes sufficiently apart.

Another source of danger may be avoided by a very simple precaution. The needles should be of the smallest size consistent with sufficient strength, for the tense wall of an aneurism is liable to inflammation from slight causes. In Mr. A.'s case the first pair were fine, and their course was invisible at the post-mortem examination. With a view to more rapid action, the second pair were made as thick as stocking wires, and the consequence was the escape of air along their sides, and slightly in the cellular tissue, extravasation of blood in their course, and on the left side the formation of a drop or two of pus. The electric action may be increased, if required, by increasing the number of electrodes, which might be united together after insertion. Several minute punctures are evidently better than two large ones. Such a course would, moreover, afford a large number of points for subsequent fibrinous deposit, if the sac were not filled with coagulum at one sitting. If, again, it be true that slowness of formation adds to the firmness of the clot, we may thus have it so without retarding the operation as a whole. Not that we should follow the example of the operator, who inserted no less than fifty needles into a popliteal aneurism, and who had, on withdrawing them, to combat hemorrhage not a little alarming.

The dangers of introducing air into the circulation and of producing emboli, have been much discussed. No inconvenience has been experienced from either in the past, and but little apprehension need be entertained for the future. The tough gas-containing froth tends to rise to the surface, is very little likely to be carried into the circulation, and is so rapidly absorbed that it can never pass the capillaries. Moreover, to produce untoward effects, the introduction into the heart of a quantity of gas much larger than can thus be produced would be required. The clot, again, adheres so closely to the needles, and is in itself so consistent, that with difficulty could any portion be carried away. Many experiments have been made on animals with arteries unligatured and uncompressed, and in none, so far as was observed, did gas or emboli pass down the vessels. In the human subject the artery has often been obliterated along with the aneurism, and even then no embolism has ever taken place, though the arteries were leading trunks of the body.

We have thus glanced at the dangers. Let us examine now the modes in which cures occur. They are three in number.

1. In a few cases the surgeon, on concluding the operation, fears that it has failed; but, after some hours or days, observes the tumour gradually becomes harder, till at length its pulsation disappears, and in time it is slowly absorbed.

2. Sometimes, on the other hand, the pulsations cease, and the aneurism becomes firm during the action of the current. One of two results may then follow. With the majority condensation and absorption proceeds; but pulsation may, I believe, return, through partial melting of the clot, and the aneurism remain half cured, or be filled up by subsequent deposit of fibrin.

3. A third mode is by inflammatory action, which begins to show itself in some almost immediately, in others after the lapse of a day or two, by the usual local and general symptoms. These subside (but rarely without giving ground for alarm) and the tumour is left fully solidified.

In order of frequency, cures have taken place most often with inflammatory accompaniment; next, by immediate and complete coagulation; lastly, by partial clot, with subsequent fibrinous deposit. But the most frequent is precisely that which we wish to avoid; and our efforts must be directed to attain such thorough mastery of our instrument as may enable us to produce invariably one of the two last mentioned. We approach this certainty by ascertaining the causes of accident; but a knowledge of the blood-changes induced by electric currents would greatly assist our endeavours. Various theories have been framed to account for the coagulation, but none of them are entirely satisfactory. According to some it is a special action of electricity unknown and *sui generis;* while others hold that an elevated

temperature in the trajeet of the current is the efficient cause. But the most popular view held by Bellingham, Rostelli, Apjohn, and others is, that the acid produced at the positive pole by decomposition of salts in the blood is the agent in coagulating the albumen. Althaus and others even go so far as to recommend that the negative pole be not inserted at all, but applied to the aneurismal wall as a flat plate. This is not only unsatisfactory, because with a sufficient battery the skin might be cauterized, and the decomposing power lessened, but is founded on a theory, to a certain extent erroneous.

To determine the point we are discussing many experiments have been made on all kinds of animal fluids, and with the most contradictory results. Passing these over let me refer to some observations made by Smee and Van Deen, which are especially valuable, because they were made without the slightest reference to the medical application of electricity. Dr. A. Smee has shown that by passing oxygen through an albuminous fluid, a substance resembling fibrin may be obtained, and that this transformation is assisted by a temperature of 98° to 110°, by making the fluid slightly acid, and by the presence of a serous membrane in it. He has also shown that by the passage of hydrogen through albumen, jelly-like and viscid substances may be produced, which he calls artificial chondrin and mucin, and that alkalinity assists their formation. These actions are extremely slow, are rapid only when the oxygen and hydrogen are produced by the electric current from the fluid itself, and then probably there are other agents at work. It is interesting to note that Van Deen, in a series of experiments on the chemical action of electricity, obtained from albumen, among other things, a substance which he characterized by putting the word fibrin within brackets, and with a mark of interrogation after it.

With the help of my friend Dr. Fraser, to whose skill in these matters I am largely indebted, I commenced some experiments on albuminous fluids. They are by no means sufficiently numerous or varied, and we intend to continue them; but some of the results we have already obtained may not be uninteresting. We used a Bunsen's battery of six cells, with generally platinum wire at the extremity of the conductors, and contrary to the assertions of Baumgarten, and more especially of Steinlein, the platinum proved a most admirable pole.

From experiments on white of egg we obtained the following facts:—

1. With the continuous current the characteristic action was produced rapidly, even with the poles four inches apart; but it increased as the electrodes were approximated, till, with one inch between them, they so coagulated in half an hour nearly one cubic inch of the fluid that nothing was spilled on the inversion of the glass. Parallelism or obliquity of the poles made no perceptible difference.

2. The interrupted current was almost inert till the electrodes were brought within a quarter of an inch of each other, and even then the coagulation was very slow, though of the same character as the other. As to the nature of the coagulum, our results correspond very much to those obtained by Smee, varying somewhat according to the specific gravity and nature of the fluid. We have acted on pure albumen of high and low density, on impure albuminous fluids, such as serum, defibrinated blood, the contents of a hydrocele, as well as upon blood itself in and out of the body. In every instance have we obtained a coagulum, and everything tends to the conclusion that in an aneurism the froth and jelly which arises at the negative electrode, would last, at all events, sufficiently long to form a most efficient nucleus, while the opaque mass at the positive pole would permanently remain unaffected, either by the water or any amount of alkalinity in the blood. It would make this paper too long to detail our numerous experiments, in the course of which we met with several interesting facts, which we propose to arrange in another essay. Let me only say, that in the blood itself we have observed that three substances are produced, viz., a viscid froth by the negative electrode, a black coagulum very distinct in the midst of florid arterial blood at the positive, and in addition to these a quantity of dark defibrinated fluid. In the living arteries, also, we have succeeded in producing coagula with each pole sepa-

rately, as well as with the two in the same vessel. It is difficult to estimate the value of the treatment by galvanism. Judging simply by its past performances, many have thought that it should be restricted to aneurisms beyond the reach of other means, or where such means are so dangerous as to be practically unavailing. I have shown, however, that most of the accidents have arisen from faulty apparatus and manipulations; and I believe that if the following precautions were taken, our statistics would be extremely favourable:—

1. The needles should be carefully insulated.
2. They should be prevented from touching each other or the sac, and to attain this are best introduced from the same side, parallel to each other, and from one to two inches apart. Thus, also, their action, while sufficiently rapid, is not too localized.
3. They should be extremely fine, but may be multiplied according to the size of the aneurism. They should be made of steel, but had better be coated with gold, as the steel is eaten away with great rapidity, and the surface acting is thus diminished.
4. A battery of medium strength, and with a continuous current, should be employed. Four to eight of Grove's or Bunsen's cells are sufficient for a large aneurism.
5. The action may be continued till pulsation stops, or gas can be clearly detected by percussion.

Our knowledge of the immediate action of electricity on the blood is not yet so thorough that we can confidently predict success, but we can, I think, so eliminate sources of danger, that only want of success will be the result in any case. Electropuncture might, therefore, be employed, and for myself I would not hesitate to employ it wherever the arterial orifice is of moderate size, and I believe it might even be farther extended. Cirsoid aneurisms, and those not seated on the main artery of a limb, are, of course, especially well adapted for its use; and in cases similar to Mr. A.'s it affords the only, but that a good hope of relief.

It is deserving of much more attention than has been accorded it in this country, were it only on account of the admirable cures effected by Petrequin, Abeille, Ciniselli, and Nélaton.—*Edinburgh Medical Journal, April* 1866, *p.* 920.

56.—ON LIGATURE OF THE FE ORAL ARTERY.

By Professor Syme, Edinburgh.

[In a paper in the Edinburgh Medical Journal for 1841, Mr. Syme showed that ligature of the femoral artery could be applied almost with perfect safety. His words were]

"It is established that the great sources of danger from the ligature of large arteries are undue laceration and separation of the connexions of the vessel, whence hemorrhage is apt to ensue, and injury to the coats of the veins, which is apt to occasion inflammation and an obstructing coagulation, causing mortification of the limb. The subclavian artery, when tied at the external edge of the scalenus, lies at some distance from the vein, and neither the carotid nor the external iliac artery adheres so intimately to its accompanying venous trunk as to render it at all difficult or dangerous to pass the needle. But the femoral artery has a closer connexion with the vein, and though it is felt by the operator's finger, after the fascia has been opened, round and distinct, and as if insulated from the surrounding parts except by the loosest connexions, any attempt to pass the ligature without further dissection, either proves abortive, or, if executed by force, exposes the patient to the greatest danger. I have seen a gush of dark-coloured blood proclaim transfixion of the vein. I have seen, on dissection, a portion of this vessel included in the ligature; and I have also seen the external coat alone grazed as it were by the needle, but, nevertheless, excited to fatal inflammation. If, on the other hand, this danger be avoided by using blunt instruments, or the

finger, to detach the artery from its connexions, the patient is exposed to the hardly less disastrous consequences of hemorrhage through ulceration or sloughing of the vessel.

"To tie the femoral artery safely, the surgeon should be impressed with the conviction that the operation is one not of difficulty but of great nicety. He should make an incision between two and a-half or three inches long in the proper situation, cut through the fascia to a smaller extent, and expose the sheath of the vessels. So far he can hardly go wrong; but then, instead of hastening to pass the needle, he should by ligature, or the temporary application of spring forceps, close every little vessel that discharges enough of blood to obscure distinct vision of the object he has in view. Let him now seize the sheath with dissecting forceps, and, gently raising it, make a small opening by means of a straight narrow sharp-pointed knife. The cellular and fatty substances which envelope the vessels in variable quantity are next to be elevated and divided in successive portions until the external coat of the artery appears quite distinct and *white*, when the needle may be passed without the slightest difficulty or danger."

At the time when this was written, my experience did not extend beyond seven cases, all of which had proved successful; but since then there have been twenty-eight more, so as to make in all thirty-five, and in none of these has the operation proved unfortunate with the exception of one, which terminated fatally from suppuration of the sac. But even on this solitary occasion the result could hardly be attributed to the operation, as the parts concerned with it were found perfectly sound and free from the morbid condition, which seemed distinctly referable to the derangement of health that had been induced by a prolonged attempt to cause coagulation through pressure at the groin. This treatment, continued for many days and nights, had disordered the patient's sleep, appetite, and mental composure, so as in all probability to excite a proneness to unhealthy action. Should such an explanation not be deemed satisfactory, the result of my experience in ligature of the femoral artery would still be one death in thirty-five cases.—*Edinburgh Medical Journal, May* 1866, *p.* 967.

57.—ON THE TREATMENT OF POPLITEAL ANEURISM.

By John Hilton, Esq., F. R. S., Surgeon to Guy's Hospital.

S. W., aged 44, married, a tall and healthy coachman. Six weeks ago he "strained" his left leg while carrying pails of water, and immediately afterwards felt stiffness of the knee, with severe cramp down the calf and in the sole of the foot. On examining the ham he found a tumour larger than a duck's egg. Within a few hours of the accident the leg below the knee became swollen and œdematous, but the swelling subsided in a few days. He has continued at work till now.

Dec. 18th, 1865.—*State on admission.*—There is a spherical tumour, with a diameter of two or two and a-half inches, situate above the centre of the popliteal space, soft and easily compressible, and pulsating regularly. On flexing the tibia to an angle of eighty degrees, pulsation is arrested in the anterior and posterior tibial arteries; and on flexing still further, the tumour itself ceases to pulsate. Pulsation is also readily stopped on pressing the femoral artery.

Treatment and progress of case.—Ordered to keep his bed. Leg bandaged and elevated, so as to empty the superficial veins, which are large, varicose, and full, especially about the calf. One grain of opium at bedtime, if restless.

Dec. 26th. Leg firmly bandaged, and knee bent to an acute angle so as entirely to stop pulsation in the aneurism.

30th. On examining the aneurismal tumour, it is found to be only half its former size and much harder, yet still pulsating. Pulsation in the superficial femoral artery scarcely distinct; an inch below Poupart's ligament the cir-

culation is carried on mainly through the profunda. Leg rebandaged, and again bent at the knee-joint; a double tourniquet, which stops all pulsation in the tumour, is applied over the superficial femoral artery, and a pad is strapped over the aneurism itself.

Jan. 5th, 1866, (ten days after first flexion; six days after applying tourniquet). Tumour about the size of a walnut, hard and solid, not pulsating. It has continued the same till the present date, Jan. 16th.

The man at first had severe cramping pains in the calf and the sole of the foot. Some sudden dilation of the popliteal artery would perhaps explain these pains, pressing, as it probably would, upon the internal popliteal nerve, and causing the pain and cramp referred to. The popliteal vein, lying between the artery and the nerve, would be pressed upon by the aneurism; and the pressure interfering with the return of blood by the vein would cause the œdema of the leg, which is reported amongst the earliest symptoms of the injury.

This aneurism was cured by simple flexion, for which method of treatment the case seemed to me to be peculiarly adapted. The aneurism was situated near the *upper part* of the popliteal artery, close to where it passed through the adductor magnus, and consequently in a comparatively fixed spot. I take it that the middle of the popliteal space is that spot which is opposite the interval between the tibia and femur. Now, the aneurism was altogether above such a centre—to which fact, I think, may be in part traced the readiness with which it was cured by flexion. Bearing in mind what I have already told you, that the artery where it passes through the adductor magnus is comparatively fixed, I think you cannot fail to perceive that, when the leg is flexed to an acute angle on the thigh, the altered direction of the continuous trunk of the popliteal artery is so sharp, so bent upon itself, that it tends to stop the supply of blood to all the parts below it; whereas, when the aneurismal enlargement is placed lower down, the leg cannot be so completely fixed, nor the artery so changed in its course as to interfere with the transmission of blood. I have seen several cases of aneurism situated in the middle and lower portions of the popliteal space, which could not be cured by long-continued flexion. The explanation just given is probably sufficient to account for the want of success. My belief is, that the higher the aneurism is situated in the popliteal space, the greater is the probability of the cure being effected by flexion, provided only that there be a short piece of the popliteal above the aneurism.

One element against the favourableness of this case was the fact of the tumour being very soft—its contents quite fluid. You know that an aneurism is cured by layers of fibrin forming on the wall within the sac. When these layers have been deposited, they communicate a feeling of hardness to the touch. The absence of this special sensation, therefore, indicated that no layers of fibrin had been formed; and that, consequently, nature had not yet commenced to cure the malady.

The principle of the treatment was to arrest ultimately the flow of blood through the popliteal vessel. Now, it was found that by bending the knee to an angle somewhat less than that of eighty degrees all pulsation was stopped in the anterior and posterior tibial vessels, and in the tumour itself; so, if the man could bear that position for a few days, it was almost sure to cure his aneurism. No alteration was made in his diet; nor was any important medicine given to him. He was simply compelled to keep in bed; his leg was bandaged and elevated on a pillow, so as to empty the varicose veins and get rid of any œdema there might be below the knee. And after eight days, the leg being firmly bandaged to prevent any swelling, the circulation was entirely arrested in the sac by the knee being fixed at an acute angle. You will remark the simplicity of the means adopted to accomplish all that was desired. After four days of the above treatment the aneurism was found to be diminished to half its former size; harder, but still pulsating. This result was certainly satisfactory, especially the commencing hardness, which indicated that coagulation was taking place within the sac. Pulsation of the superficial femoral artery, between the popliteal and the point from which the

profunda femoris comes off, had been so far stopped that it could scarcely be felt. This again was in favour of cure, because the femoral artery, not being distended to its natural size, would contract and interfere with the transit of so great a quantity of the blood-current to the aneurism. Very little pressure with the finger upon the femoral artery below the profunda at this time arrested the pulsation in the sac. Two clamps were, therefore, placed over the vessel; two being used in order that one might every now and then relieve the other, and the patient be less inconvenienced than had one alone been continuously applied. The pad over the aneurism was to favour the emptying of the sac of fluid blood. The result is that the patient has been cured by most simple means (first suggested by Mr. Ernest Hart) of a dangerous malady, without recourse having been had to the operation of tying the femoral artery, an alternative which is not unattended with considerable danger to the patient's life.

Having observed how change of direction of the arterial trunk acted as a curative means in this case by diminishing both the current and its force, let us see if there are not other kinds of cases where the same simple method of relief may be applied. Probably all the structures of the body only perform their functions in perfection as long as they retain their normal direction and position; but this rule applies especially to arteries. In cases of hemorrhage from the hand occurring from several points simultaneously, you will find that if the hand be forcibly flexed on the forearm, the flow of blood in the radial and ulnar arteries will be so much arrested that the hemorrhage will cease; or, as that flexion, if long continued, is painful, raise the arm vertically from the shoulder, and the hemorrhage will equally cease. The subclavian artery describes a curve normally with its convexity upwards and outwards over the first rib. When the arm is extremely elevated, the convexity of this curve becomes directed downwards and outwards, and the free flow of blood through the vessel is interfered with. I remember a case of hemorrhage from the hand where styptics, the actual cautery, pressure, and other means had failed to arrest the bleeding, and where it was thought that either the radial and ulnar arteries or the brachial would have to be tied. As a last resource, the man was placed upon a bed, his arm was elevated over his shoulder, and the hand tied up to the bed-furniture in that elevated position. The hemorrhage shortly ceased, and the patient did well. Therefore, I may recommend to your notice as a powerful therapeutical agent, in certain cases of hemorrhage, a change in the direction of the arterial trunks.—*Lancet*, *Feb.* 24, 1866, *p.* 195.

58.—ON A CASE OF WOUNDED ULNAR ARTERY, FOLLOWED BY SLOUGHING AND SECONDARY HEMORRHAGE, AND REQUIRING AMPUTATION.

By JOHN HILTON, Esq., F. R. S., Surgeon to Guy's Hospital.

[The ulnar artery was wounded by a cut and the hemorrhage arrested by a pad. Secondary hemorrhage ensued. The wound was opened out and the artery tied above and below the source of hemorrhage. Sloughing, however, extended far up the arm, the radial artery and the tendons of the front of the arm also sloughing. As there was no hope of regaining motion in the hand, and the case assumed so serious an aspect, amputation was performed above the elbow joint.]

After a short time pyæmia supervened. We looked out anxiously for the local manifestation, by symptoms, of the general diseased condition. No one can foretell whereabouts the secondary effects of pyæmia will occur; so it behoves you, in every case of pyæmia, to watch the patient, in order to detect as soon as possible the occurrence of any local mischief. Inflammation of kidneys occurred (probably of the left for he had pain and tenderness in the left loin on pressure), giving rise to blood and pus in the urine during many days.

You will inquire, How was he treated during all this time? Well, two things are essentially necessary in pyæmia: *stimulants*, to prevent the deteriorating influence of the pyæmic condition of the blood from so depressing the heart that the patient dies from failure of the heart's action; and *abundant nourishment*, so that the blood, from which all the tissues of the body have to be sustained and repaired, may be renewed by its appropriation. The principle regulating the constitutional treatment of pyæmia is plain and simple. The local symptoms, however, must be promptly and accurately attended to; all local collections of pus should be at once let out, &c., &c.

To return to our case. The man had secondary abscess near the shoulder-joint; it was opened above and anteriorly, but it was incompletely emptied by the incision, and the man suffered fresh pyæmic distress. I tried by continued exact pressure to keep the abscess empty; still the pyæmic distress continued, and the man's life was in great danger. I then made a deep and large opening into the abscess at its lowest part at the back of the axilla; a quantity of stinking pus was let out, and the pyæmic symptoms diminished immediately. The man still continues to improve, and, so far as I can see, now seems free from danger. Observe that the diet and all other influences were the same both before and after the abscess was opened the second time. Does it not therefore seem reasonable to suppose that the opening of the abscess at the most depending part, and the allowing of the putrid pus to drain away as soon as formed, explain his marked improvement? Undoubtedly. Yet, why so? What is the explanation? My idea is that the unhealthy matter in the abscess was being absorbed, and by poisoning the blood gave rise to what is termed constitutional hectic. I venture to reiterate the opinion which I have oftentimes expressed in my systematic lectures on Surgery, that the pathological cause of hectic fever, associated with chronic abscess, or following its being opened, is absorption of unhealthy fluid engendering pyæmia. On the other hand, it is right I should tell you that others explain this hectic condition by supposing it to depend on some ill-defined nervous morbid influence exerted primarily by the ganglionic system of nerves which induces the general disturbance.

At any rate we may learn from this case that when a collection of fetid matter is confined within the body, causing general disturbance, it should be at once let out by an opening made into its most depending part.—*Lancet, Feb.* 24, 1866, *p.* 197.

THE ORGANS OF URINE AND GENERATION.

59.—ON THE PROOFS THAT LITHOTRITY IS AN EMINENTLY SUCCESSFUL OPERATION.

By Henry Thompson, Esq., Surgeon to University College Hospital.

[The operation of lithotrity was performed successfully by Mr. Thompson, twenty-three times during the year 1865, and the mean age of the patients was sixty-two years.]

As stated at the conclusion of a preceding paper, I have operated by lithotrity successfully in forty-four *consecutive* adult cases during the last two years and a half, the mean age of the forty-four being also sixty-two years.

I still did not presume to adduce numbers sufficiently large to offer any absolute statement respecting the rate of mortality after lithotrity, or as to the result of any given method of performing the operation. But I think I may fairly lay claim to having established the following propositions:—

1. That lithotrity is capable of freeing elderly patients from stone at a very small risk, if well-constructed instruments, with delicate manipulation, be employed, and if watchful care be exercised in the management of the patient.

2. That the success of lithotomy has never been in an any way comparable

with that which I have been able to report as the result of the crushing operation.

I have only a few words to add relative to the mode of performing the operation. The principles enunciated in the paper of 1865, and also in greater detail in my work "On Lithotomy and Lithority," have been strictly adhered to—viz., to reduce to its minimum the employment of instruments of any kind; to make each sitting as short as possible; to pulverize the stone, or bring it to the condition of very small fragments, by the use of flat-bladed lithotrites; to trust very much to the natural efforts of the bladder for the removal of this débris, and not greatly to artificial means.

Respecting the first condition, I may say that I have not in any single instance injected the bladder before operating, but have always used the lithotrite with any amount of urine which might happen to be present in the bladder, so that all instrumental manipulation, and all disturbance of the bladder by injecting, have been saved. As a rule, also, the lithotrite has been introduced once only at each sitting; in very exceptional circumstances only twice. Neither the evacuating catheter and syringe, nor the double-current catheter, have been ever used.

Respecting the second condition, the lithotrite has rarely been permitted to remain in the bladder for more than one minute or a minute and a half at a sitting; often only for a shorter period. In searching, near to the termination of the case, for a last fragment, two or three minutes may occasionally have been so occupied.

Respecting the third condition, I have very rarely found it necessary to use the fenestrated lithotrite more than once at the outset of a case in dealing even with a rather large stone. For subsequent sittings, and for smaller stones at the outset, the flat-bladed instrument, which is much less irritating to the bladder and pulverizes more completely, has been alone employed.

Respecting the fourth, I have removed more débris artificially than heretofore; but not by means of the lithotrite, nor by washing out in the ordinary manner—that is, by a powerful injection of six or eight ounces through a large catheter, a method to which I entertain the strongest possible objection: first, as being almost useless to remove débris; and, secondly, as being exceedingly irritating to the bladder. But I have found Mr. Clover's new instrument very useful for removing such débris in those cases where patients have been unsuccessful or unduly slow in expelling it, and also in some instances where I have thought it advisable to remove the results of the crushing at the time of the operation. By its means a flux and reflux of so small a portion of water suffices to attain the object, and the action is so much more efficient than any other mode of washing out, that I cannot but regard it as an important addition to the appliances of lithotrity. It removes large and sharp angular pieces which would otherwise have troubled the urethra conisderably in the act of performing their transit. At the same time it requires care, and not a little nicety in its application; and even then I consider it rather more trying to the patient than a single application of the lithotrite. As a rule, it appears better, therefore, to use it in those cases in which the patient is placed under the influence of chloroform; and then not only is the stone crushed, but the results may be removed at once, and little or no débris is passed until after the next sitting. In cases of atonied bladder, in which no urine can be passed by the patient's efforts, it is exceedingly valuable, since in these patients the bladder is so accustomed to mechanical contact, from the daily employment of catheters to remove the urine, that the organ will bear with impunity a comparatively protracted use of the lithotrite and of the injecting apparatus afterwards. There is certainly no case for the employment of lithotrity which I regard as so free from risk as that of an elderly patient who has lost all power to empty his bladder for years, and is consequently compelled to pass a catheter several times daily.—*Lancet, Feb.* 24, 1866, *p.* 200.

60.—ON A NEW INSTRUMENT FOR FACILITATING THE OPERATION OF LITHOTOMY.

By Henry Lee, Esq., Surgeon to St. George's Hospital.

[Sometimes considerable delay occurs in the seizure of the stone by the forceps after an opening has been made in the bladder. To obviate this difficulty Mr. Lee has devised an instrument which]

Consists of two blades like an ordinary lithotrite, but the lower blade is deeply grooved on the right side, the groove terminating abruptly half an inch from the commencement of the curves in the blades. During the operation, the stone is first seized and fixed between the blades of the "lithocrate." The instrument is then drawn as far forward as the separated blades will allow, and the patient is placed in the usual position for lithotomy. The ordinary lateral incision is then made, and the groove on the lithocrate directs the point of the knife to the prostate gland. The knife should be carried as far as the groove will allow. If the stone be small, the blades of the instrument will be separated to a small extent, and a small incision in the prostate only will be made. If the stone be larger, the lower blade of the instrument, in which the groove is made, will project further into the bladder, and a corresponding larger incision will be made in the prostate gland. After the incision is made in the perineum, the prostate is dilated with the finger, and pressed back over the stone at the same time that this is pulled forward. The stone is then removed entire through the opening in the perineum. Should the stone slip from between the blades of the lithocrate, it will be left within reach of the finger at the neck of the bladder, where it may be seized with the ordinary lithotomy forceps, or removed with a scoop. The advantages anticipated by this mode of operating are:—

1. The stone is seized before any incision is made.
2. The stone is held at the neck of the bladder, and may be there felt when the finger is introduced, and consequently the amount of dilatation required may be ascertained.
3. A straight groove, instead of a curved one, is presented for the incision in the urethra.
4. The length of this incision through the prostate gland is accurately determined: a small incision is made when a small stone is seized by the lithocrate, and a larger incision when the blades of the instrument are further separated by a large stone.—*Lancet, Dec.* 23, 1865, *p.* 699.

61.—ON THE TREATMENT OF ACUTE ORCHITIS BY PUNCTURE AND COMPRESSION (IMMEDIATELY AFTER PUNCTURE) BY STRAPPING.

By Dr. W. Spencer Watson.

[Mr Henry Smyth, of King's College Hospital, about a year ago drew attention to the value of puncturing the testicle in cases of acute orchitis (*Retrospect*, vol. l., p. 183). Dr. Watson now offers a suggestion as to the combination of a further proceeding with, and in addition to, that of making a puncture.]

The first few cases in which I tried the plan of puncturing alone gave very satisfactory results, in almost all cases the patients expressing themselves as being relieved at once, *i.e.*, as soon as the escape of serum from the tunica vaginalis had relieved the tension of the parts, and it seemed to me that this escape of serum alone was the chief use of the puncture. I therefore tried the plan of puncturing the tunica vaginalis only, and letting off the serum by a grooved needle, and in one or two cases this seemed to give relief for

the time; but under both these plans I soon found that in a few days the opening would close and the pain and tension return as before. Under such circumstances it was necessary to make a second puncture, but few patients would submit to a repetition of this proceeding, and I therefore found it necessary to adopt the plan which I have since followed, and with which I have every reason to be perfectly satisfied.

The plan consists in puncturing the testicle through the front or lower part, so as at the same time to let off the serum in the tunica vaginalis, and to relieve the tension of the tunica albuginea, and as soon as the bleeding has ceased, (the quantity which escapes being in all cases very small, seldom more than an ounce,) to apply a firm case of strapping, put on as tightly as the patient can bear it. At the same time I prescribe a full dose of opium, and direct it to be repeated twice or three times. As an instance of this plan of treatment, I may mention the following case:—

J. H., aged 20 years, came to King's College Hospital, under my care, on May 8, 1865. He had had a gonorrheal discharge since Dec., 1864, for which he had been treated without injections. On the day previous to my seeing him he had been drinking freely, and the left testicle became suddenly enlarged and very painful and tender. The testicle was punctured and strapped immediately, and opium 1 gr. prescribed.

May 10. He was in great pain, but had been able to sleep the night before. Strapping was again applied and opium repeated.

12th. The strapping having become loose was again applied. He is now free from pain.

17th. The testicle is now very little larger than the natural size, and is free from pain or tenderness.

Thus in one week the acute symptoms of this case subsided, and the testicle was restored to very nearly its normal condition. In other cases the same result has been obtained at the end of a fortnight, and this is a much more rapid resolution than can be obtained by the ordinary method of treatment.

I am indebted to my colleague, Dr. Jno. Harley, for the suggestion of the plan of treatment by strapping; he having adopted that plan without a previous puncture; but I believe that the previous relief of tension materially assists in the restoration to health of the parts involved, and this more particularly where there is a considerable effusion of serum into the tunica vaginalis, for it is quite clear that when this is the case, no efficient compression can be made on the body of the testicle. It is also to be observed that after the escape of serum and blood, the size of the whole swelling is materially diminished, and the compression made will be proportionably effective, while the pain experienced by the patient will be greatly lessened.

As a further recommendation for this method of treatment, I may observe that should bleeding prove troublesome after the puncture, as I understand was the case in one case so treated by Mr. Henry Smith, this would be a most effectual way of restraining it. I have myself never seen more than an ounce of blood escape from the puncture, which I generally make with a Syme's knife, and most frequently not more than a drachm of blood is seen.—*Med. Times and Gazette, Nov.* 18, 1865, *p.* 563.

DISEASES OF THE SKIN.

62.—THE ACTUAL CAUTERY FOR NÆVI.

By J. R. A. Douglas, Esq., Hounslow.

[Dr. Douglas considers the actual cautery far less painful than nitric acid and other caustics, for the destruction of nævi in cases where the ligature is inadmissible or fails to arrest the vascular growth.]

Three cases lately cauterized by me illustrate well the use of this plan. An infant three weeks old I cauterized on the upper eyelid for a circular

nævus, which was growing rapidly, and would soon have produced ptosis. Two applications quite arrested its growth; on the second, the nævus was raised by a tenaculum passed through it, which was burned down upon. Months have elapsed since the operation, and the only residue is a whiter mark than the skin where the vascularity was.

Another case was that of the smallest seven months' child I ever saw reared. A nævus appeared soon after birth at the extreme edge of one nostril, which, on examination, was found to pass up the orifice for some distance. At nine months old many applications of the nitric acid had been used, giving great pain; but the growth continued to increase. I applied the actual cautery freely to the nævus, passing it up the nostril, and using some momentary tension with the semi-button-like head of the curved instrument, to press well into the depression near the point of the nose. This has totally arrested the growth, leaving no deformity (so important in such a position).

The third case I shall trouble you with was that of a child a year old, with a large raised nævus in the middle of the cheek, with large varicose-looking vessels attached to it running back to the neck; the stain apparent through the cheek on opening the mouth. The nitric acid had twenty times been applied, the child crying for hours after its use. The ligature was thought inadmissible in London from the large surface that would be implicated. I applied a cautery half the circumference of the nævus in the middle of it; it apparently gave little pain, which soon ceased. After the surface had healed, I found it had so contracted that the same instrument covered the remaining part, which having very florid edges, was spreading again. The second application has arrested all morbid growth; the vascularity gone; the large vessels almost imperceptible. I attribute all this to the known contractions of the cicatrices of burns. There is no distortion of feature, but a white irregular scar. Under these circumstances, I feel myself at liberty to urge the use of the nearly obsolete cautery in these cases. Such instruments are easily formed with the file from flat-headed bolts, nails, and screws, obtainable of every country ironmonger.—*Lancet, March* 24, 1866, *p.* 328.

63.—ON THE NATURE AND TREATMENT OF CHILBLAINS.

By Dr. Henry Samuel Purdon, Physician to the Belfast Dispensary for Diseases of the Skin.

Chilblains are an inflammatory state of the cuticle arising from exposure to cold. *Dermatitis congelationis*, as Hebra calls it. Now the specific inflammation (if I may so term it) that causes chilblains extends to the deeper structures of the corium, frequently to the cellular tissue beneath it, and is not primarily subcutaneous. In chilblains, like other inflammations, we have increased vascular action, followed by subcutaneous infiltration.

This affection is ushered in by a feeling of heat and itchiness, combined with redness and swelling of the affected part; the sensation of itching is aggravated by warmth. In some cases vesicles make their appearance, accompanied by a weeping of serum, being a typical eczema of Willan; in other instances by numerous fissures, the eczema fissum of Wilson, eczema rimosum of McCall Anderson, eczema fendill of the French. When the disease passes this stage, suppuration usually takes place, eventually ending in painful ulceration; indeed, cases have been recorded in which the integument covering the bone has completely sloughed off.

Chilblains are most frequently met with in children and females of a cold lymphatic temperament, and already several cases have been admitted at the Belfast Dispensary. This affection commonly appears on the fingers, toes, and ears, arising from exposure of these parts to cold, when being imperfectly dried after washing.

Towards the prevention of chilblains in those who have been subject to them, bathing the hands or feet, as the case may be in cold water, to which some vinegar has been added, and afterwards brisk friction employed, tends considerably towards keeping the parts in a healthy state.

In the majority of the prescriptions given by authors for the cure of this complaint rectified spirit enters largely. Tincture of arnica, on the same principle, by lowering inflammatory action and causing absorption of any effusion that may have taken place, is extremely useful in unbroken chilblains; but my favourite prescription is that of Marjolin's, and which seldom fails:—

℞. Balsami peruviani, 3ss.; spiritus rectificati, ℥iss.

Dissolve, and add.

Acidi hydrochloridi, 3ss.; tinct. benzoini comp. ℥ss. M.

A little of this is to be rubbed occasionally into the affected part, or if preferred by the patient, a small piece of linen can be moistened with some of the above and applied to the part; a sensation of smarting is usually experienced for a few minutes.

When the chilblain is broken, I find the calamine ointment (Turner's cerate) a very good dressing, and if thought desirable to stimulate the part we may combine it with equal parts of elemi ointment.

If ulceration has taken place the nitrate of silver should be freely used.—*Medical Press, Nov.* 22, 1865, *p.* 456.

64.—CASES OF PRURITUS SENILIS.

By Dr. H. Bnioel, Assistant Physician to the Metropolitan Free Hospital.

George Grant, a coalwhipper, 55 years old; six weeks ago he perceived an intense itching on both legs, particularly of the calves. He was obliged to scratch until the little pimples which had formed in great number on the skin began to bleed, which was followed by relief. Sleep made no difference, and sometimes he awoke covered with blood. In a few days the same condition had spread over the whole body. For some days he has now been free from itching, but as he gets warm in bed the sensation seems unbearable; the more he then scratches, the greater then seems to be the itching.

Dec. 6, 1865. He was admitted as an out-patient to the hospital. The skin bears marks of the scratching in every direction. Injection of one-fourth of a grain of morphia.

Dec. 8. No inconvenience after injection, but very great relief; for the first night, since the beginning of his complaint, he slept without being forced to scratch. Repetition of the injection.

Dec. 17. No recurrence of the former state; patient is perfectly free from irritation, and wishes to be discharged.

George Hyams, an upholsterer, 18 years old, has suffered for one month from an intense and very troublesome attack of *prurigo*, which, as in the former case, is tolerable at day time but unbearable during night.

Dec. 23. An injection was made with morphia (one-fourth of a grain) and no return of the itching has occurred since that day. This is the more remarkable as every practitioner knows how obstinate cases of prurigo generally are to every kind of medicine applied both through the stomach and locally to the skin.—*Medical Mirror, Feb.*, 1866, *p.* 88.

65.—ON ECZEMA.

Mr. John Gorham, of Tunbridge, has tried the plan recommended by M. Erasmus Wilson, of treating the eczema infantum with minute doses of arsenic and one grain of calomel twice a week at bedtime, unless active diarrhœa be present. He uses at the same lint spread very thickly with zinc ointment; the lint being removed every morning, but *not the ointment*, which is allowed to cake on for a week, when it is removed with a spatula, generally exposing a nearly healthy surface. A fortnight's treatment is usually sufficient to reduce the case to a state that may be called convalescent.—*Lancet, May* 5, 1866, *p.* 503.

66.—TREATMENT OF ACUTE ECZEMA.

[Mr. T. P. JOHNSTON, Assistant Surgeon H. M. 26th Regt. N. I., in a note to the editor of the Lancet, gives the following treatment for allaying the itching in a case of acute eczema in a child aged nine months.]

I never find a lotion containing five grains of sulphate of zinc to the ounce of water fail to afford relief in cutaneous affections accompanied with intense itching. The pain it causes soon passes off. I should apply it with a camel-hair brush.—*Lancet, May* 5, 1866, *p.* 504.

DISEASES OF THE EYE AND EAR.

67.—ON EXAMINING THE FUNDUS OCULI.

Dr. ROSEBURGH of Toronto, finds that the fundus of the eye can be examined without the mirror of the ophthalmoscope as follows. The lamp is placed on a table in a darkened room. The patient is seated on a chair two or three feet from the table, and facing the light. The chair of the observer is placed with the back against the table and between the patient and the lamp. When the surgeon is seated, the lamp should be immediately behind the right shoulder and very nearly on a level with the eye. By now bringing the right eye in a line with the lamp and the eye of the patient, but so as not to shade it, if the pupil has been previously dilated with atropine—the fundus is seen to be illuminated. In order to see the details of the posterior internal surface of the eye, a lens of about two inches focus is placed within an inch or two inches of the eye under examination. An inverted image of the fundus can now be seen forming an aërial image about two inches in front of the lens. The optic nerve entrance and the radiations of the retinal vessels can be seen by directing the patient to look at the light or two or three inches farther from the surgeon's eye, but in the same horizontal line.—*British Med. Journal, Jan.* 13, 1866, *p.* 33.

68.—ON THE TREATMENT OF GRANULAR OPHTHALMIA BY PRESSURE.

By Dr. WILLIAM STOKES, Jun., Surgeon to the Meath Hospital and County Dublin Infirmary.

[The disease known as granular ophthalmia, as observed in Ireland, is of a more formidable type, and is more intractable to treatment than in England. The author of this paper wishes it to be understood, that he does not advocate this treatment to the exclusion of other methods which are adopted for the cure of this disease.]

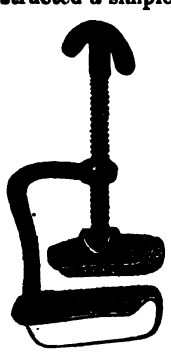
Although, as far as I can determine, no mention of the treatment of granular ophthalmia by pressure, in any form, has as yet appeared in the literature of ophthalmic surgery, it has, nevertheless, been applied. In some of the Egyptian hospitals, as I have recently learned from a trustworthy authority, a favourite treatment for granular lids, which so frequently occurs as a sequela of the so-called Egyptian ophthalmia is to strap down the eyelids by a layer of adhesive plaster. This must produce a certain amount of pressure, which, most probably, accounts for the treatment proving so beneficial. I have also learned from Dr. Wordsworth, of the Moorfields Ophthalmic Hospital, London, during a visit which he paid to Dublin, last Autumn, that he has treated some cases of granular ophthalmia solely by the application of compress bandages, and with satisfactory results.

It was during a conversation with my friend, Dr. Robert M'Donnell, on the subject of granular ophthalmia, that it occurred to me that pressure would, most probably, best succeed if applied to the granular surface of the conjunctiva palpebrarum, without, at the same time, causing any undue pres-

sure on the bulbus oculi. Dr. M'Donnell had already in some cases of granular ophthalmia, inserted between the affected lids and ball of the eye masks made of glass and of the form of the ordinary artificial eyes, for the twofold purpose of preserving, if possible, the cornea from the mechanical irritation produced by the granulations, and also to subject these to a certain amount of pressure. Owing, however, to the difference in the convexities of the mask and the bulbus oculi, the amount of pressure on the latter produced at the edges of the mask prevented the patient from bearing the instrument beyond a short space of time. The problem to solve, therefore, was to devise some means by which a continued pressure could be applied to the granular surface of the conjunctiva, without, at the same time, distressing the patient by any undue pressure on the bulbus oculi. After a number of experiments, the instrument which I devised, appeared to me, at first, to possess all the necessary requirements.

It consists of two highly polished ivory plates made somewhat thicker at the upper part corresponding to the reflected portion of the conjunctiva, at which situation the granulations are generally most abundant and of the largest size; at the lower extremity of the inner plate a very delicate spring, made of gold wire, was attached, terminating in a broad button, which made slight pressure on the anterior or convex surface of the ivory plate. The outer or anterior ivory plate is removable, there being no attachment to the broad button at the end of the gold spring. This instrument I found easy of application and capable of being borne by the patient without any difficulty. The inner plate being first introduced under the eyelid, and the anterior plate then being placed on the outer surface of the eyelid, was held in its place by the spring.

One disadvantage of this instrument was soon obvious, namely—the want of power to regulate the amount or degree of pressure to be applied. To be able to regulate this, is of great importance in treating the disease in different stages of its development. To rectify this deficiency Mr. W. Pearsall, a pupil of the Meath Hospital and a skilful mechanician, devised and constructed a simple but ingenious modification of the first compressor I have described, and by which the pressure can be increased or diminished according as the surgeon thinks fit.



We may now proceed to consider briefly some cases of granular ophthalmia which have been treated by pressure in the Meath Hospital. The first of these was that of S. C., aged 19, who was admitted into the Meath Hospital on the 25th of last August. She had suffered from an acute conjunctival inflammation, three months previously; and at the time of her admission into hospital, the conjunctival surface of both upper eyelids presented numerous and well-marked examples of what are recognized by ophthalmists as pure papillary granulations. There was considerable impairment of vision in consequence of corneal opacity, depending partly on secondary keratitis, and partly on the well-marked development of the superficial form of pannus, or pannus of the first degree, as described by Von Græfe in his clinical lectures. In addition to these, there were several small ulcerations situated, for the most part, on the upper portions of the cornea.

I looked upon this case as one particularly well adapted for pressure, and the result showed that my anticipations were well founded. I commenced by applying the lid-compressor for one hour daily; and at the end of ten days, finding that the patient bore the instrument without any annoyance, and that there was a marked improvement not only in the affected portion of the conjunctiva, but also in the cornea, the pannus having to a great extent disappeared, I increased the time of application to two hours daily. This I continued up to the end of the fourth week. At this time the cornea and affected portions of conjunctiva began to assume quite a normal appearance, and an obvious improvement in the patient's power of opening the lid was observed.

The patient now could bear the instrument for four hours daily; and at the end of the seventh week I looked upon the case as one which had been brought to a successful termination. I did not, however, dismiss the patient from the hospital, as I wished to satisfy myself, if possible, that this signal improvement would remain permanent. At the end of three months the patient returned home.

The next case to which I shall allude is that of J. B., a young woman aged 24, who was admitted into Meath Hospital on the 20th of last September. She had been under treatment in the country for some time previously, and the surgeon under whose care she had been, had repeatedly scarified the granulations, and followed up this treatment by the free application of caustics. This case, which at the time of the patient's admission into hospital presented at and about the upper portion of the reflected conjunctiva, a large cluster of well-formed granulations, I treated by combining pressure with applications of a weak solution of nitrate of silver. At the end of three weeks the patient, at her own request, left the hospital. The improvement certainly was very great, and showed, even in a more marked manner that in the former case, how easily the instrument is borne.

The third and last to which I shall now allude, is that of C. H., aged eighteen, who was admitted into the Meath Hospital on the 12th of last October. This case presented an example of the highest degree of development to which pure granular trachoma may come. The palpebral conjunctiva of both lids were thickly studded with the vesicular or "sago grain bodies," which, in consequence of their form and semi-transparency, the German oculists have not inaptly compared to frog-spawn. The swelling of the conjunctiva, at its reflected portion, was especially remarkable. The cornea presented all the characteristics of an aggravated form of secondary ulcerative keratitis pannosa. At the time at which this report was being made (Dec. 9) the patient exhibits in every respect a most marked improvement both as regards the conjunctiva and cornea; and no treatment has been resorted to in this case except the application of the lid-compressors from two to four hours daily. These cases, few though they be, are sufficient to show that the treatment by pressure, applied in the manner I have described, can be adopted without discomfort to the patient, and without causing any irritation to a cornea, which is the seat of ulceration and pannus. The general improvement, in both conjunctiva and cornea, appears to proceed with more rapidity than it does under the ordinary method of treatment.

One great advantage which I claim for this treatment over the ordinary methods of scarification, and subsequent application of various caustics, is, that there is nothing in pressure which can tend directly or indirectly to the production of that most unfortunate, but too frequent result, not so much of trachoma as of its usual treatment, namely, entropium, with trichiasis. I have little doubt that this condition is more frequently the result of the use of caustics and the scarifying knife than the affection for which these are so frequently, and in many instances so injudiciously, made use of. Certain it is, that nothing can tend more to the development of cicatrices in the conjunctiva, and consequent contraction of that membrane and inversion of the lid, than the injudicious use of those ordinary methods of treatment to which we have before alluded.

The treatment by pressure is also quite free from all danger of the occurrence of diphtheritic and other destructive forms of inflammation of the conjunctiva, which not unfrequently supervene after the treatment by inoculation. —*Dublin Quarterly Journal, Feb.*, 1866, *p.* 38.

69.—THE TREATMENT OF OPHTHALMIA.

The treatment of gonorrhœal ophthalmia and of purulent ophthalmia of infants has undergone considerable change of late years. In place of the heroic treatment of former days—leeches by the dozen, low diet, tartarised

antimony, &c.,—we frequently see only the mildest forms of treatment pursued, and apparently with greater success. At St. Mary's Hospital, Mr. Ernest Hart has cases of both varieties now under treatment, which have been brought through severe attacks successfully by the persevering use solely of weak alum lotions and of the spray douche, with the local application of the *diluted nitrate-of-silver stick* to the mucous lining of the lids. The alum lotion is of the strength of three grains to the ounce; it is to be used every half hour at first, and then every hour and two hours. In the case of a child the head of the patient is to be fixed between the knees, and the eyelids then drawn apart, and the discharge washed away first with lukewarm water, linen rags being used. The nitrate-of-silver sticks are diluted with nitrate of potash, as recommended, we believe, by De Graefe; this localizes the action of the caustic, and limits it to the parts touched. In order to prevent the excess of action from superfluous caustic, Mr Hart uses milk with a camel-hair brush immediately after applying the nitrate of silver. On the Continent it is usual to employ a solution of common salt, but this is in itself irritating. Mr Hart learnt to use the milk from a suggestion of Dr. Horace Dobell, who applies it to the throat, and as a bland fluid it has many advantages over salt water, in the treatment of the eye more especially. The nitrate of silver usually needs to be applied every second day. This treatment Mr. Hart describes as very successful, and preferable to more heroic methods. The spray douche may be obtained by the use of the double tubes sold by perfumers for showering scented fluids. Many such have been devised. A particularly convenient form has been arranged by Dr. Andrew Clark. Those patients who cannot afford the few shillings must be content with ordinary cold douches applied every half hour for five or ten minutes in succession, a wet rag being applied in the interval.—*Lancet, Jan.* 6, 1866, *p.* 7.

70.—ON BLACK CATARACT.

By Haynes Walton, Esq., Surgeon to the Central London Ophthalmic Hospital.

[This form of cataract is almost unknown to the profession, except by a paper read before the Royal Medical and Chirurgical Society a few years ago, by Mr. Walton. By a naked eye examination no cause of blindness can be discovered; but by the ophthalmoscope it is seen that the lens is of a very deep colour. Whenever the cataract has been extracted in any of the few cases which have been met with, it has always been of a deep brown colour.]

An actual black cataract has been lately removed by Mr. Walton, of which we shall give the particulars, as there are some points of physiological interest connected with it.

A man, 65 years of age, was sent to Mr. Walton by Mr. Philbrick of Colchester, because of his blindness. In both eyes there was the congenital defect, coloboma iridis, or deficiency of a segment of the iris, in a marked degree. His sight was so little affected by it that he worked successfully as a gardener, and his employers never knew that he had any defect.

The coloboma is sometimes accompanied with a split or fissure in the retina. It is impossible to say whether this existed; probably not, because of the quality of the sight.

Except that there were a few minute dots of grey in the pupil of the left eye, nothing could be recognised there, as all else was black. Even when the light of the ophthalmoscope was concentrated on it, nothing more could be seen; but as the interior of the eye could not be illuminated, sufficient light not entering for the purpose, it was evident that there was some black body that intercepted the rays. There was scarcely any sight in the eye, only the mere perception of light from darkness.

Mr. Walton operated by extraction, and made the lower section of the cornea, because the iris was cleft in that direction. The cataract proved to be quite black, and of the usual consistence. It was put aside for a minute

examination, but unfortunately was destroyed. The grey markings spoken of must have been in the capsule, for no such appeared on the cataract.

The case proceeded without a bad symptom, and the cornea healed well, but there was no beneficial result. There is no more sight than existed before the operation. This important question then arises. Are those changes in the interior of the eye, by which the eye is spoiled, and the nature of which cannot be ascertained by the ophthalmoscope when the crystalline lens is lost, associated with the peculiar alteration of the lens? Do they go together as allied diseased states, or is their association accidental?

But a few years ago, every case of loss of sight, in which the pupil looked black, was called amaurosis. It is very likely that many a black cataract was so designated. This shows how necessary it is to use the ophthalmoscope, both for the positive and the negative information we get from it.

In the right eye also there was a cataract. The centre of it was quite black, while a small marginal portion was rather deep brown than black. The interior of the eye could not be illuminated by the ophthalmoscope. There was very little sight. Mr. Walton did not operate; he thought it most probable that the back of the eye was defective, as on the opposite side; and the patient was not at all willing for an operation.—*British Med. Journal, Jan.* 27, 1866, *p.* 91.

71.—ON THE TREATMENT OF STRABISMUS WITHOUT OPERATION.

By C. Holthouse, Esq., Surgeon to the Westminster Hospital and to the Surrey Ophthalmic Hospital, &c.

An essential preliminary to every rational mode of treatment, is a knowledge of the pathology of the affection with which we have to deal, and the more nearly we approach exactitude in this respect, the more likely is our treatment to be rational in its scope and certain in its results; I shall, therefore, premise the discussion of treatment by a few preliminary remarks on the pathology of strabismus, and the class of cases which are best adapted for tentative efforts at a cure without operation.

For practical purposes nearly every case of strabismus may be grouped under one or other of the two following divisions: 1. The nervous or paralytic. 2. The muscular, or non-paralytic; the loss of association in the movements of the eyes depending, in the first group, on a loss of innervation or paralysis of one of the ocular muscles; whilst in the second, the innervation is perfect, or may be in excess, but one or other of the muscles has undergone some functional derangement, or structural change. Of the nervous or paralytic strabismus, we are too little acquainted with the changes which take place in the nervous elements as a consequence of disease, to be able to say, in all cases, how the suspension of their functions is brought about; but reasoning from analogy, and from what we know of the causes which give rise to more general paralysis, as hemiplegia, we may assume that pressure plays a not unimportant part; and this may arise either from over distention or congestion of the bloodvessels, from extravasation of blood or serum, from inflammatory effusions, or from tumours, whether osseous, neuromatous, vascular, or of any other nature; and the action of these may be either on the brain, or on the nerves issuing from it in any part of their course. We have, therefore, to consider in the first place, whether the paralysis has a cerebral or a nervous origin; and, in the latter case, whether it is within the cranium proper or within the orbit; thus we may not unfrequently arrive at a correct estimate both of the locality and the nature of the mischief.

The larger proportion of cases of paralytic strabismus which fall under the notice of the ophthalmic surgeon are not of cerebral origin, or at least there are no symptoms to indicate any such lesion, while of the nervous cases, rheumatism and syphilis furnish the most abundant examples. The following

case, though narrated in a former work (*On Squinting, Paralytic Affections of the Eye, and certain Forms of Impaired Vision*) is worth republication from the bearing which it has on the foregoing remarks.

Case of Paralytic Convergent Strabismus, of Itracranial and Nervous Origin, from Syphilitic Periostitis of the Basis Cranii.—James P., aged 30, a compositor, applied to me at the Public Dispensary, Lincoln's Inn, March 6, 1847, with the following symptoms. He had a convergent squint of the left eye and was unable to abduct it, or even to bring the cornea into a central position; he could, however, rotate it further inwards, as well upwards and downwards. The size and movements of the pupil were normal, and the vision of each eye, when examined separately, was good, though when both were open the patient experienced much confusion of vision. When told to put out his tongue, it was observed that the point deviated to the left, and that the two sides of the organ presented a remarkable contrast, the left half being atrophied, broad, flat, thin, and flabby, whilst the right was firm, plump, and roundish. The paralysis of half the tongue was attended with considerable inconvenience in mastication; but produced no appreciable effect on the patient's articulation. Sensation and taste on the paralysed side were perfect. The patient complained of constant pain in his head and at the back of his neck. The above symptoms first made their appearance in July of the preceding year, and had hitherto resisted the treatment adopted. When he applied to me, I recognised him as a patient who had been under my care some months previously to the present attack, for syphilitic periostitis of both tibiæ, and coupling this fact with his present symptoms, I concluded that they arose from a similar disease affecting a portion of the basilar process of the occipital bone or its perio-teum, and implicating the sixth and ninth nerves at their origin.

Under the influence of antisyphilitic treatment this patient was quickly relieved of the pain in his head and neck, and slowly recovered power over the external rectus. The paralysis of the left half of the tongue was more slow in disappearing, so that on July 12th, 1847, when he again consulted me for a slight return of the head symptoms, the tongue had undergone no alteration, but the strabismus was less marked; he was able to move the eye slightly beyond the central position, and to maintain it in the centre of the orbit, though of course the sound eye then became inverted. Six years afterwards, when I again saw this patient, all traces of the strabismus had disappeared, and a scarcely appreciable difference could be discerned between the two sides of the tongue.

This case is a good example of complete recovery of the external rectus of one eye from a paralysis of many months' duration, and which led to no shortening of its antagonist, or to any implication of the adductor muscle of the opposite side. Both of these results may follow a paralysis; so that when the latter is cured, either of them, as pointed out by Von Gräfe, may lead to an ordinary concomitant squint.—*British Med. Journal, Feb.* 17, 1866, *p.* 173.

72.—ON ATROPINE POINTS.

By J. Z. Laurence, Esq.

The use of *solutions* of atropine in ophthalmic practice is attended with certain practical inconveniences. Mr. Streatfield proposed to substitute disks of thin paper or gelatine, steeped in solutions of atropine. A disk is placed on the eye; the tears then dissolve out a sufficient quantity of the atropine to dilate the pupil. These *dry* preparations recommend themselves by their simplicity and portability.

I have lately had made for me by Mr. Curtis, of Baker Street, pencils or points impregnated with atropine. These atropine points are, like nitrate of silver ones, adaptable into wooden holders, and may be carried about in the waistcoat pocket. If the atropine point is lightly passed, once or twice, over the conjunctiva of the lower eyelid, a sufficient quantity of atropine is

washed off to dilate the pupil for ophthalmoscopic or other purposes.—*Ophthalmic Review, Jan.*, 1866, *p.* 426.

73.—BLUE GLASSES.

Nothing strikes the visitor more, not at the Clinique only, but in Berlin generally, than the extraordinary number of persons wearing blue glasses. These, mostly plane, and ordered for the purpose of protection from the yellow rays of light, are now frequently preferred of the globular shape. The neutral tint or smoke glass is seldom seen. The sunlight, throughout the year, is more glaring abroad than we have it here. Moreover, there is at Berlin, besides Von Graefe, an authority perhaps greater than he in this particular, favouring the resort to blue glasses. Professor Böhm, who has made the merits and effect of the blue light his special study, and to whom practical ophthalmology is certainly indebted for valuable information on this head. Whoever has cared to enquire into, and in suitable cases try, the effect of blue glasses, will not, on account of a possible excess due to fashion, too unfavourably judge the ascendency obtained by this therapeutical agent.—*British Med. Journal, March* 24, 1866, *p.* 308.

74.—TREATMENT OF MALIGNANT AND RECURRENT TUMOURS OF THE ORBIT.

During the last few weeks we have had under notice a somewhat novel treatment for the more complete eradication of these obstinate and distressing affections; and, as far as we are able at present to form an opinion, the results seem most promising. The treatment appears to be applicable, not only to the malignant growths within the orbit, but also to those forms of recurrent fibroid tumour which spring from the periosteum of the orbital bones, and recur almost as rapidly as they are excised.

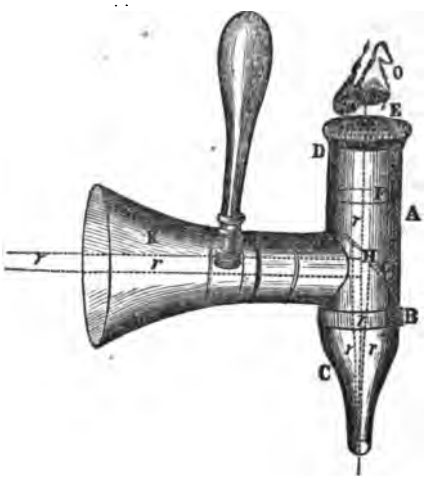
The first case was under the care of Mr. De Morgan, the second under Mr. Hulke, both of the *Middlesex Hospital*, and the third under Mr. Geo. Lawson, of the *Royal London Ophthalmic Hospital, Moorfields*.

The treatment consists in rapidly excising the whole of the growth from within the orbit, removing as far as practicable all the tissues within the bony cavity as far back as the optic foramen, and then freely applying the chloride of zinc paste spread on strips of lint to the bottom and sides of the orbit, so as to effectually destroy any germ of the disease which may have escaped the knife. In two of the cases, the actual cautery was used to the periosteum lining the orbital walls previous to the application of the chloride of zinc, as there still remained small adherent particles of the disease, which it was difficult to get away with the knife. No untoward symptoms followed in either of the three cases; the pain was not more severe than after an ordinary cutting operation; and each of the patients made a rapid recovery. In the first case, more than twelve months have elapsed since the disease was removed; in the second, more than six months; and in neither have any recurrent symptoms appeared. The last case is still under treatment, the operation having been performed only about three weeks ago. Each of these three patients might have been fairly considered most unfavourable for any surgical treatment, as in two the disease had been twice removed and had returned, and in the third the tumour had attained a size which it seemed almost rash to attempt to meddle with. We shall at some future period take an opportunity of reporting these cases in detail, when more time has elapsed to justify a positive conclusion as to the beneficial results of this mode of dealing with these terrible affections.—*Lancet, Jan.* 6, 1866, *p.* 7.

75.—A NEW OTOSCOPE OR SPECULUM AURIS.

By Dr. John Brunton, M. A.

In the spring of 1861, while examining a patient's ears with the ordinary aural instruments, two serious difficulties arose to me in forming a correct diagnosis with such instruments—viz: 1. That the observer's head very greatly obstructed the light; 2. That the eye could not get near enough the object to permit of minute examination, and more so if *sunlight* instead



of *artificial* light was used. The instrument of which a description and drawing are here given then suggested itself to me.

The instrument consists of a brass tube (A), two inches long and three-quarters of an inch in diameter, to one end of which (B) is made to fit on, by sliding, an ear-piece similar to Toynbee's aural speculum. At the other end (D) is an eye-piece (E), with a lens (F) of moderately magnifying power; the eye-piece slides so as to admit of focal arrangement to suit the eye of the observer. In the body of the instrument, near the ear end, and set at an angle of 45°, is a concave mirror (G), with a hole in the centre (H); this aperture in the mirror is in the line of the axis of the tube and ear-piece. At a right angle to the body of the instrument, and opposite to the mirror, is adapted a sliding funnel-shaped polished silver reflector (K) for collecting and concentrating the rays of light, so that the rays (*rr*) are let in at the side, and, falling on the mirror (H), are reflected and concentrated into the ear, and carried back (*rr*) to the eye of the observer (O) through (H), the hole in the mirror, and are magnified by the lens of the eye-piece (E, F). There is also a handle attached to the light-reflector, which works on a sliding band, and can be turned about to suit either hand of the observer as he may wish, when looking at the right or left ear.

With this instrument the observer's eye is brought in close contact with the patient's ear, and the light coming in at the side is in no way obstructed. The otoscope is easily used, and with the best advantage by sunlight. It is applied thus: With the left or right hand, as the case may be, the surgeon pulls directly outwards the auricle, inserting the ear-piece into the external meatus. He then desires the patient to turn his head round till the light falls directly upon the mirror, when the meatus, membrane of the tympanum, &c., will be observed very clearly, the smallest change of structure may be noted, and the movement of the membrane readily seen. If the membrane and ossicles are gone, the tympanic cavity can be observed and noted. With

this instrument the blood-vessels that traverse the membrane of tympanum can be seen in the healthy ear.

The advantages of the instrument are—

1. Simplicity of construction.
2. Ease of application, a few trials sufficing to make the observer expert.
3. Precision and minuteness with which the ear can be examined.
4. That it can be used with artificial or sun light (the latter preferred).
5. That it can be used with the magnifying power or not, at pleasure.

The instrument was made for me by Mr. White, of Renfield-street, Glasgow, optical and mathematical instrument maker to the University. It was shown some time ago to the Medical Society of London. It is used in Glasgow.

The principle of the instrument can be adapted in many ways. I have used the otoscope with advantage in examining the nasal passages.—*Lancet, Dec.* 2, 1865, *p.* 617.

76.—ON A NEW METHOD OF APPLYING REMEDIAL AGENTS TO THE CAVITY OF THE TYMPANUM.

By Dr. Edward Bishop, Surgeon to the Metropolitan Infirmary for Diseases of the Ear.

[One of the most valuable practical teachings of modern surgery is the recognition of the fact of how much may be done within the cavities of the body. Great care in manipulation is necessary in applying anything to so delicate and sensitive a part as the tympanic cavity.]

The method I am adopting at the hospital, is to apply lotion containing such soluble remedial agent as may be considered appropriate, by means of tepid pulverised water. It is evident, as far as the *principle* is concerned, lotion of any strength, up to the point of saturation, may be used. The quantity required to come into contact with the whole lining membrane is so small that there is not the least fear of mechanical injury, and by slightly turning the point of the instrument, the pharynx, the nasal passages, and the parts connected therewith, may all be subjected to the action of the remedy—an important fact, as it is found that the whole tract of mucous membrane lining these parts is generally suffering from the same morbid condition. As the pulverised lotion is driven off in the finest state of subdivision, it may be sent into the cavity of the larynx; but on this point I have not yet had much experience, and therefore, refer to it with diffidence. In the treatment of ozœna, however, it will be found useful, as a solution of such agents as carbolic acid or creasote may be sent into every crevice and cranny of the tortuous nasal passages and the parts connected with them.

The apparatus consists of a small graduated glass syringe similar to that used for subcutaneous injection. The solution is gently forced guttatim into a cylinder, and at the point where it leaves the nozzle of the syringe it is caught by a current of air sent by a pump worked with a proper degree of force by the hand. This drives the fluid forward in a pulverised state. This small apparatus is then attached to an ordinary Eustachian catheter previously introduced, and suspended by a suitable apparatus. To ensure complete pulverisation, the end of the catheter is covered by fine gauze wire.

Some amount of dexterity is required in the successful use of the instrument, but this is soon accomplished by any one accustomed to the introduction of the catheter.

It is equally applicable to those cases where it is thought desirable to use Politzer's catheter, introduced a short distance only within the nasal passage—the pulverised solution finding its way through the Eustachian tube into the cavity of the tympanum during the act of swallowing.

The instrument described above has been made for me by Weiss and Son, Strand, London. I am conscious it is capable of much improvement, and in the hands of those highly scientific gentlemen has already been put into a *more* practical form than I first anticipated.—*Med. Press and Circular, Jan.* 3, 1866, *p.* 9.

77.—ON DIFFICULTY OF HEARING IN CASES OF PERFORATION OF THE MEMBRANA TYMPANI.

By Dr. F. E. Weber, Berlin.

In order to formulate my statements let us imagine—

1. A person with a large perforation in the membrana tympani, not of long standing, which has entirely passed off, without thickening of the mucous membrane lining the tympanum; one, further, whose ossicula are not yet injured, and the rest of whose acoustic organs are intact. But let us suppose the tube to be closed, proving on examination to be completely obstructed by catarrh, and incapable of being cleared either by the experimentum Valsalvæ or by the air-douche aided by a swallowing movement, so that the probe cannot pass the stricture without resistance. Let us imagine further, that a watch which under ordinary circumstances can be heard at a distance of thirty feet, is in this case not audible by the patient beyond a range of two feet, vocal utterances being equally inaudible. Now let us suppose a few days to have elapsed, and the inflation produced by the catarrh to have somewhat subsided, so that the tube on examination, though still charged, seems already somewhat less swollen, proving on the attempt to swallow to be uniformly filled with mucus, the range of hearing, being, say three feet. If now the entire bore of the tube—the cavity of the tympanum having previously been found quite free and clear—be at this moment opened by means of a bougie of suitable size, which is allowed to remain a few minutes, and an air-douche be then passed through, so that the noise of the air passing through the mucus is distinctly heard by aid of the otoscope, the patient, after this opening of the passage, will hear the watch at a distance of from twenty-five to thirty feet.

2. Now let us take another case in which, along with the defect in the membrana tympani, further derangement has resulted from chronic inflammation of the middle ear, such as sclerosis of the mucous membrane, or thickening of the coatings of the fenestræ, or it may be even that, through a slowly-progressing process (antecedent perhaps to the inflammation of the cavity and the rupture of the membrane) secondary changes have passed over to the auditory nerves. Supposing such a patient, while the tube remains closed, to hear the watch at a distance varying, according to circumstances, from about half an *inch* to a few inches, he will, after it has been opened, perceive the ticking at a distance of one *foot*, or at most of a few feet.

These results, so far as regards the general drift of these remarks, occur with such marked uniformity, that hitherto, amongst some hundreds of cases carefully examined in reference to the point in question, I have only found exceptions in two instances of highly tuberculous individuals.

2nd. A further highly important and interesting observation stands in close connexion with what precedes—namely, that in these cases of subsided chronic inflammation in the cavity of the tympanum with perforated membrane, the opening and restoration to a normal condition of the passage of the tuba Eustachii produces in most instances an equal improvement in the power of hearing—the effect being in few cases less, more frequently greater, to that which ensues on the application of the so-called artificial ear-drum.

Definitive observations on the nature of this coincidence I have as yet not made, still (seeing that the application of the artificial ear-drum is relatively but seldom effectual, whilst the opening of the tube is a remedy which nearly always affords relief) the agreement between the results which ensue on the employment of the several remedies will be found mainly where there is a wider range of hearing, whilst in extreme cases of difficulty of hearing, arising in the manner referred to, the remedies cease to be equal in their effects, or even to produce uniformly, when combined, the sum of their separate effects.

I have endeavoured to arrive at an explanation of this striking fact; but even from the most distinguished physiologists who have occupied themselves with investigations of the organs of hearing, with whom I have entered into correspondence on this subject—namely, Professor Helmholz, of Heidelberg,

and Professor Henle, of Göttingen, I have failed to receive such an explanation; nor have I been able to discover the cause why the Eustachian tube should prove to be of such eminent importance where the membrane of the tympanum is wanting, as used *only* to be assigned to it where that organ is preserved.

What I surmise in reference to these striking phenomena—though my views require for their confirmation the collection of yet further experiments and observations—is this, that the tube acts in these cases in the manner of a sounding-board, or rather as a resonant pipe, in connexion with the cavity of the mouth. This idea, founded on investigations and experiments which are reported in the celebrated Joh. Müller's Physiology, I have also communicated to Professor Henle. The reply I received was, that he could no longer think of advocating this opinion, since he had learnt from dissections that the parietes of the Eustachian tube, from the isthmus down to the orifice in the throat, ordinarily (except during the act of swallowing) lie flat together, so that there can hardly be any question of resonance or resonant connexion with the cavity of the mouth; we might, he adds, rather explain the phenomena in question by supposing that, in the particular cases referred to, the tube attains a physiological importance which it does not ordinarily possess.

I could never share this view, because the state of the organ in the dead body by no means proves that the same applies also during life, when the vital tension of the parts of the organ and the various degrees of pressure to which they are subjected alter the mutual relations of the parts.

I have been accordingly most agreeably surprised in this respect by the results of some quite recent labours in reference to the Eustachian tube, published by the prosector of the University of Munich, Dr. Rüdiger, which prove that *even in the dead body*—contrary to the received opinion—the smaller upper part of the bore of the tube is always open through its entire length, and that therefore there is an unbroken column of air in communication with the organs of respiration. One chief objection to my explanation is thus removed.

The effect of the artificial ear-drum has also been explained by some on the principle of resonance; whilst others think it must be attributed to a pressure which the artificial drum (of Toynbee) or the pellet of wadding (of Yearsley) exerts upon the remaining fragments of the tympanic membrane, or of the ossicula auditus, and thereby mediately upon the labyrinth.

Much may be said in favour of each of these explanations, and much also against their exclusive adoption. Thus, for instance, the artificial drum proves effectual in cases where there can be no question at all of the pressure above alluded to; whilst, on the other hand, the phenomena of resonance in most cases predominate greatly; and during the *application* of the little instrument in question, patients chiefly complain that the sound hums and *reverberates* too much, and that every noise is heard as if proceeding from a hollow cask.

Well, then, this resonance theory, which, though it may be open to objections, is alone applicable in both cases, explains why the two remedies, differing so widely in their physiological and therapeutic effects, produce nevertheless interferences which frequently coincide.

Now, without dwelling further on the manner in which this conception of the mode of operation of the artificial drum harmonizes with the account which Toynbee so ingeniously gives of the normal membrana tympani (according to which the drum with its ossicula, like the iris in the eye, must be regarded only as a kind of adjusting apparatus for the correct perception of the highest as well as the lowest sounds), I am of the opinion that it is reserved for further investigations to arrive at a correct solution of the problem.

3rd. Meanwhile, the physiological fact already established leads to a still more important practical application in therapeutics. Whilst I acknowledge how, to the praise of its inventor, the so-called artificial drum produces an almost magical effect, yet every one familiar with the subject will admit that

in the majority of cases, often quite contrary to expectation, the magnificent effect fails to ensue on the application of that little instrument; that its use is, under any circumstances, for many patients very disagreeable, and in unskilful hands affords a doubtful relief—nay, is even prejudicial; that it is only when the patient or the physician *accidentally* hits upon "a certain" right spot that improvement in hearing ensues; that all sounds and noises are heard frequently with disagreeably loud reverberations and hummings; and that, especially in the hands of not very cautious laymen, its constant application for a series of years always, as an irritant, as a foreign body, operates prejudicially upon the exposed mucous membrane of the tympanic cavity and the edges of the membrane, and thus reproduces and keeps up the state of suppuration. I accordingly only employ it as an occasional resource, whilst habitually the *methodical* treatment of the tube, with the object of *restoring its passage to as normal a condition as possible*, by means of bougies (often combined with injections of astringents and nitrate of silver, and with treatment of the mucous membrane of the tympanic cavity and the throat) effects *everything*, often more than any other means, and *always more harmlessly*, and of course affords a more constant uninterrupted relief than can be rendered, in the majority of cases where the tympanic membrane is ruptured, for the merely *temporary* heightening of the power of hearing, by means of the so-called artificial drum.—*Lancet, Feb.* 17, 1866, *p.* 173.

SYPHILITIC AFFECTIONS.

78.—ON SYPHILITIC INOCULATION IN 1865.

By Henry Lee, Esq., Surgeon to St. George's Hospital.

Much has been said and written as to the identity or plurality of venereal poisons. Observers at one on the different nature and properties of the soft and indurated sores are at variance as to the identity or duality of their cause. To different minds the same array of facts present a different view, and carry opposite conclusions. The pathologist, in his study of morbid causes, encounters the same difficulties that divides the schools of naturalists —for instance, the Darwinians and anti-Darwinians.

It is foreign to the purpose of this lecture to enter into the question of the unicity or duality of the syphilitic poison; but it is important to remember that the natural and artificial inoculation of the skin or mucous membrane of a healthy individual with a virus of what we will term true syphilis produces a characteristic affection locally and constitutionally, and does not induce a local, soft, suppurating sore with any concomitant suppuration of the neighbouring glands. The apparent exceptions to this rule are so very rare that we may fairly question their reality. Dr. Danielssen's experience of the inoculation of lepers afforded confirmation to his mind of the long-established axiom—that the soft chancroid does not affect the system, and consequently does not produce general syphilis, for among the many thousand inoculations he had witnessed he only noticed *one* exception, and this exception was afterwards perfectly explained by the discovery that after nearly four hundred inoculations of a leper with the virus of the soft sore, without producing constitutional manifestations, an indurated chancre, followed by unmistakeable signs of secondary syphilis was the result of the accidental inoculation of the same leper with secretions obtained from an indurated chancre. Again, the same body of men—of soldiers, say—have before now contracted the local soft chancre at one station, and the indurated one with constitutional syphilis at another station.

What we find true of the many we discover likewise in individual cases The same patient may have suffered from two, three, or six attacks of the soft sore without secondary infection; but when he has contracted the indurated chancre then secondary infection has manifested itself. The modern history

of syphilis points the same way. The widespread outbreak of syphilis at Rivalta and the inoculations performed from time to time with the view of proving the possibility of inoculation with the products of constitutional lesions were, in all cases, preceded by an affection *corresponding to the descriptions* of the *indurated sore and glands, and not to that of the soft sore*. But, it will be asked, what are the characters on which we rely for our distinction of the two affections? Shortly these :—

The simple, soft, local venereal ulcer is marked by a process of active ulceration and suppuration, attended with inflammatory phenomena. Neither the ulcer nor its cicatrix presents any well-defined and strictly limited induration; it discharges a purulent secretion which readily inoculates the neighbouring parts, and the ulcers are often multiple or become so. If inguinal adenitis ensues, it is confined to one gland, which tends to inflame and suppurate. The virus of this sore can be *invariably* inoculated on the same subject *through several generations*, always producing effects *essentially like the original ulcer*. The disease manifests itself two, three, four, or rarely five days after exposure to contagion; one attack conferring no immunity whatever against subsequent ones.

The indurating, infecting, or true syphilitic chancre, is characterized by the adhesive form of inflammation; it is more or less ulcerative in character, but not by any means invariably so, the specific induration being sometimes formed *à froid* without inflammatory phenomena. This sore is generally solitary, or if multiple, it is so from the first, the base and periphery of the lesion possessing a well-defined but variable amount of induration. The concomitant adenitis is generally symmetrical, consisting of a chain of enlarged indurated glands, or indurated lymphatic vessels, and without any active inflammatory or suppurative action in them. The lesion appears from between five days to five weeks after contagion. The discharge from the indurated chancre is not auto-inoculable, except under certain conditions of *attendant irritation*; and then the inoculation never produces another indurated ulcer, but a postulation, with some erosion or trifling ulceration at the point inoculated. The indurated chancre is protective against subsequent attacks, conferring a *relative*, although not an *absolute*, immunity for the future. Constitutional syphilis no more frequently repeats itself in the same subject than other diseases depending on animal poisons, such as the contagious exanthemata.

I have purposely described the most typical and common form of these diseases, as illustrated by the cases just given, and avoided the more anomalous, mixed, and non-comforable cases, in order to fix the attention upon the points of contrast between these two forms of venereal lesion.

An examination of the edges and base of a venereal ulcer will generally indicate its character. In the local sore these are *dis*-continuous; the vertical edge, as it joins the base, is a little undermined. You can move the former laterally a little on the latter, and a little pus will ooze out. The base is also cellular, honeycombed-looking; and on raising the ulcer between the finger and thumb you will find it quite soft, or doughy it may be. In the other form it is quite different. The edges slope inwards to the base, and join it. In other cases the chancre is a flattened induration, or an enlarged papule, covered, perhaps, with an adherent scale. The indurated ulcer on the prepuce rolls over *en masse* as the part is retracted; the soft one bends on itself. Again, a shining, gummy exudation covers the dull-red surface of an indurated sore at first, and in time this surface becomes dotted with little points of molecular disintegration; while the local soft lesion is either a pustule at first, or an erosion, which speedily becomes an ulcer, painful, and discharging pus freely on pressure.

The character of a chancre at one time need not, of course, be its character at another; nor is the absence of induration a proof of its local nature. Before deciding on that, it would be necessary to examine into the attendant signs, the state of lymphatics and glands; but when a specific induration can be detected in the chancre, with a similar state of the neighbouring glands, the *positive* evidence becomes so reliable *that you may safely and certainly prophesy the subsequent evolution of a syphilitic manifestation in the subject of it.*

We now approach the subject of *inoculation*. It is well known that the virus of the soft sore is not only auto-inoculable, but the inoculations may be propagated through several generations. The secretion of the soft sore is invariably that of pus, which forms the vehicle of the virus. The ordinary secretions from the indurated sore are not inoculable in the same individual. Originally Ricord maintained that a person could only have constitutional syphilis once; that the diathesis did not double itself in the same constitution.

In 1853, I showed that the indurated infection sore, in its origin, was attended with the adhesive form of inflammation, and was therefore an essentially different morbid process from the soft, local, suppurating sore. In 1856, I asserted the non-inoculability of the indurated chancre upon the patient himself, supporting the doctrine by the publication of the cases in which I have made this the subject of experiment; and subsequently in 1858, in continuation of these observations, I pointed out the exceptional circumstances under which such inoculations might succeed.

The correctness of these observations received abundant confirmation at the hands of a large number of Continental and British surgeons. The latest writer on the subject of venereal disease, Dr. Hammond, a man thoroughly accustomed to scientific observation, speaking to his pupils, says: "I have, as you know, endeavoured very many times to inoculate the virus of an infecting chancre on a syphilitic person, and never once with an affirmative result.... ....Try it for yourselves—and most of you have already done so—if you entertain any doubt, and you will be satisfied."

On the other hand, Drs. Boeck and Bidenkap assert that they not only employ, but prefer, the secretion of the hard sore, as more effective than that of the soft in the process of syphilization.

The experiments on the two sides have been numerous: those of the first set of observers have been indeed so numerous that the question was considered a settled one until the method of treating constitutional syphilis by "syphilization" was adopted. Dr. Marston tells me that the result of a great many artificial inoculations performed in the Military Hospital has been, that they have *never* failed in getting an affirmative result with the virus of the soft sore; while they have *generally* failed with that of the hard in producing any effect upon the same individual. In one instance lately he employed the purulent secretions from the two forms of sores at the same time upon the same patient. He was careful to use different lancets, *because he holds it to be essential, when dealing with an agent possessed of such peculiarly penetrating and irritating properties as the virus of the soft sore*, to guard against the presence of even the minutest quantity of it. He succeeded in propagating a lineal series of ulcers, which left large cicatrices on healing, from the pus of the soft: but only two generations of pustules with that obtained from the hard sore.

Assuming the statements of Drs. Boeck and Bidenkap to be perfectly correct as to the inoculability of the hard sore in a syphilitic soil, we are brought face to face with the question, How can the differences between the results obtained on the continent, in England, America, and Italy, be reconciled with those obtained in Norway? Two answers might be urged to this question.

First, that the virus was not in reality that of the infecting sore at all, although obtained from the seat of an indurated chancre.

Secondly, that in the other case the suppurative action superinduced upon an induration by causes of irritation gives rise to a secretion possessing a morbid potency—an irritating contagious property—belonging to, and the product of, this artificially induced action, as distinguished from that belonging to and set up by the original syphilitic virus. In illustration of these positions I shall endeavour, as far as possible, to avoid all the theoretical explanations of the phenomena described.

In practice we occasionally meet with examples of infecting sores which are non-conformable to the description given of the typical indurated sore, such as the following:—

1. An initial lesion consisting of a vesicle, which becomes a pustule, then

an ulcer like a soft ulcer, but which acquires a specific induration at some stage of its progress. Or,

2. A patient presents himself with one, or perhaps many, small ulcers possessing the characters of the local soft sore for some time, but before cicatrization a hardness developes itself at the seat of one of these chancres, the ulcerative action and secretion of pus continuing.

A close inquiry as to the exact date of the appearance of these forms of venereal lesion will generally bring one very suggestive fact to light—namely, that the first symptoms in these cases follow very shortly after exposure to contagion; i.e., within a few days. Never, within my own knowledge, has the interval been long between the date of the last sexual intercourse and the first appearance of the lesion.

The exact pathology of these chancres has yet to be worked out to the satisfaction of the medical world. One set of observers regard them as the strongest evidence of the identity of the soft and hard sore; another set think they see the characters of two different morbid processes evolved in the same seat, and regard them as the exponent and proof of a twofold inoculation—two seeds sown at the same place and time. With this we have at present nothing to do. The practical, the important fact in their history, to my mind, is this: that we can produce a lineal series of inoculations from these lesions, notwithstanding they are the precursors of general infection, and the effects of the inoculations are yet strictly analogous to those induced by the virus of the soft chancre. A reference to the contribution of Dr. Marston in the forty-fifth volume of the "Transactions of the Medico-Chirurgical Society" will supply observations corroborative of this statement.

The lesion which begins as a pimple; which becomes an induration; which is non-ulcerative in character for some time, but covered with a gummy sero-epithelial secretion; which takes on a molecular disintegration at the surface; which remains an indolent induration, until that induration is dissipated by a process of absorption, instead of being disintegrated by one of ulceration,—is the most common, the most typical form of infecting chancre; and the secretions from it are *not auto-inoculable*, unless we first of all induce suppuration on its surface. And this leads me to the second method of solving the practical difficulty.

Inoculation, in its more extended sense, implies the production of a disease in a healthy person, or in healthy parts, by the insertion or contact of some morbid secretions; and the effects may be simple or specific in character.

An eczematous eruption may be occasionally induced on the healthy skin by the contact of fluids from a neighbouring eruption; the secretion from a wound on an extremity may poison, inoculate, the lymphatics—by intravasation of a pus-globule, for instance, within a lymphatic tube,—and may produce an abscess at the nearest gland, with or without pustulation of the skin, corresponding to the seat of the obstructed lymphatic; the contact of pus from a balanitis or urethritis with another mucous membrane may give rise to pustular disease; and this mere insertion of ordinary pus into the integument with the point of a lancet will sometimes be followed by an inflammatory reaction. But the experiments of Dr. Van Roosbroeck, Dr. Piringer, and Sir Patrick M'Gregor, prove that a conjunctivitis, of different degrees of intensity, may be induced by the inoculation of purulent fluids obtained from various sources. Mr. Simon in his essay on Inflammation (Holmes's "System of Surgery," vol. i.), remarks—"There is ample room to question the popular impression that only specific inflammations are communicable, and much reason for suspecting it, on the contrary, to be a generic and essential property of inflammation, that its actions (or some of them) are always, in their kind, to some extent contagious."

It is, in part, generally recognised by our profession that many diseases, such as skin affections and inflammations of mucous membranes, may develope contagious properties by undergoing changes of a dynamical character—that is to say, by affording purulent discharges.

There is, however, a great difference between these examples and those

truly specific effects obtained by the inoculation of variola, vaccinia, glanders, and syphilis.

If we inoculate a healthy person with the fluids obtained from an indurated chancre or a constitutional syphilitic lesion, the product is an enlarged papule, induration, or hard chancre at the point inoculated, if the result be an affirmative one at all; and this effect is not developed in two or three days, but only commences after an interval more or less considerable. No one, as far as I know, has ever succeeded in propagating from the pus of an indurated chancre another, much less a series of indurations, in an individual labouring under constitutional manifestations of syphilis. The inoculations, if successful, are developed as vesicles and pustules about the third day, and the ulceration, if any, is not surrounded by induration. Yet this hardness is unquestionably the best, the only reliable or positive anatomical sign of the infecting character of the lesion. The individual having contracted the disease in the natural way, and once exhibited on his person the anatomical sign of the specific action of the virus, cannot be again subjected to it.

The evidence obtained, therefore, from the results arising from the auto-inoculation of pus from a hard sore *affords no conclusive proof whatever that those results depend directly upon the syphilitic virus which caused the original indurated chancre.* But it has been urged, that when you can obtain a lineal series,—a successive generation of pustules and sores,—it proves the existence of some special agent in the secretion inoculated over and above any of those simple effects to which ordinary pus might give rise. And unquestionably it is so, for we hold this very property inherent in the pus of the local soft sore as the proof of its being the vehicle of a specific virus.

With those varieties of the hard sore which I first mentioned, the results obtained by auto-inoculation are specific; but then these results are such as you get from using the virus of the local variety of sore, and there appears to be nothing illogical in ascribing them to that virus—in fact, to the virus of the local ulcer superimposed upon the infecting form of the disease.

I have applied the pus from a soft sore upon the surface of an indurated one, and have succeeded in producing an ulceration upon that induration exactly such as would have been obtained elsewhere, and the ulcer was certainly exactly parallel to those non-conformable chancres now described. Any matter thus put on an indurated sore, or mixed with the secretion of that sore, and again inoculated, would produce the same effect as if inoculated by itself in a healthy part of the body. Its nature is not changed by its being put on a hard sore and taken off again.

Further investigations into the dynamical change by which an inoculable secretion is developed are doubtless required to throw light upon the obscurity which yet hangs about it; but I am tempted to introduce some observations from Bidenkap which appear to bear upon the subject. This author, after showing that an indurated sore may afford an inoculable pus after the application of irritants, gives some illustrations of the effects of inoculating such artificially-produced pus in healthy persons. Three girls in his hospital inoculated themselves with matter obtained from the artificial sore produced on infecting ones. The first patient had a history of constitutional syphilis, and the inoculation produced in her a pustule without leaving an ulcer. The second had never had syphilis. She produced eighteen ulcers by her own act; and Bidenkap produced twelve more by re-inoculation from them. No symptoms of constitutional syphilis followed in her case during the six months that she remained under observation. The third was suffering from gonorrhœa, but had never had syphilis. She produced a large ulcer by inoculation. No constitutional symptoms followed, but a year and a half afterwards she contracted in the natural way an infecting chancre, and subsequently had constitutional syphilis. The description given of these cases undoubtedly brings them under the category of local soft sores.

If the syphilitic poison itself was the cause of these effects, then, that virus gave rise to this local soft variety of chancre without being preceded or accompanied by any solid elevation of the skin, a circumstance in itself

extremely unusual; but if we suppose that the effects might have been due to the *irritative quality of the matter inoculated*, which quality might have been acquired through that very artificial process of ulcerative inflammation by which the purulent secretion from the infecting chancre was produced, then these exceptional results may be referred to another action than that of the syphilitic poison.

In some cases, the two actions from the same inoculation may be distinctly and separately traced. In an experiment cited by Vidal, for instance, the secretion from a syphilitic pustule, was inserted into a gentleman's hand and produced a pustule, which lasted fifteen days and then subsided. No further action took place until the thirty-fifth day, when the pustule broke out afresh and constitutional symptoms appeared, one hundred and twenty-eight days after the inoculation. In such a case the distinct actions must be allowed, whether they be both attributable to syphilis or not.

It is scarcely justifiable to repeat the dangerous experiment of inoculating another and healthy individual with fluid obtained from a pustule artificially induced upon a specific induration; but it has been done, and, what is more, it is positively asserted that the local affection only is produced by this plan, provided you are careful to take only the fluid from the pustule unmixed with any blood. The positive evidence, if substantiated, appears to be conclusive of the fact that you may not only have the products of a twofold inoculation at the same part, but that you may even succeed in propagating them separately. *Lancet, March* 31, 1866, *p.* 336.

79.—ON THE TREATMENT OF SYPHILIS.

It has been asserted that the best mode of treating syphilis is by the repeated inoculation of the same poison which originally produced the disease. To demonstrate this fact was the object of Dr. Boeck's visit to England. The interest of the profession has in consequence been keenly excited; and it is due to them and to the public that the evidence afforded upon the subject should be fairly given.

1. It has been shown in my two former lectures that the inoculation of the syphilitic poison on a person who has real syphilis is not much more easy than the inoculation of the vaccine poison on a person who has been properly vaccinated. In all the experiments which I have performed, where due precaution has been taken, I have failed to produce a lineal series of inoculations from an uncomplicated indurated sore; and Dr. Boeck, during his residence in England, did not, I believe, succeed in producing such a result in any one instance. Mr. Walter Coulson has, however, had two cases in which, after repeated inoculation, he has obtained well-developed pustules. In one of these, which he was kind enough to allow me to see, I was informed that the patient was inoculated with the matter from a soft chancre before the attempts were made to inoculate him with the secretion of his own indurated sore, and that in some of these attempts at inoculation the same lancet was used to take the secretions from both kinds of sore. The lancet was wiped, but not washed after each attempted inoculation. Some of the secretion from the soft sore may therefore in this case have been left by the lancet on the hard sore, and thence removed so as to produce the supposed inoculation from the indurated chancre. Hence a possible source of error in this case; but in the second instance I could not detect any source of error. From the first of these cases Dr. Boeck continued to inoculate, and he also inoculated from some matter which he had received from Norway. This last-mentioned matter was unlike any that I have ever seen taken directly from an indurated sore. It was in considerable quantity, and presented all the appearance of pus derived from a well-formed abscess. If the matter had been collected from a large number of pustules, such as those produced in Mr. Coulson's cases, it would probably have presented the same appearance.

Now, whether matter of this description be a direct product of true syphilis,

or whether it depend upon some dynamical change in the action of the part, it is essentially of a different nature, and formed by a different action, from any that naturally pertains to an infecting sore. This secretion, whether, theoretically we suppose it to have its origin in syphilis or not, is practically not that which produces syphilis. In the change of action which has taken place during its production it has lost its sting, so far as its power of producing an indurated sore is concerned; and it has also lost, as far as our present evidence goes, its power of infecting a patient's constitution.

On the other hand, it has gained a contagious property which it had not before, and can be inoculated with a rapidity and certainty quite different from that which was possessed by the natural secretion of the part upon which it was elaborated. It is therefore changed, not only in appearance, but in vital power. It is different in its immediate effect and in its subsequent results. It, in fact, possesses all the characters of the secretion of a soft chancre, and none of those characteristic of an indurated sore. Whether therefore, I say, it be derived from an infecting sore originally or not, it must be regarded as resulting from an essentially different action, and as possessing qualities essentially different from the secretion of the infecting chancre. Practically, therefore, the secretion from a pustule and the secretion from an indurated chancre are the results of different affections, and it is evidently incorrect to talk of the one as of the other, although the two should have been produced in the same part, or originated on the same spot.

The very conflicting statement made about the auto-inoculability of the secretions of the indurated or infecting sore leave ample room for the suspension of our judgment, to say the least.

An Italian surgeon has quite recently put the subject of inoculation to the test of observation and experiment, and his results are totally opposed to those obtained by Drs. Boeck and Bidenkap.

Dr. Amilcare Ricordi ("Sull' Irreinoculabilita delle forme di Sifilide;" extracted from the "Annali Universali di Medicina," vol. clxxxv., Fasciculus of January, 1866) has only this year published his paper. His object was to test the truth of the assertions of those syphilographers who declared they employed the purulent secretions from the indurated or true syphilitic sore for the purpose of syphilization, and to determine at the same time the truth, or otherwise, of an hypothesis put forth by M. Langlebert as to the different properties of the serous and purulent fluid of syphilitic lesions.

In syphilization if the indurated chancre, or mucous tubercle, does not yield pus, it is made to do so by slight artificial irritation, and the secretion is said to acquire new and peculiarly active properties, which adapt it for inoculation.

The experiment is an eminently simple one, and Dr. Ricordi instituted a series of observations with pus obtained from thirty-six infecting ulcers, and forty-three times with that obtained from ulcerating mucous patches. The fluids inoculated were manifestly purulent, he says; yet the inoculations were always followed by a negative result. Prof. Pelizzari, in 1865, he states, also made a similar series of experiments, which led to similar conclusions. Occasionally, although very rarely, an inflammatory reaction certainly ensued after the inoculation, but the morbid phenomena were never more than might fairly be ascribed to the traumatic effect of the instrument used, and the slight irritative action of the purulent matters inserted, for the artificial affection always disappeared spontaneously in a few days. How then, asked Dr. Ricordi, are we to reconcile the experience obtained at Christiania with the want of success at Milan and Florence?

I am further tempted to make a short and pertinent extract from Dr. Ricordi's pamphlet. Dr. Gustavo Bargioni communicated to Prof. Pelizzari the results of Bidenkap's experience in Paris. Dr. Bargioni personally learnt from F. Follin and his interne, M. Bouchard, that Bidenkap made between seventy and eighty experiments with the secretions of the indurated sore, but he never succeeded in producing a characteristic soft ulcer. The size of the lancet employed by Bidenkap, and his mode of using it, were open to some objection, as it appeared to MM. Follin and Bouchard, and they requested him to employ a needle such as is used for vaccination. This Bidenkap declined

to do, although the interne had perfectly succeeded with it in inoculating the pus of a soft venereal ulcer.

"A circumstance of some interest was the following :—There was a patient present suffering from a well-marked "mixed sore," so-called. M. Follin raised some objections to the pus of that ulcer being employed. Bidenkap, putting aside the objection, desired to employ the pus of this ulcer, and the result of the inoculation was such as M. Follin had predicted—viz., a characteristic soft ulcer.

Not to follow Dr. Ricordi tediously through his other observations, I will only remark that this observer succeeded in inoculating some of the animals with the pus of a soft venereal ulcer, but he failed entirely to engraft syphilis or any characteristic lesion upon them by inoculation with the virus of the hard chancre.

If an action be superinduced upon a sore which cannot be produced by any chemical or mechanical irritation, it shows that some fresh morbid process has been set in operation, and it would be manifestly unjust to attribute any results produced by inoculation from such a source to the influence of the secretion of the original sore alone. To assume this would be to admit a source of error which would run through the whole series of experiments performed.

Where a source of fallacy may be thus seen to lie at the origin of every series of inoculations, which unless guarded against may readily run through the whole, it is of the highest importance, in a scientific point of view, that the greatest precaution should be used in performing the first inoculation of the series, and especially that a fresh lancet, or a lancet that had been placed in boiling water, should be used. In the practice of Drs. Boeck and Bidenkap this precaution is not usually taken. When in a field of wheat we see here and there some oats, however much we may be assured that wheat only was sown, yet we feel satisfied that, either by accident or by seeds left from a former crop, or in some way or other, each blade of oats had its origin in a seed of the same nature. If allowed to grow together, a mixed seed would be produced. If the seeds from this were again and again sown in a soil in which the wheat would not grow, we might in time get a prolific crop of oats, but we could never suppose that the oats had originally been derived from the wheat.

That in syphilitic inoculation we have to do with at least two distinct actions may be proved, not only by experiment, but also by practical observation.

[It is urged in favour of this plan of treatment that it prevents the internal organs of the body, and the bones, from becoming affected with syphilis, and the relapses under such treatment are extremely rare. On the other hand, Mr. Lee states that he has never seen any severe complication of the deeper structures where the calomel bath has been properly used for primary syphilis.]

If we have reason to be disappointed with the evidence on the subject of the so-called syphilization here, we have, on the other hand, extensive and most accurately kept tables in Dr. Boeck's great work, which furnish abundant proof that the symptoms of constitutional syphilis will disappear under the continued irritation kept up by repeated inoculation. It is urged in favour of this plan of treatment that it prevents the internal organs of the body, and the bones, from becoming affected with syphilis ; and that the relapses after such treatment are comparatively rare. We are not, perhaps in a position to judge of what means are at Professor Boeck's disposal in order to ascertain the number of relapses which take place. But in this country it would certainly be no indication of the number of patients permanently cured if we were to calculate the number who voluntarily returned after a four or six months' course of what must appear to them an ineffectual treatment. With regard to absence, under this plan of treatment, of diseases of internal parts, I can readily believe that the continued irritation on the skin would have the effect of preventing the morbid manifestations elsewhere ; for I have observed the same fact with regard to the use of the calomel vapour bath in primary syphilis. The continued action that this maintains on the skin has apparently

the effect of preventing the deeper structures from becoming affected; at all events, I may state very positively that out of a large number of cases treated both in hospital and private practice, I have seen no severe complication of the deeper structures where the calomel bath has been properly used for primary syphilis. We have no means of instituting a comparison with regard to the efficacy of the treatment by repeated inoculation, and by the calomel vapour bath, respectively, with regard to primary syphilitic sores; because the former plan does not appear to have been tried before the evidence already existed of the patient's constitution having become affected; and if it had been tried, the conclusion, as already explained, would have been most unsatisfactory so long as the distinction between the infecting and the non-infecting form of the disease was not kept in view.

In secondary syphilitic affections we have, however, the means of instituting a comparison between these two methods of treatment. I have carefully gone over ten of the case-books kept at the Lock Hospital during the period that I held the office of surgeon to that institution, at a time when the calomel bath was frequently used. Excluding the cases which left the hospital before they were considered to be cured, on the one hand; and cases complicated by other diseases, not venereal, or which required some operation to be performed, on the other; these ten case-books gave the following results:—

Seventy-eight men were treated for constitutional syphilis by the calomel vapour bath, and discharged as cured during 426 weeks, giving an average of a little more than five weeks and three days for each case.

Eighty-seven women were treated for constitutional syphilis by the calomel vapour bath, and discharged as cured, during 605 weeks, giving for each case an average of something less than seven weeks. Many of these women were received into the asylum and remained under observation for different periods, not exceeding two years. The reason why the period of treatment was longer in women than in men was, that the proportion of cachectic cases in women (many of whom were received in a state of great destitution) was larger than in men.

It is not easy to get patients in this class of life to remain in hospital for primary affections, especially when attended with little local inconvenience, and the number of those who were admitted, and remained until properly discharged, for primary infecting sores, was therefore small.

I find that sixteen men were thus treated by the calomel vapour bath, and dismissed as cured, during a period of eighty-nine weeks, giving an average for each case of five weeks and four days.

Thirty-two women were similarly treated during 158 weeks, giving an average for each case of something less than five weeks.

Taking all the figures together, we have 213 cases treated during 1278 weeks, giving an average for each case of exactly six weeks.

If we compare with these figures the length of time employed in the process of repeated inoculation, we find 252 patients mentioned by Dr. Boeck ("Recherches sur la Syphilis," p. 471) as having been treated during 33,828 days, which gives for each case an average of rather more than nineteen weeks. The result which Professor Boeck gives with regard to the time required for the treatment of this disease with antimonial plasters by "derivation" is still more unfavourable. Small antimonial plasters, in these cases, were placed on different parts of the body, so as to keep up a continued series of small ulcerations. Eighty-five patients were treated for constitutional syphilis in this way, and, on an average, the treatment of each patient occupied a period of twenty-six weeks.

Professor Boeck mentions (p. 463) 537 patients as having been treated by calomel internally administered, each case having occupied, on an average, rather more than eighteen weeks and a half. He also mentions 3200 altogether as having been treated by different proportions of mercury (p. 470), each case, on an average, having required very nearly eighteen weeks. Out of these 3200 cases 108 deaths occurred.

Dr. Boeck gives some elaborate tables, showing the results of other modes

of treating syphilis. The treatment by purgatives and iodide of potash are considered. It would prolong this lecture beyond its necessary limits to consider the conclusion at which Professor Boeck's figures point. Suffice it to say that the results obtained are not favourable with regard to any of the plans enumerated, and in none does the time occupied in the treatment appear to have been anything like so satisfactory as that which the statistics above given show to be required for the treatment of the disease by the calomel vapour bath.

The calomel vapour bath has, moreover, the advantage of being applicable where other means of treatment, including mercury internally administered, have been used, and failed. It may be given where the strength of the patient has been very greatly reduced, provided there be no organic disease of internal organs.

I must now say something about the mode of the administration of the calomel vapour bath, especially with regard to one or two points which I have reason to believe have as yet been very imperfectly considered by our profession.

The object of depositing the vapour of calomel on the skin is, first, to excite an action there, and, secondly, that something may be absorbed from the powder thus deposited. In order that such absorption may take place, it is necessary that the powder should be allowed to remain on the skin. It is also necessary that the skin should be soft and clean. If the skin be covered with dry scales, or if any grease be allowed to remain on it, it is obvious that its absorbing power must be greatly interfered with. In order to ensure the proper action of the skin, I generally recommend that a patient should take a hot-air or vapour bath first. When a free perspiration has been produced in this way, the skin is wiped dry, and a clean surface presented for the action of the calomel. Without this precaution, many baths may sometimes be taken without producing any sensible effect. On the other hand, when the skin is thus prepared, its absorbing power is often at once perceptible. A patient of mine, who has for years kept his skin in the most perfect condition by means of the hot-air bath, always shows the characteristic influence of mercury on the gums if he takes a calomel bath (from a scruple to half a drachm) two days in succession. This patient always inhaled as little of the vapour as possible. I may here mention that the action of the bath is materially assisted, in the majority of cases, by the inhalation of a certain quantity of the vapour; but this is by no means a substitute for the action of the calomel on the skin.

Messrs. Pollard, surgeons, of Alfred-place, Brompton, have now for a long time very efficiently carried out my idea of the way in which the calomel bath should be given. They have had some rooms fitted up, in which a patient may take a hot-air bath, or the first part of a Turkish bath; and when the skin is acting freely, the patient is rubbed down and placed in the calomel bath. I might give many cases where patients have recovered in a very short time under this mode of treatment. It is peculiarly applicable to cases in which the skin is thickened and scaly, either naturally or from disease. Two or three years ago, a patient was sent to me by my friend Dr. Marston, of the Royal Artillery. He had been under Dr. Marston's care, and I need not say that he had thus as good a chance of recovery as he could possibly have had from the administration of ordinary remedies. The body and limbs of this patient were covered with one mass of thick scales. Occurring in a young man, it was the most marked case of inveterate psoriasis which I have ever seen. He could not move without pain, and was quite incapacitated from doing his duty. This patient was requested to go to Messrs. Pollard's establishment, and to take daily a sweating bath first, and a calomel bath afterwards. In about three months I had the satisfaction of seeing this patient's skin quite clean, and his general health restored. He has since married, and has suffered no further inconvenience from his former most troublesome complaint.

In concluding the comparison between the treatment of syphilis by the calomel vapour bath and by syphilization, I would call attention to what appears to me to be a very important aspect of the matter—viz., the *practicability*

of syphilization as a mode of treatment in this country. The method unquestionably makes great demands upon the time and patience of the sufferer and the surgeon, but particularly upon the former. We may obtain a rough approximation as to the time consumed perhaps by fixing three months as the minimum and eight as the maximum period over which the inoculations will extend. These inoculations are sometimes "terribly painful." During the whole period the patient must attend or be seen every third day for the purpose of being inoculated. The surface of his body will be the seat of numerous small ulcers and pustules, the marks of which, be they small or great, he will continue to bear upon his person afterwards. The majority of cases of syphilis, as witnessed in persons of average health and constitution, certainly recover in time under mercurial treatment without ever presenting that formidable train of symptoms which have very unjustly been ascribed to its use. Even if we should admit that mercury and other remedies only remove the manifestations, leaving the patient subject to an outbreak of syphilis at subsequent or remote dates, and that syphilization is not liable to this objection, still will not the sufferers, under ordinary circumstances, be more likely to prefer incurring the risk of subsequent relapses, or that of some remoter internal lesion, to undergoing a process of cure which will consume so much of their time, demand so much of their perseverance, and leave such traces—indelible they may be—of its action?

Again, the practice can hardly be capable of general application so long as the number of Lock hospitals and the sources for the supply of the chancre-matter is limited. So long as the inhabitants of country towns or villages may contract the disease, just so long will the pressing necessities of business, family, poverty, and other causes require that syphilitic patients shall be treated at their places of residence. In order to carry out syphilization effectually, not only will an abundant supply of matter be required, but supplies from fresh sources will be needed likewise in order to attain the requisite immunity, or, it may be, to effect the cure. Within the walls of Lock and other large hospitals and in those of our public services, there will, of course, be no such difficulties as must be felt elsewhere. But in private the treatment by syphilization would tend to bring these diseases more and more under the care of specialists. Let the power, the superior efficiency, of syphilization be never so firmly established, there are difficulties attending its application which it behoves us to consider as men of the world, as members of a practical money-getting nation, where men are so extremely jealous of their time.

Jenner warned the profession in his day against the belief that every successful inoculation with matter taken from a vaccine vesicle was necessarily the real vaccine disease; and he especially notices that after a vesicle had suppurated, it was very liable, upon re-inoculation, to produce an affection which was not followed by the legitimate results of the vaccine disease upon the patient's constitution. To apply the warning which Jenner gave to our present subject may not be out of place. It no more follows that the successful inoculation of matter from the surface of an indurated sore produces real syphilis than it follows that the production of a pustule by inoculation from what was once a vaccine vesicle necessarily communicates the genuine vaccine disease.

The transmission of a poison, and of an action produced on a poisoned part, may be clearly distinct; and to conceive them to be necessarily the same would be to give up a distinction which Jenner clearly and particularly recognised.

We conclude—

1st. That no evidence has hitherto been adduced satisfactory to the profession, that the infecting form of syphilis can be inoculated upon a patient who is at the time the subject of constitutional syphilis.

2nd. That both from a soft sore, and also occasionally from the surface of an indurated sore, matter may be taken which may be made to produce a number of local specific ulcerations having the characters of the soft chancre.

3rd. That during the continued irritation of such ulcerations the manifestations of secondary syphilis will disappear.

4th. That the time required for the treatment of syphilis in this way is so long, and the inconveniences attending it are so great, that it is not in any degree likely to be adopted in private practice in England.—*Lancet, April 14, 1866, p. 391.*

80.—ON SYPHILIZATION.

By James Lane, Esq.

[The following paper is an abstract of some observations made by Mr. Lane before the Medical Society of London, on the cases which have been under treatment by Prof. Boeck in the Lock Hospital. Dr. Boeck considers that by syphilization properly carried out, constitutional syphilis can be completely and permanently cured, and that relapses, which are unfortunately so frequent after mercurial and other treatment do not occur at all, or in so slight a degree as to be of little or no importance.]

Twenty-one cases had been under treatment by Dr. Boeck during the last four months. Sixteen of these had been well-marked examples of secondary syphilis, uninterfered with by any previous mercurial treatment. Four had been cases of secondary disease previously treated by mercury, and one was a very severe tertiary case. Dr. Boeck had been desirous of confining himself as much as possible to non-mercurialized patients, as affording the best illustration of the value of the treatment, having found that a previous mercurial course interfered materially with the regular progress of the inoculations, and with the permanence of the result.

Mr. Lane then gave a brief account of one of the cases which had been longest under treatment. The patient was a girl, aged 18, with mucous tubercles and a well-developed squamous eruption. She was admitted into the Lock Hospital in August last, having undergone no previous treatment whatever. The inoculations were commenced by Dr. Boeck on the 5th of September with matter from a non-indurated sore, two punctures being made on each side of the chest. The inoculations were repeated sixteen times in this region of the body at intervals of three days, the matter being taken each time from the inoculations immediately preceding. The resulting pustules became smaller by degrees, and at last failed altogether. A like process was then (Oct. 26th) commenced upon the arms with fresh matter, and proceeded with there till Nov. 19th, when the inoculations failed. The same thing was then done on the thighs. and has been persevered with there till the present time (Dec. 18th); fresh matter, however, having been several times required. Punctures have also been made several times on the arms with matter from the thighs, with positive results. The treatment of this case might be considered as nearly but not quite concluded, it having extended over a period of three months and thirteen days. She is now evidently losing the susceptibility to receive the poison by inoculation, and latterly, as that susceptibility has diminished, her symptoms have been rapidly disappearing.

A second case was alluded to, the treatment of which Dr. Boeck considered to be now concluded. In this case all the original symptoms had disappeared, and the immunity to inoculation with fresh matter was nearly if not quite complete. The important question of relapse of course remains to be decided.

Mr. Lane stated that in none of the cases had there been any spreading ulceration at the seat of the inoculations. The earlier sores seldom exceeded half an inch in diameter, the majority, especially the later ones. having been much smaller than this. Their average duration had appeared to be about three weeks. The health of the patients had, in most instances, improved under the treatment; in none had there been any deterioration. The disappearance of the original symptoms, it seemed, could not be expected

till towards the end of the treatment. On the contrary, a fresh accession of symptoms had been several times observed, and, in one patient, a severe attack of iritis. The inoculations, however, were persevered with notwithstanding. The patient with iritis had recovered in about the usual time without any permanent damage to the eye.

After alluding to some further details, Mr. Lane said there was one point of great interest which these experiments had served to illustrate. The inoculations had been practised with matter derived from indurated as well as from non-indurated sores. Matter from patients undergoing syphilization, but originally derived from an indurated sore, had been sent from Norway by Dr. Bidenkap, and had been inoculated on several of the patients in the Lock Hospital, with no appreciable difference in the size or appearance of the resulting pustules. Matter derived from a well-marked indurated sore in an out-patient of Mr. Walter Coulson had also been inoculated on one of the female patients, and well-developed pustules had resulted, from which many re-inoculations had been made. The conclusion to be drawn from this fact was obviously adverse to the theory that had been put forward of late years—that matter from an indurated sore could not be inoculated upon another individual already the subject of syphilis.

In conclusion, Mr. Lane said that, although he and his colleagues had not yet sufficient evidence to enable them to form an opinion as to the curative power of syphilization, they had seen quite enough to induce them to consider the investigation of great interest and importance, and fully intended to pursue it as suitable cases came before them. His association with Dr. Boeck had inspired him with a profound respect for that gentleman's scientific attainments, as well as for his straightforward and truthful character; and he felt certain that in leaving England he would carry with him the good wishes and the sincere esteem of all with whom he had come in contact.

[Mr. C. Drysdale, who had watched the progress of the cases under Professor Boeck's care, observed that no damage appeared to be done by the inoculations, no phagedena had occurred, and the pain was almost nil.

Mr. De Meric considered that the advocates of syphilization were mistaken, and that the practice must be condemned whether it be considered in an experimental, a therapeutical, or a moral point of view.]

As an experiment, the successive inoculations of the matter of chancre, from various sources and from various species, were extremely interesting; the pathologist could only gain by observing the transformations, and might perhaps be led to the discovery of some great pathological principle. But the patients thus operated on would not gain by these proceedings; the possible results just pointed out were too dearly bought; and he (Mr. de Méric) thought—though he had himself inoculated on a comparatively small scale—that we were not justified in indulging in hundredfold inoculations upon the same subject. In a therapeutical point of view, he considered that syphilization was quite nugatory. Dualists would at once condemn it: because they would justly answer that inoculating the matter of soft chancres had had no effect on the disease diffused through the whole organism; and that, even where the operators succeeded, by dint of irritation, in inoculating the matter of hard chancres, the results were simply the phenomena of the soft sore. But even unicists might say that a patient suffered from generalized syphilis by his own peculiar receptiveness; and how could those who practise syphilization hope to act upon this peculiarity by inoculating a number of chancres? Unicists knew, in spite of their belief in one virus, how difficult it was to inoculate a hard chancre upon a syphilitic patient; therefore, seeing the ease with which soft sores could be multiplied, they must admit the inability of the latter to act upon the disease. When they saw the secondary eruptions fading during the successive inoculations, they could not help recollecting that such eruptions might also fade without any treatment whatever, as shown by the non-mercurialists. Therapeutically, the patients were therefore no gainers, and were tortured to no purpose.—*Lancet, Jan.* 13, 1866, *p.* 37.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

81.—ON PUERPERAL FEVER.

By Dr. Snow Beck.

Dr. Snow Beck lately read a paper before the Obstetrical Society of London on "Puerperal Fever," carefully detailing the particulars of two cases in which the uterine sinuses were shown to be pervious after death, and from which the following deductions were drawn:

1. The phenomena of puerperal fever may be produced by the introduction of poisonous fluids into the general system.

2. The uterine sinuses remaining pervious to the flow of fluids would afford a means by which the poisonous fluid or fluids would enter the system.

3. The previous condition of the sinuses remained in consequence of the absence of that firm and persistent contraction of the uterus after childbirth which appears necessary to effectually close these canals, and prevent all circulation of fluid along them.

4. The secretion from the interior of the uterus was probably sufficient, when mixed with the blood, to induce the effects observed. And it would further follow that—

5. The various phenomena observed in puerperal fever may arise from this cause, modified infinitely by many incidental states; and the various inflammatory actions and products observed in the course of the disease would not be the essential parts of the disease, but morbid phenomena which occurred during the course of it.

6. The primary, though not the only, object in the prevention of these attacks of puerperal fever will then be to procure a firm, complete, and persistent contraction of the uterus after the birth of the child, and thus effectually to shut off all circulation within the vessels of this organ.

The author considered that the cases gave a decided negative to the opinion that puerperal fever was caused by the uterine phlebitis, lymphangitis, endometritis, metritis, or any similar inflammatory condition of the uterine organs, as no product of inflammation was anywhere discovered after a careful and even microscopic examination. The results were also opposed to the opinions of Cruveilhier, who compared the internal surface of the uterus to a vast solution of continuity, and the gaping orifices of the sinuses to the open-mouthed vessels of an amputated limb. For the internal surface of the organ was everywhere covered by a soft membrane containing all the elements of the mucous membrane and covered by a red mucous secretion; whilst the gaping sinuses could be only compared to the open-mouthed vessels of an amputated limb when the uterine sinuses were pervious and admitted fluids to circulate within them,—the point of comparison not being the open orifices, but the open canals leading from these to the veins of the general system. The comparison between puerperal fever and surgical fever was also examined and considered imperfect, inasmuch as a natural condition of the vessels of an amputated limb was compared with an unnatural state of the vessels of the uterus, which ought not to be, and might in the majority of cases be prevented. The propositions given by Dr. R. Ferguson, in his

valuable essay on puerperal fever, were examined. The author differed essentially from Dr. Ferguson on the causes of the vitiation of the blood and on the varieties of puerperal fever.

The effect of this noxious impregnation of the general system was next considered, and it was shown that the quantity modified the result in a most singular way—a small quantity being eliminated by intestinal or urinary secretion, whilst a larger dose killed. When the uterus was very lax, and admitted of a ready flow of noxious fluid through the sinuses, the woman was stricken down as if by some fell pestilence, and sank in a few hours—"where the secretions are all suspended, and the patient sinks with rapidity." Where the deleterious fluid was introduced in smaller quantity, the system, after a vain struggle with the poisonous infection, sank in a few days, the chief morbid appearance after death being extensive peritonitis of a peculiar character, copious exudation of soft friable lymph, and much serosity. To a yet smaller amount of deleterious impregnation were attributed uterine phlebitis, metro-peritonitis, distension of the lymphatics with purulent fluid; the effects being more local, and the inflammations being the consequence of the first changes induced. Whilst a still smaller amount of infection would produce low febrile conditions, extending over an indefinite period, and sometimes inducing purulent infiltration in various organs of the body. Each of these states being influenced by a variety of concomitant circumstances—as the original constitution, state of health, the character of fluids secreted, the existence of any diathetic disease, epidemic influences, &c.

On the question of contagion and the occurrence of epidemics, the author showed that the most experienced in this disease varied so much that no decided conclusion could be drawn. The prevention of the disease, it was urged, could be most effectually accomplished by procuring a complete and persistent contraction of the uterus after the completion of the labour; and that the means usually recommended were not sufficient for this object, it being generally considered sufficient to procure such an amount of contraction as to prevent any hemorrhage, though it was necessary to go beyond this point of contraction before the safety of the woman could be secured. A nourishing and supporting diet was further necessary to remove the physical fatigue and mental anxiety of the labour, to restore the health from the waste occasioned by the previous months of pregnancy, and to enable the woman to pass through the subsequent changes which had yet to take place; much, however, depended upon the previous habits, state of health, and other incidental circumstances.

On the subject of treatment, when the disease was once developed, the principles were considered to be—(1) to prevent the further injurious impregnation of the system either by obstructing the further flow along the uterine sinuses, or by removing the noxious fluids from the interior of the uterus; (2) by supporting the system during the struggle in which it was engaged, and by meeting any incidental complication which might present itself; and (3) perhaps a further source of treatment was now afforded, which might enable us to counteract, to some extent at least, the deleterious impregnation which had already taken place. The first would be attained by procuring, if possible, the further contraction of the uterus, or by inducing the coagulation of the blood in the uterine sinuses. But the principal curative means appeared to rest upon the removal of all noxious fluids from the interior of the uterus, by cleansing it with a tepid solution of any sulphite, or hyposulphite of soda each day or oftener; and should any fluid gain entrance into the uterine sinuses, it would probably be more beneficial than otherwise. The means to support the system were too well known to require further notice; whilst deleterious impregnation, which had already taken place, might be met by the internal administration of sulphite of magnesia or lime, in doses of one scruple to half a drachm every two or three hours.

Dr. GRAILY HEWITT stated that, having had considerable opportunities of witnessing the disease now under discussion, he would give some of the results of his own observations, although it was impossible in a few words to include a consideration of all parts of the subject. He had long entertained

the idea that a very close connexion subsisted between a loose relaxed condition of the uterus after delivery and the supervention of puerperal fever. He had followed a plan of treatment based on this idea, and had frequently insisted upon it in teaching. He considered that the author of the paper had offered anatomical proof of what had been a matter of surmise among many obstetric authorities of late years—namely that puerperal fever and allied disorders are not necessarily and inseparably connected with the existence of inflammatory changes in the tissue of the uterus and neighbouring parts. He believed, with the author, that the disease arose from the introduction of putrescent or decomposing material into the uterine sinuses, and thence into the general circulation. This was the cause in by far the majority of instances, but he believed that the poison was sometimes introduced by other channels. He could not regard puerperal fever as a disease *per se*, and he considered it would be difficult to distinguish anatomically and pathologically between ordinary puerperal fever and that condition produced by the contagion of scarlet fever or other fevers in the lying-in woman; for though the cause might be different, the effect was or might be the same. The essential part of puerperal fever is poisoning of the blood, and this may be effected in various ways. He had, as he had already stated, observed a very close connexion to subsist between the relaxed uterus and puerperal fever; he had always found the uterus larger than it should be at the outset of the attack, this increase of size being accompanied with tenderness and other signs. In this respect, therefore, he quite agreed with the author of the paper. It would afford confirmation of the truth of this view of the subject to describe the treatment which he (Dr. Graily Hewitt) had for some time adopted in cases of puerperal fever and its results. Disregarding entirely and completely the old ideas as to inflammatory changes in the uterus, he was in the habit of applying the bandage very tightly over the uterus the moment the first symptoms appeared, and of administering internally a stimulant diet, including large doses of alcohol. Other remedies were used, but they were quite subordinate. Depleting and lowering medicines were wholly omitted. The quantity of alcohol given was sometimes at the rate of as much as two ounces of brandy every two hours. This treatment he had seen cut the disease short in the course of twelve hours. He had the greatest confidence in the efficacy of such treatment in bad cases of puerperal fever, having seen not a few apparently hopeless cases recover under it. In reference to the prophylaxis of the disease, these observations were interesting. The binder served a very important purpose, and he was in the habit of paying the greatest attention to its careful application, believing that in a well-contracted uterus we have the best safeguard against puerperal fever. The facts related by the author of the paper bearing on this latter subject were very important.

Dr. BARNES said the subject was one of such paramount importance that it deserved a special discussion every year. He believed, if attention were thus continually brought to it, that much would be done in the way of preventing this the most destructive of all causes to puerperal women. It was impossible to advert now to more than one or two points. He agreed with Dr. Beck that puerperal fever was a distinct thing from typhoid or scarlatina, which often attacked puerperæ, but preserved their essential characters. On the leading idea of the paper—the necessity of contraction of the uterus as a preventive of puerperal fever—he would say, that contraction was eminently desirable to accomplish, but he had seen repeatedly puerperal fever occur in women after perfect contraction, and, on the other hand, no puerperal fever although the uterus remained relaxed. After the great discussion at the Académie de Médecine, Dr. Martineng published a memoir setting forth this view, and advocating the use of ergot and all means to ensure contraction as preservative against fever. Dr. Barnes having under observation 2000 or more cases yearly of poor women attended by the Royal Maternity Charity, thought he was able to negative the proposition that puerperal fever was more frequent amongst the poor and badly fed. The cases in the charity were very rare. For one case amongst these women, he saw ten amongst

the easier classes. As a means of preventing the loss of blood—as hemorrhage undoubtedly predisposed to puerperal fever—he had found nothing of equal efficacy to the injection of a solution of perchloride of iron into the uterus after clearing out the cavity of placental remains and clots. He had used this plan for several years, and in a large number of cases after labour and abortion, and had always had reason to congratulate himself upon the result. The perchloride of iron had the further advantage of being antiseptic. He had certainly saved several women from death from flooding, and believed these and others had been rendered less liable to puerperal fever. In reply to a question from Dr. Timothy Pollock, the President stated that the plan he adopted was to carry in his "obstetric bag" a saturated solution of the salt. Of this solution he used one part to eight of water. But he thought that even a weaker solution was sufficient. It instantly coagulated the blood in the mouths of the uterine vessels.

Dr. Snow Beck briefly replied, and stated that he considered puerperal fever essentially distinct and different from typhus fever, typhoid fever, scarlet fever, or any of the acute specific diseases; each of the latter being caused by a distinct and definite poison, which might be communicated from individual to individual in the same ward; whilst the former was produced by the introduction of putrid or similar fluids into the general system through the sinuses of the uterus, when they remained pervious after a confinement, and was *not* communicated from individual to individual in the same ward. Perhaps the term "puerperal fever" was not the most appropriate, but it had been consecrated by long usage, and it was not desirable to change it without good cause. There was a great distinction between puerperal fever and febrile states occurring during the puerperal period, the former being incurable up to the present time, the latter readily admitting of cure. This distinction had been clearly drawn by Professor P. Dubois. He had used the soluble sulphites as injections for cleansing the interior of the uterus, from the opinion that they were the most effectual agents in correcting the condition of the fluids secreted. But he had no doubt other agents would be found equally and perhaps more efficacious, and amongst them possibly the preparation of iron mentioned. But if it were even admitted that this plan of treatment was advisable, and devoid of the great danger sometimes attributed to it, then an important step had been attained. He, however, would strongly urge the necessity of procuring complete and persistent contraction of the uterus, and of carrying this contraction further than was usually considered sufficient, as being one of the most effectual means of preventing this most serious disease. He thought that by separating the many diseases occurring during the puerperal period, and included under the one designation "puerperal fever," though differing essentially in their nature, course, and treatment, great good would result.—*Obstetrical Transactions*, *Vol.* vii., 1866, *p.* 31.

82.—ON THE USE OF WIRE-RIBBON IN SOME CASES OF DIFFICULT TURNING.

By Dr. Val. Heyerdahl, Bergen.

The application of a running noose of tape round the ankle of the child, in order to facilitate version, is easily made at the outlet of the pelvis, without any special contrivance for its performance. But when the feet are in the brim or pelvic cavity, this little manœuvre may be attended with great difficulty, and must sometimes be dispensed with altogether (as impossible).

To remedy this difficulty, several more or less useful instruments have been constructed, and in some text-books of midwifery, students are recommended always to carry a "proe-nœud" in their obstetrical armamentarium. I will only cite the names of some inventors, Pugh Neverman, Gerner, Braun, Trefurt van Huevel, Wasseige, &c., to show that their number is not so limited. Of modern writers upon the subject I will mention a most valuable treatise by Dr. Hyernaux, of Brussels. This distinguished accoucheur

points out most impressively that, in some cases of difficult turning, an instrument to apply the tape is indispensable; at least he has succeeded by its aid where others have completely failed in accomplishing version. He pretends that he, by his instrument for applying a running noose of tape high up in the vagina, has been able to turn in cases where embryotomy apparently was the only method practicable. Dr. Hyernaux had the kindness, during my stay in Brussels, two years ago, to show me his instrument and explain the manner of using it. It is a staff of German silver, with a horse-shoe shaped bi-valvular tube, with open bevelled extremities fixed to its top. At the handle is a spring, by means of which the horse-shoe shaped tube can be opened into two semi-circular grooved valves. A loop of the running noose of tape is put into, and fastened in, the bi-valvular tube. The instrument is then introduced, with the horse-shoe, along the arm and hand of the accoucheur, up to the foot, which is then slipped into the circle formed by the tube and the free part of the loop, between the two extremities of the horse-shoe. By pressing on the spring, the tube readily opens, and lets the loop of the running noose free, surrounding the child's ankle. It is then tightened by pulling on the free ends of the noose.

I found the instrument rather ingenious, and easily applicable, and, as far as I could judge without putting it to test, the very best of the many invented for the same purpose, by other accoucheurs, from the time of Smellie and Siegemundin up to the present day.

I agreed with Dr. Hyernaux in his views. I had myself sometimes found it exceedingly difficult to get a running noose of tape up to the foot, and in such an emergency much missed the aid of a proper instrument. I therefore procured one of Hyernaux's, with the view of trying it on the first opportunity.

But at the very first case of difficult version I met with on my return to Norway, I had forgotten to carry the instrument along with me, which necessitated the employment of a substitute for it, which I am now going to describe.

It was a case of transverse presentation, in the country, some miles from Bergen, in which the diagnosis had not been made out, and the labour, therefore, had gone on a considerable time unrelieved,—the waters having flowed away about two days before my arrival. The pains had been rather energetic for the last twelve hours, with strong bearing down. I found the left shoulder presenting low down in the vagina, and considerably swollen; the uterus and os firmly embracing the fœtus. The fœtal heart was heard indistinctly about the umbilicus. I put the woman—a strong primipara at the term of gestation—under the influence of chloroform, and proceded to turn. It was impossible to push the bulk of my hand into the uterine cavity, although the bearing down pains were very much lessened; but with two fingers within the os I reached and got hold of one foot, aiding this purchase by forcing the pelvic extremity of the fœtus lower down with the other hand. So far I succeeded; but I soon found that I wanted strength enough in my finger to bring the foot farther down than to the brim, the foot being in such a degree slippery that I lost my purchase every time I tried to pull down. At the same time it was impossible to raise the presenting shoulder up to get a little more room. I tried in vain to bring up a tape round the ankle. I failed repeatedly on account of the looseness of the tape, moistened in the discharges from the vagina, and I regretted very much that I had not the instrument of Hyernaux at hand, while I was convinced of its usefulness and importance in this case.

It then struck me to try another material for a running noose, with more stiffness and not so liable to slacken in moisture as common tape. I found amongst a large stock of ribbons, tapes of different kinds, &c., a peculiar sort of tape with thin wires interwoven, commonly used by milliners to give bonnets their different shape and fashion, called wire-ribbon.

I took about a yard of wire-ribbon, half-an-inch broad, doubled it up, and formed it into a running noose. I made the loop a little elongated, and large enough to slip a foot through, and twisted the free ends a little to give them

more stiffness. I now succeeded in sliding a well-oiled running noose of wire-ribbon up to the foot, and to get a firm hold of it, and then, by moderately pulling on the foot, securely grasped in this manner, and at the same time pushing the presenting shoulder upwards with the other hand introduced into the vagina, I was fortunate enough to turn it in no time. Soon after, the child was born by nature alone, with the exception of a little manual force to extract the head. The child was dead; but the patient did well, according to the report given me two days thereafter.

This case is, I think, of some interest for the practitioner, because it proves that the difficulty of grasping the foot high up in the vagina, by a running noose of tape—sometimes baffling our best endeavours—may be overcome without instrument in so simple a manner,—namely, only by substituting the material commonly used—tape—by another equally simple material—wire-ribbon, to be procured wherever bonnets are worn.

Of course the method of applying wire-ribbon may be modified in different ways; for instance, by doubling the wire-ribbon, then twisting it in its whole length, only the uppermost portion being left untwisted and forming a loop. To the other end is fixed a handle, made of a small piece of wood. This little apparatus is used in the same manner; the only difference is, that the loop is tightened by augmenting the number of twistings by turning the handle. The material possesses this quality, that the twistings once made, do not untwist again, and thus the foot is securely and firmly grasped.

But I am inclined to think that the method put into execution in my case is perhaps the simplest, and, therefore, also the best.

Should the stem of the running noose give too much during its introduction, this inconvenience may be remedied by using a small surgical forceps to introduce it with.

There is one drawback to its use; wire-ribbon is sharper and more apt to cut than common tape, and I was obliged to protect my hand by means of a handkerchief. Around the ankle of the child was left a deep furrow, but it had not cut through the skin.

By applying the wire-ribbon running noose to the feet of the dead body of the child, I was not able to cut through, although I purposely used much more force, and for longer time, than advisable in any circumstance connected with the operation of turning a living child.—*Edinburgh Med. Journal, Jan.*, 1866, *p.* 673.

83.—A REMARKABLE CASE OF "MISSED LABOUR": THE FŒTUS BEING RETAINED IN THE ABDOMEN FORTY-THREE YEARS.

By R. W. Watkins, Esq., Towcester.

On January 10th, 1866, I visited, by request, Elizabeth Jones, aged 74, widow of William Jones, a small village shopkeeper at Stoke Bruerne, Northamptonshire. She gave me the following history, which I will narrate as nearly as possible in her own words.

About forty years ago, she was in labour with her second child. Her first, born two years previously, having had water in the head, had survived its birth only a few hours. The labour being very lingering, they had sent for my late father, who attended her, and remained with her during the night. On the following day, being very busy, and the labour having made no progress, he left her in care of the midwife who was in attendance upon her. The pains were lingering, but not very severe. On the third day, she felt something "drop down suddenly inside her; and the child, the movements of which she had constantly felt up to that time, at once became cold as a stone." She was in great pain, and my father attended her for a considerable period. She refused to allow any other surgeon or physician to attend her, or to have any operation performed. She gradually improved in health, although she was very weak for a long time, and did not decrease in size for several years.

Upon examination, I found her much emaciated, with a hard bony tumour in the lower part of the abdomen, exactly resembling the fœtal head. It could be easily moved from side to side; and on careful manipulation, I believe that I felt the back part of the thorax in close approximation to it. She was evidently sinking from chronic renal disease; and knowing that her end was approaching, had sent for me to request that I would make a *post mortem* examination. She had previously made a similar request to Mr. William Knott.

The story was corroborated by some of her neighbours, and by the rector of the parish, who had heard from my father a full report of the case. It was also stated, that at different times three little bones "like finger bones," had come away from her; but they had not been preserved.

On referring to old ledgers, I found the entry of the attendance on October 8th, 1822, in the handwriting of my grandfather, with the subsequent payment of the fee marked in my father's handwriting, and with his initials.

The woman died on January 13th, and the examination was made on the 15th by Mr. William Knott, in the presence of Mr. Knott, sen., Mr. Garlike, and myself. On making an incision through the abdomen parietes and opening the peritoneum, we immediately observed a hard white substance, which proved to be the vertex of a fœtal skull; and, on enlarging the opening, a perfect fœtus was extracted without difficulty. It was covered with plastic lymph; the limbs were flexed anteriorly on the body, and the head bent forwards to meet them, in the manner usually depicted in plates of the gravid uterus. It was attached by the umbilical cord to a vascular tumour of about the size of half an orange, which appeared to be the atrophied placenta, and which was connected by ligamentous attachment to the peritoneal covering of the broad ligament near the left ovary. One portion of this vascular tumour appeared to be a mass of unorganized lymph, containing fluid. Neither the fœtus nor the supposed placenta had any adhesions to the peritoneum, except the ligamentous attachment I have already mentioned. The uterus was perfectly normal. There was no cicatrix or other marks of injury on any portion of its surface. The ovaries were pale, but quite natural in size and form. There were no adhesions of the peritoneum in any portion of the abdominal cavity, and no appearance of previous inflammatory action in any part of the large or small intestines. The kidneys were extensively gorged with venous blood, and very friable; the left being more affected than the right. The spleen also was much congested and friable. The stomach and liver were healthy. The gall-bladder contained about twenty hard dark gall-stones. The pancreas was very much diminished in size. In front of the aorta, and immediately above the inferior mesenteric artery, to which it was connected by condensed cellular tissue, was a white encysted tumour, of about the size of a hen's egg, containing a milky fluid.

The above extraordinary case is, so far as I am aware, quite unique, and will be interesting, not only as an instance of recovery from tubular gestation and probably rupture, but also from the comparatively slight local and constitutional effects of a foreign body retained in the cavity of the peritoneum for more than forty-three years. The specimens have been forwarded to Dr. Barnes, for exhibition at the meeting of the Obstetrical Society.—*British Medical Journal, March* 3, 1866, *p.* 223.

84.—ON ENLARGEMENTS OF THE UTERUS WHICH FOLLOW ABORTIONS, PREMATURE OR NATURAL CONFINEMENTS; WITH CASES.

By Dr. Snow Beck.

(Abstract of a paper read before the Obstetrical Society of London.)

The author remarked that these enlargements had been long recognised by pathological anatomists, and quoted some microscopical observations by himself, and communicated to the Medical Society of London in 1851, which

showed that the pathological condition essentially consisted in an enlargement of the muscular tissue of the uterus, without the presence of any inflammatory or heterologous deposits. The causes were considered to depend chiefly upon—(1) a want of complete and persistent contraction of the uterus, which permitted an increased circulation of blood in the gravid organ, and interfered with the changes which took place after parturition; and (2) on the partially developed state of the uterine tissue in abortion, which appeared to be unfavourable to the development of those changes necessary to its complete reduction in size. The enlargement of itself gave rise to few and comparatively slight symptoms, unless it existed to such an extent as to be felt as a tumour in the hypogastrium; but it rendered the patient liable to profuse hemorrhages, coming on suddenly and without appreciable cause. These enlargements might exist for many months, or even for some years, without any symptoms of importance; but from the recurrence of the catamenia, or other causes, congestion of the enlarged organ was gradually induced; or congestive inflammation, which may be either of the whole or of any portion of the uterus, might take place, the usual symptoms of uterine affection being then present. Amongst the subsequent changes which took place were anteversion and retroversion, with more or less bending of the organ, which reduced it to an indolent state, and rendered it very rebellious to treatment. The modifications of symptoms thus produced was shown by the cases recorded, and the physical examination of the organ detailed. The author found that in these cases the uterus was equally enlarged, smooth, pyriform, the cavity enlarged, and the orifice open. The sound readily passed to an extent varying from three to five or six inches. When inflammation was added the organ became tender, the arteries were felt to beat with more or less force, and the interior became very sensitive. Subsequent and various alterations were made: the lips became enlarged, often lobulated, projecting into the vagina, red and raw in appearance, and bounded by a distinct line, which marked the division between the mucous membrane of the vagina and that of the uterus. It was this condition of the organ which had been so frequently described as ulceration, although no such morbid change actually existed. With respect to the treatment, various cases were recorded showing the importance of injecting the cavity of the uterus with astringent lotions, and the safety with which it could be done, provided the actions of the uterus were perfectly quiescent. The cavity of the uterus being enlarged, and the orifice open, impregnation readily took place: and the physiological changes which followed were decidedly the best means of restoring the organ to the healthy state. Congestion or inflammation, when present, would have to be met by the usual means; and when the uterus was in the hardened, modified condition, in addition to the ordinary means of treatment, it would require some local stimulant to rouse the local action, and enable the other remedies to act. For this purpose cauterizing the lips with potassa cum calce had been generally employed.

Dr. BARNES observed that he continued the practice of injecting a solution of perchloride of iron into the uterus to arrest hemorrhage after abortion and labour, and with excellent effect. He no longer dreaded flooding as of old. So far he could illustrate by experience the safety of intra-uterine injections. But he thought a more desirable method of applying fluid styptics or caustics to the inner surface of the uterus would be by swabbing; that is, soaking a bit of sponge or cotton-wool in the liquid, and passing it into the cavity. He had contrived an apparatus for this purpose. An excellent plan of applying solid nitrate of silver was one he had learned from Sir Benjamin Brodie. That eminent surgeon dipped a silver probe in fused nitrate of silver, thus obtaining a thin stratum, which could be passed freely and safely into a sinus. This was the safest way of cauterizing the inner surface of the cervix or body of the uterus.

Dr. GREENHALGH said that Sir J. Y. Simpson had described these enlargements under the terms of subinvolution or incomplete involution of the uterus. He (Dr. Greenhalgh) quite agreed with the author as to their frequency and the obscurity of the local symptoms. He regarded it as a com-

mon cause of sterility, but that where impregnation resulted it was always curative, except where abortion ensued. In this disease he had found the uterus enlarged, flabby, and ill-defined, the sound entering an enlarged cavity four inches, and even seven inches and a half. He considered that hemorrhage was not a frequent symptom in this disease, and, where it did exist, was mainly attributable to some affection of the lining membrane of the uterus. He advised, where hemorrhage was frequent or profuse, so as to affect the general powers and resist the ordinary treatment, the injection of the compound tincture of iodine into the uterus; but laid great stress upon the importance of first freely dilating the internal os uteri, which dilatation in itself was more or less curative. He had found resolvent and sedative pessaries of value, as also douches of tepid and cold water with a Kennedy's syringe, and medicated fluids. He likewise advocated the administration of tonics, with the iodide of potassium and liquor of the ergot of rye, and alterative doses of bichloride of mercury. He relied greatly for success upon improvement of the general health, out of some impairment of which this affection frequently originated and persisted.—*Lancet, March* 17, 1866, *p.* 288.

85.—ON HEMORRHAGE FROM DETACHED PLACENTA NEAR THE EIGHTH MONTH OF GESTATION: LABOUR INDUCED BY BARNES'S DILATOR: RECOVERY.

By W. J. Tubbs, Esq., Upwell, Cambridgeshire.

Mrs. ——, aged 44, had had three children, each labour being normal. She said she was standing in a cart on the 25th of November, and giving her little girl to her husband, who had just got out at his own door, when the horse went on and she was thrown forwards, but was saved by her husband from falling. She was frightened, but soon recovered from the shock. During the night, she got out of bed to pass urine, when a large quantity of blood poured from her. When she returned into bed, the hemorrhage ceased. In the morning, she rose and continued her domestic duties as usual.

On that day week (December 2nd), she had a return of the hemorrhage, which according to her account, was profuse. It came on while she was in bed, and continued until she was faint. Her husband got up and came for me about twelve o'clock at night. I went immediately, and found her still faint. She had lost a great deal of blood, which was still oozing from the vagina. The os uteri was tilted upwards and forwards; it was a little larger than a sixpence, and rigid. I plugged the vagina with a silk handkerchief; applied cloths dipped in cold water over the pubes and to the vulva; raised the legs; admitted air freely into the room; removed some of the bedclothes; and gave twenty drops of oil of turpentine and a dose of ergot as soon as I could.

I watched her the following day and night, giving her diffusible stimulants, with an egg in some tea and gruel occasionally. She did not have any pains from the very first of my seeing her.

I left word that, in the event of any more loss, I was to be sent for immediately; and that she was not to get up for some days. In the course of two days, she came down stairs at noon, and continued much the same until seven o'clock in the evening, when, on crossing the room, the hemorrhage returned, and she fainted. She was taken to bed, and I was again sent for, and arrived shortly afterwards, when I found her very pale and almost pulseless. The vagina was filled with coagulated blood; the os uteri flaccid, and a little more dilated than on my first examination. I could just make out that it was a vertex presentation. I did not think it advisable to rupture the membranes, as I might, if necessary, turn better with them entire. I decided to use No. 1 dilator, and deliver by turning as soon as the os became sufficiently expanded. Desiring assistance, I sent to Wisbeach for the first surgeon who might be in the way, and at once began dilating the os. I found some difficulty in making the India-rubber remain in the os; but at last I

succeeded by passing it in with a piece of whalebone. This answered admirably. There was no pain complained of during the dilatation; and, when the os was sufficiently opened, I left off injecting, and allowed it to remain until the arrival of the surgeon. When Mr. Wm. Groom arrived, he examined the expanded os, and agreed with me that I might introduce my hand. Just at this time, it seemed that the dilator had got its full volume of cold water; for, in attempting to inject a little more, to show Mr. Groom how it acted, the dilator burst with a loud report. As soon as the gush of water had come away, I passed up my hand by the side of the head and brought down the feet. The only little difficulty I experienced was from the os uteri contracting on the head, which I liberated as soon as I could introduce my finger into the mouth. The patient had not felt the child move for some hours; and it was still-born.

Dec. 15th. She was going on favourably, feeling only weak from the excessive loss she had sustained.

I have now used the dilators three times, with good results.—*British Medical Journal, Jan.* 20, 1866, *p.* 67.

86.—ABSTRACT OF A PAPER ON PROCIDENTIA UTERI.

By Dr. J. Marion Sims, Honorary Fellow of the Obstetrical Society.

[Several separate and independent conditions must exist in a case of procidentia uteri. Thus, there must always be a broad pubic arch, with very divergent rami and a relaxed perineum; and then the axis of the uterus must be turned back in a line with that of the vagina and the pelvic outlet; in other words there must be a retroversion. With the uterus anteverted, a procidentia is utterly impossible, be the attendant circumstances what they may.]

Huguier has written extensively on procidentia uteri, and I believe he was the first to point out the distinctive characteristics of its anatomical peculiarities. He found elongation of the cervix in all cases, either above or below the insertion of the vagina; and he suggested and performed amputation of the neck of the uterus in every case, and with great success.

I amputate the cervix only when its lower segment is too large or too long, and projects so far into the vagina as to present a mechanical obstacle to the retention of the uterus *in situ* when replaced. If there should be elongation of the infravaginal cervix, amputation is the remedy; but we often find procidentia without any extraordinary elongation of the infravaginal portion of the cervix. There is then nothing to amputate.

In these cases Mr. Baker Brown, Dr. Savage, and others, contract the vulvar outlet by the perineal operation; but generally I prefer to narrow the vagina above, which usually very effectually retains the uterus in something like a normal position within the pelvis.

The idea of narrowing the vagina is by no means new. I suppose we may justly claim it for the great Marshall Hall. However, I do not think the operation ever succeeded till my own day, and this success is due wholly to metallic sutures.

The author then described a case in which he observed that the descent was not by the protrusion of the cervix uteri, but invariably by a prolapse of the anterior wall of the vagina, which always preceded the cervix, and drew down the uterus. He noticed also that, by pinching up the anterior wall of the vagina into a longitudinal fold, the parts had no tendency whatever to come down; and that it was impossible for the patient to force them down if the anterior wall of the vagina was prevented from descending. Hence the idea of removing the redundant portion of the anterior wall of the vagina; but "it did not occur to me to operate simply by removing strips of vaginal mucous membrane. I seriously proposed to this lady to make a complete vesico-vaginal fistula, by removing at one blow, as it were, a large portion of the base of the bladder with the anterior wall of the vagina. She agreed to it, and it was adopted."

Proposing to excise the anterior wall of the vagina, I hooked it up with a tenaculum, pulled it well towards the posterior wall, and grasped the base of the mass thus elevated with a pair of curved forceps made for the purpose, acting on the principle of Ricord's phymosis forceps, which held the parts firmly embraced, while with scissors cutting under the forceps I removed, at once, a very large portion of the anterior wall of the vagina. The portion removed measured two inches and a half transversely, by two inches and five-eighths longitudinally, and was very thick. The chasm made by this operation was fearful, the lateral retraction of the divided edges being so great as to present at a superficial glance some difficulty in bringing them together by sutures. There was, however, no trouble whatever.

The bleeding was not profuse; but I at once rapidly filled the chasm with cotton, to stop the hemorrhage by pressure. A few minutes sufficed for this; and then the tampon was removed for the purpose of closing the edges of the opening by transverse sutures. My surprise was equalled only by my delight, when I found that I had not succeeded in doing what I intended; for instead of excising the base of the bladder with the anterior wall of the vagina, I had, by the tenaculum, simply raised the hypertrophied vaginal tissue up between the blades of the forceps, luckily separating it from the lining membrane of the bladder, which remain intact. Thus, by a mere accident, the operation was really far better than if I had succeeded in accomplishing what theoretically I proposed to do.

The lateral edges were brought together longitudinally by silver sutures passed transversely. She was soon well, and is so to this day. The operation was done nine years ago. This method of operating was continued till 1858, when an elderly woman, with an enormous procidentia of fifteen or twenty years' standing, was sent to the Woman's Hospital by Dr. Duane, of Schenectady. It was a very bad case indeed. I operated by the plan of simple denudation over an immense surface; the parts were brought together and healed kindly; but I had not removed tissue enough, and there was a considerable cystocele left, which required that she should use a pessary; and then it was that I devised another method of operating. Instead of the broad scarification of the anterior wall of the vagina, I simply removed the mucus membrane in the form of a V, the apex being near the neck of the bladder, the two arms extending up on the sides of the cervix uteri. These two denuded surfaces were brought together by silver sutures passed transversely, thus making a longitudinal fold narrowing the vagina and crowding the cervix backwards. This simple operation was thus repeatedly performed, and always successfully, by Dr. Emmet and myself, at the Woman's Hospital, from 1858 to 1862, when I left New York.

In Paris I had occasion to perform it for Sir Joseph Olliffe, on an old lady sixty-five years of age, who had had procidentia for twenty years. The parts united; the uterus was held in its place, and she returned home in a fortnight. Her general health was very feeble, in consequence of a long residence in India; and in two months the whole cicatrix gradually gave way, and the procidentia was reproduced. This was the first and only case of failure that I had ever seen after this method. The operation was subsequently repeated; but this time, instead of a V-shaped scarification, it was made in the form of a trowel, the point presenting below, the shoulders above in the anterior cul-de-sac. The denuded surfaces were brought together by transverse silver sutures. A small portion of tissue was left undenuded, for the purpose of permitting the escape of any secretions naturally forming in the shut pouch.

It is not my intention to draw a parallel between this and the perineal operation for procidentia. I only wish to add another resource to our means of permanent cure in this distressing affection. So far then, as mere surgical resources are concerned, we have three processes from which to choose; always, of course, adapting this choice to the peculiar exigencies of the case.

1st. Amputation of the cervix, as recommended by Huguier, when its infra-vaginal portion is too long. I have often seen procidentia cured by this alone.

2nd. The perineal operation, as performed by Mr. Baker Brown, Dr. Savage, and others; and

3rd. The operation of narrowing the vagina by the trowel or triangular-shaped denudation on its anterior wall.

Mr. CHAMBERS, at the meeting of the Obstetrical Society at which the foregoing paper was read, said he felt that the Fellows of the Society were much indebted to Dr. Sims for his valuable contribution to female surgery; but he submitted that even this operation was inadequate to meet requirements of all the cases of complete prolapsus uteri which come under the notice of the surgeon from time to time. Thus, this, which may now be called the anterior operation, only takes rank with that which may properly be called the posterior operation, or Mr. Baker Brown's. Mr. Brown has never asserted that his operation is successful in every case of prolapsus uteri. On the contrary, he has for years past combined the two operations in cases necessitating such a procedure, though he (Mr. Chambers) was bound to say that the anterior operation, as performed by Mr. Brown, was a very simple one when compared with Dr. Sims's. He had had two very bad cases of this kind under his care within the last twelve months. One case was of three years' standing. This lady had not been able to leave her house for more than two years. He performed Mr. Brown's operation in November, 1864, and the patient is now in excellent health, having had no signs of a relapse. The other case was that of a poor woman, who had suffered from a complete form of the disease for "over *two years*." Mr. Brown's operation was performed in this case, and for six months she has discharged the duties of her station with perfect ease and comfort. This patient came to the out-patients' room a month ago, and the parts appeared as perfect as when the operation was first performed. There are two important reasons why the posterior operation so often fails in the hands of some operators—first, the operation itself is *in many cases* very imperfectly performed; and second, the patient is permitted to get up and walk about long before the united surfaces have had time to become perfectly consolidated, or the enlarged uterus to become reduced to anything like its normal size and weight. Cases of this kind ought not to be permitted to leave their bed for eight or ten weeks at least, if the cure is to be permanent.

The PRESIDENT said he had asked for the experience of Fellows on the result of Mr. B. Brown's perineal operation, because, if this were found to fail, the argument in favour of Dr. Sims's operation was much strengthened. He had himself performed Mr. Brown's operation several times. He had found prolapsus return in spite of the provision of a good perineum. Further, he had known complete prolapsus take place in virgins; in one case especially, that of a girl aged sixteen, prolapsus was caused by epileptic fits, the vulva and perineum being quite normal. Again, he had repeatedly seen the uterus remain *in situ* notwithstanding large perineal laceration. It was clear to him, therefore, that defective perineum was not the cause of prolapse, and that operations for restoring the perineum or contracting the vagina posteriorly could not be expected to cure it. It seemed to him that Mr. Brown's operation was performed in the wrong place. It had no effect in providing support where it was wanted, namely, anteriorly, at the connection of the cervix uteri with the base of the bladder. This object seemed to be perfectly accomplished by the admirably reasoned-out and ingenious operation of Dr. Sims. Dr. Barnes briefly described his view of the etiology of the hypertrophic elongation of the cervix. It began with congestion, inflammation, and partial eversion of the lower margin of the cervix. The outer or vaginal surface of the vaginal portion was the more fixed, whilst the inner aspect of the canal more easily bulged outwards. Then the products of congestion became organized in the tissues, the fluid elements being absorbed. This process continually going on—eversion or growing out of the rugæ of the cervical canal—extension and hypertrophy went on. A demonstration of this kind was obtained by passing the uterine sound up to the fundus, and then gently pressing upwards, so as to push the prolapsed uterus back towards the vagina. The last-formed everted portion of the canal then disappeared under a process of reinversion. This showed how the abnormity might be prevented if the

original inflammation were cured in the early stage, and if the uterus were properly supported in its normal position.

Dr. Sims, in reply to Dr. Aveling, said he left the undenuded portion of mucous membrane near the cervix uteri, because this was the most dependent part when the patient was lying down. In reply to other speakers, he said that no one method of operating could be applicable under all circumstances to all cases. If there was a lacerated perineum it was proper to restore it. If there was elongation of the infra-vaginal portion of the cervix, then the proper operation was amputation of the cervix, and this would probably be all that was necessary in such cases. His friend Dr. Eastlake had shown him a case of procidentia, two or three days ago, where it depended entirely upon hypertrophic elongation of the lower segment of the cervix, which projected at least two inches below the insertion of the vagina. This patient had been the rounds of many of the hospitals in London, and nothing had been done for her. The perineal operation and his own method were each equally powerless to remedy such a case. Amputation here was the only resource. He had the pleasure of operating on a patient of Dr. Henry Bennet's a few months ago, where the procidentia depended upon hypertrophic elongation of the cervix. The operation of amputation alone cured the case. In the majority of cases of procidentia—in all those where a vagina-cystocele formed the first stage of descent—he was satisfied that the operation he had described, viz., that of narrowing the vagina by forming a longitudinal fold of its tissue on the anterior surface, reaching from the neck of the bladder to the neck of the womb, would be one of the safest, simplest, and best. As to mechanical means for such cases, he had seen the instruments fail which Dr. Williams had shown us. He had seen cases in which Hodge's lever, Meigs's ring, globes, discs, and air-balls, were all useless. In some cases Zwanck's pessary would answer; but no instrument could be compared with his simple operation for a radical and permanent cure. Besides this, there were many who could not bear any mechanical apparatus. In old women the vagina was often so delicate and tender that none of the ordinary forms of pessaries could be borne. In such cases, if the patient would not submit to the operation, and could not wear an instrument, there was still one thing left to palliate her condition, and that was she could wear a tampon of cotton, the size of an English walnut. It should be moistened with glycerine, and introduced every morning, to be removed at night.—*Obstetrical Transactions, Vol.* vii., 1866, *p.* 238.

87.—ON THE RADICAL CURE OF PROLAPSUS UTERI.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

[Within the last few years many have attempted the permanent cure of prolapsus uteri by plastic operations on the vagina. Many cases have ended successfully. All cases of course are not suitable. Perhaps the most favourable conjunction of circumstances is to be found in an otherwise healthy woman at the close of the child-bearing period, and from that time up to fifty-five or sixty.]

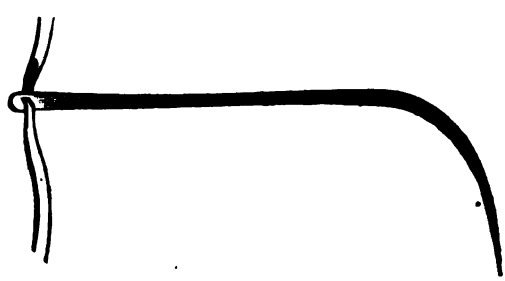
The operation requires the assistance of two or three surgeons (not including the chloroform administrator) and also of a good nurse. It is one which is attended with very little pain for the greater part, but the unavoidable exposure clearly demands the aid of the anæsthetic. The only instruments necessary are a few scalpels; a forceps long and fine at the points, with a tenaculum grip; a duck-bill speculum for stretching the vaginal canal and holding up the uterus; appropriate needles, and the silver wire and quills which I shall describe.

The patient having been made insensible before the introduction of any of the assistants into the apartment, is drawn to the edge of the bed—a mackintosh being placed under her,—and is placed in the lithotomy position before a good light, an assistant holding each a limb, and a third the duck-bill speculum, while the operator seats himself in front. The parts are closely shaved; the

surgeon has then to decide the amount of space which he intends to denude. From the ease with which the lower portions of the vagina can be stripped of their mucous membrane, the temptation to make this denudation pyramidal in its figure is not slight. But upon observing almost the reverse of this figure the future success of the operation really depends—as may be understood from my former article. Let the base of the pyramid, if it be possible, be at or near the cervix uteri, rather than at the vaginal orifice; for there the pressure will be felt the most, and the resistance should be proportionate. I can conceive it possible that if a sound broad surface could be made to unite just below the cervix for a few lines down the canal, it would be far more likely to cure the prolapsus than the most elaborate closing up of the entrance to this canal. I do not, of course, recommend the figure of an inverted pyramid as that which is to be really described; but rather that a square surface should be laid bare, as broad near the cervix as is possible, and extending on either side of the vagina below in such a manner when the sides are apposed as to close up the canal to about half of its former diameter, or even more in cases where there is great relaxation. We thus secure a solid column almost as broad above as at its base, on which the uterus can be supported henceforth. The denudation is only to be very superficial. With a very keen knife a fine film of tissue may be taken off, and this is quite sufficient to secure adhesion, while it diminishes the risk of the operation and the liability to hemorrhage. Over some parts this is best effected by a species of shaving—as one would cut thin films off a cork; but for this purpose the structures must be made as tense as possible. I must also make one very important practical observation, which is this, that however carefully the parts in this and other plastic operations on these mucous tracts are brought together, union on the first intention will not take place on the *edges* adjoining the mucous membrane. I have very carefully noted this fact. The parts there almost invariably heal by granulation, while the deeper parts become agglutinated and adhere together often without any suppuration whatever. It is therefore necessary to lay bare an amount of surface somewhat in excess of that which we really wish to cause to adhere together. I have nowhere seen any allusion made to this point in the directions for operating on these parts. Plenty of time must be allowed for this part of the operation, and it may be the better because the patient is losing very little blood—altogether not more than a few ounces—and is in no other way injured by a little delay; whereas if the denudation be done in haste, it is almost certain to be attended with hemorrhage to a considerable amount, and there is the probability of some part being left unbared—the seat of a suppurating sinus hereafter.

The denuded surfaces, being carefully wiped and freed from clots and shreds of membrane, are then laid together. Hitherto they have been recommended to be kept in apposition by two or three deep ligatures of cord, with quill sutures and several superficial interrupted ligatures. But I have to introduce a method of effecting this purpose which will, I believe, prove of great simplicity and value. In the stead of cord I employ a *flat ribbon* of silver, very thin and soft, rather less than the one-sixteenth of an inch in breadth.

FIG. 1.



The needle requires, of course, a transverse slit for carrying it. I have found a shortish needle, well curved, without a handle, in some respects more easy to manage than the ordinary needle in this operation. The thrust necessary to carry the common needle through the tissues (and to get a good hold it is absolutely necessary to make a deep thrust) to the cut edge, and then beyond it and through the opposite side is very considerable, and cannot be effected without a certain violence. By aid of a good forceps, a shorter needle without a handle can be got easily through and brought out on the opposite side. Fig. 1 represents the form of needle I recommend. It may be useful to note that it is very easy to curve these needles into the various forms required for operations on these parts by gently heating them in the flame of a gas lamp. They can be afterwards tempered by plunging them when hot into a little sweet oil or water; but the latter requires care and judgment, or the needle becomes very brittle.

The flat ribbon of wire being passed through (two or three are generally necessary), is secured in a very simple manner by the peculiar arrangement of the quill which I have adapted to it. This is shown in Fig. 2. I have made this for my own use in the following manner:—A piece of stout silver wire, of the shape called "half-round," which may be procured at any good tool warehouse, is straightened by a few heavy blows on a flat surface, and then cut into suitable lengths with a file or metal saw. Place two pieces of equal length, say two inches, with their flat surfaces almost in contact, and then with an ordinary soldering tool run a little fine solder for an eighth of an inch down the two ends—no further. In this way we have produced a

Fig. 2.

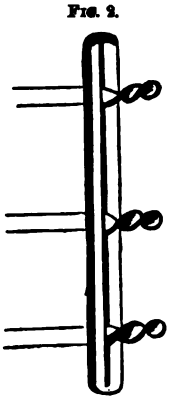


Fig. 3.

metal quill with a fine slit running through it, but closed at the ends. The flat ribbon is passed through this slit (formed by the contact of the opposite flat surfaces of the wire), and it may then be made perfectly fast by the simple device of giving it one or two twists; thus throwing the flat surface of the ribbon transversely across the slit, and making it impossible to pull it through or get it loose. This is done, of course, on both sides, and when the three are secured we have as perfect a suture as can be imagined. For the flat ribbon produces no suppurating tract like the cord or silk, nor does it cut through the tissues like silver wire. If the ligatures be too tight, or the parts swell much, it may be eased by simply untwisting it by a turn or two, and it can be then made just as secure again; or if it be too loose, it may with the same facility be tightened. And by having a little lateral movement up and down the slit, it has a self-adjusting tendency which is certainly not without its value. The great convenience and cleanliness of this method of operating is one of its chief recommendations, and it has this advantage in addition, that there is not the same necessity for removing

the deep ligatures so quickly as when they are of silk or cord.* A few interrupted sutures of fine silver wire are then necessary to bring together the outer margin of the wound, and these should not be carried too superficially.

I consider it quite unnecessary to divide the sphincter ani for this operation as a general rule, it being important to reduce the action of the surgeon to its smallest limits.

The after-treatment is the same in its general principles as that for rupture of the perineum. But I believe a grain of opium, at first twice a day, and afterwards every night, to be ordinarily sufficient; and I do not by any means approve of the system of drowning the patient with wine except under very special circumstances. Neither do I find it necessary or expedient to have the bladder emptied oftener than three times in the twenty-four hours. The opium checks the secretion of urine, and there is therefore less occasion for drawing it off. Besides this, the parts are much better left undisturbed as long as possible, especially at first. I prefer using the catheter myself to entrusting it even to the best of nurses. I also greatly prefer washing the wound myself, and this is best done after catheterization. After the tenth or twelfth day the bowels may be opened by a dose of castor oil and an enema,

Before the patient is allowed to rise, astringent injections of alum and tannin should be used twice a day; and a firm bandage with a perineal pad is necessary to be worn for two or three months, until the cure is in fact consolidated. Sometimes a small metallic pessary—the folding wire kind is best—will support the uterus conveniently until the newly-joined structures have become quite firm.—*Lancet, Dec.* 9, 1865, *p.* 645,

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88.—ON THE OPERATION FOR THE RADICAL CURE OF PROLAPSUS UTERI, BY CAUSTIC APPLICATIONS.

By Dr. C. H. F. Routh, Physician to the Samaritan Hospital for Diseases of Women and Children, &c.

The operations practised in the intention of mechanically preventing the descent of the womb may be spoken of as of three kinds:—1st. *Kolporaphy*, where there are confined to portions of the vagina, slips of which are removed either by the knife, caustics, or the actual cautery. 2nd. *Episioraphy*, where portions of external skin of the labia or perineum, as well as more or less mucous membrane, are removed. 3rd, M. Huguier's operation where mucous membrane and a portion of the uterus itself is removed.

The following cases are examples of episioraphy, only strong caustics were applied, and when the sloughs had come away other parts were brought together by quill sutures.

The caustic I used was strong nitric acid. In this there is nothing new, as it was used by Benjamin Phillips in 1839. The nitrate of mercury having been used by Langier in the same year in France.

The quill sutures usually employed by Dr. Davidson in 1838, Gedding in 1839, I. B. Brown, Dr. Savage, and a host of English operators that have followed in their track during late years, when used with ordinary string, have the advantage of being very easily applied or removed, and the amount of tension to bring the edge together can be easily regulated. But they have one disadvantage, and it is, that if kept longer in than the second day, they make a very large lateral hole in the side of the perineum, which is much enlarged by the sloughing which invariably succeeds. So far, convalescence is delayed. To prevent this result, some have contrived to place between the quill and the skin a thin metallic plate, with a narrow slit along its centre, so as to keep the two parts of the thread close together. This is decidedly an improvement, and there is no doubt that, if the ordinary ligature

* Mr. Hilliard, of Glasgow, has taken some pains in preparing the flat ribbon for me and the needles and quills; but I prefer my own home-made, ruder tools, since they can be adapted to the differing requirements of different cases.

thread is used, it is the better plan. To obviate the inconvenience of thread quill sutures, some years back, with the assistance of Mr. Coxeter, I devised two metallic clamps, so constructed that wire may be used instead of thread, and, by turn of a key, secured *in situ*. The wire used in this manner may be single, and thus the hole made through the perineum is small. The wire is introduced through a circular needle, or it may be used singly without a needle if one point of it is tempered in steel and made very sharp. This last plan has been tried by some with success, although, in such cases, they do not use the clamp, but merely bend the wire after having pressed it through an ordinary wax bougie. The advantage of the single wire is that it may be kept in longer: thus I seldom remove it till the beginning of the fourth day. By the seventh or eighth the superficial may be safely removed. I may remark that, in the usual plastic operation by the knife, when putting on the interrupted wire sutures in the parts anterior to the clamp, I put them on deeply too, only just anterior to the clamp: a plan which, I believe, was first practised by Mr. Brown.

I was led to use caustics because I had felt there were a class of cases in which the knife was dangerous, and, although I now find I was anticipated by Mr. Wells in 1858, my first experiment was at least undertaken in total ignorance of his previous experience.

The operation, although a simple one, is always accompanied with a great loss of blood, and it may occur that owing to the patient's age, the varicose state of the veins about the parts, and general debility of the patient, the loss of blood, or the use of chloroform, may become dangerous contingencies. In these cases, the first part of the operation may be performed by caustics, and the cure is as complete in the end, although, possibly, not so speedy.

[The patient in the following case was 72 years of age, and in very poor health.]

The patient being very weak, and the aspect of the parts leading to the impression that there would be a good deal of hemorrhage if the ordinary plastic operation was performed, I preferred the employment of caustics. The womb and parts being replaced, the strong nitric acid was painted over a portion of the external skin extending upwards over the mucous membrane of the vagina, about 1½ inches upwards, and for the lower, half of the external opening. No chloroform was given. The pain at first was considerable, but entirely subsided after four hours. Poultices were applied every three hours, and a suppository of opium ordered if the pain continued.

From this time up to May 3, she appeared to be doing well. On inspection, however, the separation of the sloughs was found to be proceeding very slowly. The bowels were constipated, and there was some abdominal pain. She was ordered a purge. On inquiry it was found that the nurse had neglected the application of the poultices. These were now ordered to be regularly applied both within and without the vagina. On the 5th the slough had not come away, and two or three spots were again touched by nitric acid. On the 9th the sloughs had completely separated. The granulations being well washed by a stream of warm water, the parts were brought together by clamps, and two deep sutures applied. A catheter was left in the bladder. About half an ounce of blood only was lost by the introduction of the deep sutures. No superficial sutures were applied. In the course of the 11th some sickness supervened, which was relieved by creosote and salines. There was considerable vesical irritation, which ceased on the removal of the catheter. The deep sutures and clamps were removed on the 12th. The union was complete and the perineal cushion very good.

Some days subsequently (18th), the bowels having been kept constipated, a sort of diarrhœa supervened, and in the passage of a large fæcal mass she felt as if something had given way, and a small quantity of blood passed out. This was at the time attributed to piles. On the 19th, however, it was found that a great portion of the new perineum had given way. The operation was therefore performed a second time. The result of this second operation was much more satisfactory, union again taking place posteriorly, and only deficient a little anteriorly. This point had again to be touched by nitric acid

and brought on the separation of the slough by stitches. This case ultimately did very well, the uterus receding backwards, and the vulva opening smaller, contracting gradually independently of the new perineum.

This contracting effect of caustics at this part has not, I believe, been insisted upon before, and I make no doubt that had superficial sutures been applied as well as the quill, the case would have done well after the first operation.

I am informed by Mr. Spencer Wells of two cases in which he had used caustics for the purpose of forming a granulating surface, which, when brought into apposition, might unite as readily as the raw surfaces usually made by the knife. These cases occurred in private practice in the spring of 1858, the caustic being used because the ladies were very unwilling to submit to any operation requiring the presence of assistants, and were fearful of the knife. In one case nitric acid was used, and the granulating surfaces, after the separation of the slough, were brought together by three silver sutures. Union took place, but it was very superficial, and only served to keep up the prolapse by the assistance of a T-bandage and perineal pad. In the other case, the acid nitrate of mercury was used, a horse-shoe piece, an inch and a half in depth destroyed, and poultices used until the slough separated on the sixth day. On the seventh day the granulating surfaces were brought together by the quilled suture, and a very firm thick cushion was obtained. Mr. Wells has informed me that he had heard very lately from this patient, and that the result has been most satisfactory, a prolapse of twelve years' standing having been completely retained within the vulva. He did not use chloroform in either case, but applied Dr. Arnott's frigorific mixture before the acid. In both cases there was a good deal of pain for two or three hours after the application.

The employment of caustics in these cases prove it has some advantages:—

1. It needs a smaller staff of assistants.
2. It precludes a loss of blood.
3. It causes contraction of the parts around the vulva, and relieves the uterine congestion.

1. In hospitals, where we have proper tables, dressers, nurses, &c., in abundance, the disadvantage of few or many attendants is not so much felt, but in private practice it is not so. Ladies do not like to exhibit to several men. Here one attendant suffices, and even, if desired, none but the operator need be present.

2. It precludes the loss of blood; and this is really a matter of great importance. The state of weakness to which many patients after the operation are reduced is very great. The blanched look of the women for several days after it is painful to contemplate. Few practitioners have operated by the knife upon female organs of generation without being often surprised at the great amount of hemorrhage which will often follow the slightest cut of the knife. Nor is this all. Secondary hemorrhage after this operation is not rare, and this in cases where the known skill of the operator left no doubt as to the accurate manner in which it had been performed. Many of those weak females with flabby hearts before alluded to, as cases in which the administration of chloroform is dangerous, are precisely those in which loss of blood is so injurious. Hence one of the reasons why recovery in these instances is often so slow, even after union is completed. The expense, moreover, of keeping up such patients by wine is felt to be a heavy tax on some hospitals. But there are, besides, cases in which operation by the knife would be exceedingly hazardous. One such case, I remember, some time back, and in which, upon consultation, all idea of operation was in consequence abandoned. It was the excessively varicose condition of all the veins about the vulva and upper part of thighs. True, this state of things might have been in some measure relieved by drastic purgatives, but even then the hemorrhage which might have followed during the operation seemed entirely to preclude the attempt. Aged patients, also, cannot afford to lose blood, particularly if weak and long ill-fed, as my patient had been.

3. The application of the caustics, even though it be not actual cautery,

has the tendency to cause all the parts around to contract. It is my belief that the mere application of a strong caustic so as to destroy deeply, would in time so contract the opening that it would not be necessary to bring the parts together by sutures at all. However, in great looseness of the parts, it appears more prudent to use them, and the opinion just expressed requires direct experiment to prove it.

There were some disadvantages met with in the three cases, which require to be noted. 1st. The pain endured by the patients for three or four hours after the application. This is, of course, an objection, but measures could be taken to overcome it by opiates. 2nd. In two out of my three cases a small sinus existed at the lower portion. I am at a loss to understand why it should occur here, but as it was not observed in Mr. Wells's two cases and my first, it was probably accidental. At any rate this is a mishap readily rectified, as it was in my second case. But if the edges are paired and brought together by wire ligatures, the hemorrhage is inconsiderable, and the cure certain. I prefer not bringing the edges laterally, but from before backwards, the threads being passed from the rectum externally. In one very bad case in which I operated and in which the entire external sphincter and part of the internal were lacerated the plan succeeded perfectly. We should remember that the disposition of parts is to this kind of union, and it is remarkable how, after a time the sphincters seem to acquire strength and greater power. One or two more practical hints, and I have said all I wish to say on this operation.

1. As to the keeping the catheter within the bladder, I think it unnecessary, if the top of the wound is kept guarded by a piece of lint the urine will do little harm, the cut edges being brought together by apposition and not exposed to the contact of urine; however, it avoids all chance of cystitis, so common and so distressing a symptom after the operation, when the catheter is kept in.

2. The vagina should from the first be frequently washed out with cold water; I like it done three or four times in the day, and at night if need be, and especially after the patient makes water. The operation is very grateful to the patients, and keeps the parts always sweet and free from much pain. The ordinary chlorate of potash and lavender lotion is the best I know.

3. I think it wise to remove the deep sutures by the third or fourth day at the least, the superficial should, I think be removed about the seventh or eighth, but as a precautionary measure the superficial should be kept in until the bowels have been opened by some laxative like pulv. jalap, co., or bitartrate of potash, which produce watery movements, and preceded by an injection of water. I think, also, it is a great improvement of Mr. Brown's to apply the superficial sutures deeper.

Lastly, admitting the disadvantage of sloughs formed by the caustic applied, and the irritation set up by the discharges from the large ulcer formed for the time being, and often so difficult in this situation to heal when once made, this very ulceration has often a very great and favourable influence on uterine congestion. I believe this result obtains when the caustic is used in the perineal operation in lieu of the knife more effectually on the more neighbouring parts. Then once the perineum has been formed and the two sides brought together into apposition by quilled sutures the uterus must needs become absorbed, because the two causes before referred to, the invaginating action, and the greater weight pulling down at the vaginal end, and from filled bladder, &c., no longer exist. Other reasons have been mentioned by other authors as co-operative, but as these will be found referred to in text books and Dr. Savage's new work on the "Female Perineal Organs," I do not refer to them here.

In conclusion, I may say I do not propose the use of caustics as if I wished them to supersede the ordinary plastic operation, which I have practised and will still continue to do so in most cases, but only in the special instances referred to in this paper when the use of the knife is contra-indicated.—*Medical Mirror, March, 1866, p. 121.*

89.—CASES OF POLYPUS UTERI: WITH REMARKS.

By Dr. J. G. Swayne, Physician-Accoucheur to the Bristol General Hospital, and Lecturer on Midwifery at the Bristol Medical School.

[The two following cases of polypus uteri are similar in many respects, and are worthy of notice; because in both the polypi were larger than usual, and it was, in consequence, found necessary to modify the operation proposed for their removal.

Case 1.—On May 18th, 1865, Mr. Parsons requested me to see an unmarried lady, Miss P., of about 38 years of age, whom he had been attending for six weeks. He stated that she had had a tendency to menorrhagia for the last two years, but that he was first consulted on account of a rather profuse hemorrhage, followed by a copious and very fœtid discharge, from the vagina, and accompanied with some pain and aching in the loins. On examining, he detected a large tumour in the vagina, which he was inclined to consider a cauliflower excrescence; but, as he did not feel quite satisfied on that point, he requested a consultation with me about it.

When I first saw her, her general health did not appear to have suffered much, although she was rather pale and exsanguine in appearance. The discharge from the vagina was so offensive, that it rendered the room in which she was very disagreeable. On examining *per vaginam*, I felt, just within the os externum, a tumour, apparently of about the size of a large orange, filling the vagina and distending it considerably. The lower portion of the tumour was rather soft and irregular, as if shreds of it were partially detached and in a state of decomposition; but it had not the peculiar soft spongy feel of cauliflower excrescence, nor did it bleed so readily on being examined. When viewed through the speculum, its surface appeared of an ashy gray colour, and not bright red like a cauliflower excrescence. On passing my finger up as far as I could reach around it, I was able to feel the pedicle of the tumour, which seemed to be about as thick as my middle finger, and more firm and fibrous than the rest of the tumour.

After a short consultation, we agreed that it was a large polypus growing from the os uteri, and we decided on an operation for its removal.

This I accordingly performed on May 23rd. The patient being placed on a firm table and lying on her back in the lithotomy position, I endeavoured for some time to draw down the tumour beyond the os externum in order to excise it; but the vaginal orifice was rather narrow, and although I made repeated efforts both with a vulsellum and an ordinary obstetric short forceps, I could not bring the polypus out beyond the os externum without using more force than I thought justifiable. The tumour also gave way in two or three places, and in consequence of this a very copious hemorrhage ensued. At least a pint of blood was caught in a basin, besides a good deal which saturated her clothes and ran on the table. I therefore, as she was becoming faint, thought it prudent to desist from further efforts; and contented myself instead with passing a firm ligature of whipcord round the neck of the polypus by means of Gooch's double cannula. As soon as the ligature was tightened, the hemorrhage entirely ceased.

When we visited her the next day, the polypus did not feel so firm as before; and, by getting three or four fingers of the left hand behind it, I succeeded in dragging it completely out. The double cannula and whipcord were then removed; and, instead of it, I passed a needle armed with a double thread of strong silk through the pedicle. This was tied on each side, and the pedicle cut through close to the ligature. No unfavourable symptom followed the operation.

On examining with the speculum two days afterwards, the ligature was observed to have come away, together with the remains of the pedicle, and to be lying loose in the vagina.

The patient made a good recovery.

Case 2.—On August 7th, 1862, Dr. Crocker requested me to see with him an unmarried lady (Miss H.), aged 40, who had been under his care for some

weeks. She had been suffering from repeated hemorrhages from the vagina, which had brought her into a very weak state; and also from retention of urine, necessitating the daily use of the catheter.

On making a vaginal examination, I felt, just within the os externum and pressing very much on the meatus urinarius, a large ovoid tumour, which filled the vagina, and presented most of the characteristics of an uterine polypus. It was firm and fibrous to the touch, and bled freely when roughly handled. I could not reach any pedicle with my finger; but by using the uterine-sound and passing it round it, I could make out that it was attached to the uterus by a narrow stalk. The discharges from the vagina were not offensive, and no kind of decomposition seemed to be going on in the tumour. As the tumour was evidently a polypus, we at once decided on its removal.

This I proceeded to do on August 21st. Having a vivid recollection of the hemorrhage which followed my attempts to draw down the polypus in the former case, I began by placing a ligature round the pedicle by Gooch's double cannula. When the ligature was tightened, it appeared, in a great measure, to control the hemorrhage from the tumour. I then applied a short obstetric forceps, and having obtained with it a firm hold of the tumour, I made several endeavours to draw it beyond the os externum; but I was completely foiled in my attempts by the contracted state of the entrance of the vagina, which I felt that I could not overcome without undue violence. I therefore tightened the ligature, and left her.

I saw her next day, and again tightened the ligature. The tumour had now become less firm and resisting; and from the offensive smell of the discharges, appeared to be undergoing rapid decomposition; but yet I found it impracticable to remove it.

On the day following, the whipcord broke whilst I was tightening the ligature. I then made several attempts to draw the tumour out by the obstetric forceps; but still failed, owing to the great resistance of the os externum. No hemorrhage, however, resulted; but so much soreness of the parts was occasioned thereby, that she begged me not to make any further attempts for some days. I therefore merely directed her to inject twice a day into the vagina about a pint of water containing Condy's solution.

On the 29th (eight days after the ligature was first applied), whilst using this injection, a powerful bearing-down effort was induced by the stimulus of the fluid, and the tumour was suddenly expelled into the chamber-utensil. It was large and pear-shaped, and appeared to have been attached by a rather narrow pedicle.

I saw her again on September 1st. She was then apparently going on well. I examined her with the speculum, and observed a small remnant of the pedicle just within the os uteri. This I touched freely with nitrate of silver.

About a week after the polypus came away, she had an attack of phlegmasia dolens in one leg, apparently in consequence of the absorption of the putrid discharges. The attack was not severe, and soon passed away under the usual treatment. With this exception, she made a good recovery.

Remarks.—There are two or three points in these cases which are worthy of notice, on account of the bearing which they have upon the pathology and treatment of polypus uteri. The source of the repeated hemorrhages which characterise this disease has long been a *vexata quæstio;* some being of opinion that the bleeding proceeded from the uterine mucous membrane, whilst others have thought that it came from the polypus itself. The history of the two cases above related would seem to favour the latter hypothesis; because, in both, copious hemorrhage ensued when the surface of the polypus was torn or even roughly handled, and this was at once arrested when a ligature was applied round the pedicle; nor could it be re-excited by any handling or dragging of the tumour immediately afterwards. Such an effect is too instantaneous to be accounted for (as has been stated by Dr. West), on the supposition that "the ligature interrupts the vital relations between the tumour and the wound, and thus renders the polypus a far less powerful excitant of the uterine mucous membrane than it was before." On the other hand, the amount of blood which flows at various times from the uterus seems to bear

no relation to the size or vascularity of the polypus, as it ought to do if it proceeded entirely from it and not from the uterine mucous membrane. In a case, for instance, where I removed by excision a polypus not larger than a nut, the hemorrhage previously had been quite as great, if not greater, than in either of the two cases I have related above, and yet the excision was unattended with any bleeding. On the whole, I am inclined to think that the hemorrhage may proceed from both these sources, the quantity furnished by each depending very much upon their respective vascularity and a variety of other circumstances.

With respect to the operation required for the removal of polypi, the ligature was at one time very generally adopted for this purpose; but it has of late years fallen, I think, very deservedly into disrepute, excision being now more frequently adopted. The old idea, that the removal by ligature is a much safer operation, has been effectually disproved by facts. The removal by ligature is a tedious operation, for frequently several days elapse before the tumour sloughs off; it is a dirty operation, for, all the time the tumour is decaying, a horribly offensive discharge is issuing from the vagina; and it is a dangerous operation, because the putrid *débris* of the decaying polypus may be absorbed and set up uterine phlebitis and peritonitis. I shall not readily forget a case of this kind which occurred some years ago at the Bristol General Hospital. A woman was admitted who had a polypus, of about the size of a hen's egg, growing from the uterus. A ligature was placed around the pedicle by Gooch's double cannula; and the operation was so effectual, that the polypus came away in three days. But, before a week had elapsed, the woman died of metro-peritonitis. The danger of this accident is, I believe, much greater when the polypus is attached, as it was in this instance, to the mucous membrane lining the cavity of the uterus, than when it grows from the lips of the os. Many such untoward events have occurred; but, as Dr. West well remarks, "there is nothing that places the dangers of this operation in so strong a light as the fact that, out of twenty cases of removal of fibrous polypi by ligature, recorded by a most strenuous defender of that operation, Dr. R. Lee, nine, or almost half, had a fatal result—a mortality more than double that of the operation of lithotomy, as high as that which occurs in placentia prævia, and higher than the mortality from malignant cholera."

In the two cases described this evening, I was compelled by circumstances to employ the ligature; but in one of them I was able to excise the tumour on the next day, whilst in the other I found it impossible to do so; and the risk which the patient incurred in this instance is shown by the attack of phlegmasia dolens which supervened.

The operation of excision, which is in every other respect so preferable to the ligature, has been objected to on account of the risk of hemorrhage which attends it—without, as Dr. West observes, any sufficient reason. He states that, according to the experience of Velpeau, Lisfranc, and Dupuytren, such an accident is very rare; and that, in his own experience, twelve cases of excision were unattended with any hemorrhage whatever. My own experience does not prove that the operation is quite so free from danger in this respect, although it is very limited, as I have excised polypi in but seven cases, yet in one of these considerable hemorrhage ensued. I removed *in situ*, by a pair of curved scissors, a small soft polypus, not thicker than the first joint of the forefinger, and was astonished by observing a copious gush of blood from the vagina immediately afterwards. To arrest this, I introduced a large speculum, in order that I might cauterise the pedicle; but found it quite impossible to do so, as the speculum, in a very short time, became quite filled with blood. I therefore introduced a large sponge coated with gallic acid, and firmly plugged the vagina. This completely checked the hemorrhage, but not before rather alarming syncope had ensued. On examining the polypus, I found that it was supplied with blood by an artery quite equal in size to one of the digitals.

In the first case which I read this evening, an examination of the polypus showed that it was fed by an artery quite as large as the radial at the wrist; and in this instance, no doubt, simple excision would have been followed by alarming hemorrhage.

On account of the risk of hemorrhage, it is better, in all cases where it can be applied, to remove a polypus by the écraseur, rather than by the knife or scissors; and this, could it have been adopted, would have been by far the best operation in the two cases above mentioned. But, in consequence of the large size of the polypi and the contracted state of the vaginal orifice in both cases, I was unable to get at the pedicle so as to use the écraseur or even the curved scissors; and I therefore very reluctantly used the ligature and excision in one case, and the ligature alone in the other.

[Mr. GEORGE MAY, Jun., of Reading, writes in the next number of the British Medical Journal as follows:]

I am induced by the report of Dr. Swayne's Cases of Polypus Uteri, to remind your readers that the contracted vagina in the unmarried female may be rapidly and safely expanded by Barnes's dilators sufficiently to admit the hand. The patient should be under the influence of chloroform.—*British Medical Journal, Jan. 20 and 27, 1866, pp. 69, 112.*

90.—ON A NEW UTERINE SUPPORT.

By Dr. WILLIAM MAIN, Lasswade.

[Perhaps the most generally useful appliance yet known for the relief of displacements of the uterus is Dr. Simpson's loop pessary of gutta-percha. Yet there are many cases in which that instrument affords little or no relief, though fitted with all the care possible. The instrument here described is perhaps an improvement on Dr. Simpson's.]

FIG. I.—Side View of Uterine Support for Retroversion.

It consists of a circle of gutta-percha, enclosing a copper wire of No. 12 thickness, fitted to receive the os and vaginal portion of the cervix uteri. Connected with this circle is a stem of the same copper wire, a continuation of that enclosed in the gutta-percha, which terminates in a sliding loop. The instrument is very simple, is easily made, and does not cost more than two or three pence. Its application is also very easy. A bandage round the loins, with thigh straps, having first been adjusted, a sound is introduced into the uterus, and its position rectified. The gutta-percha circle is then passed over the sound, which acts as a guide, and is pressed gently upwards till it receives the os and cervix. The stem is then bent to fit the vagina, and, being sustained there by the fingers of the left hand, the remaining portion is carried forwards to the upper edge of the mons veneris. The running limb of the terminal loop is now brought to this point, and secured to the main stem by a piece of tape. It now only remains to bend the loop into two semi-cir-

cular horns, one to run along the lower part of the abdomen on either side, immediately above the groins, and fasten these horns to the abdominal bandage by tapes or stout pins. The instrument is now secure, and, when properly applied, it has never in my hands given rise to pain or irritation; but, on the contrary, has invariably afforded relief to all the annoying uneasiness of a displaced uterus, and permits of any movements, such as walking, sitting, or stooping, with perfect freedom.

FIG. II.—Front View of Uterine Support for Retroversion.

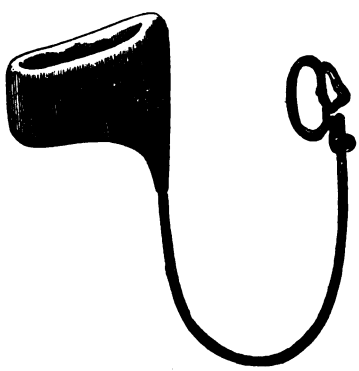


FIG. III.—Side View of Uterine Support for Cases of Anteversion.

I have had an opportunity of trying the instrument in a few cases of prolapsus, retroversion, and anteversion of the uterus, and have found it equally serviceable in all of these, a slight change in the form of the gutta-percha circle being all that is necessary to suit the different kinds of displacement. In cases of simple prolapsus, the upper margin should be horizontal. Again, in those of anteversion, the anterior part of the circle should have a raised cushion of gutta-percha, so as more effectually to support the uterus in its proper place; while in cases of retroversion, the cushion or bolster should be on the posterior part of the circle, with a similar object in view.

The instrument may be made large or small, deep or shallow, to suit the circumstances of each particular case; and should the patient be large or fat, a wire of greater thickness than No. 12 may be substituted with advantage.

I may also mention that I have found the copper wire frame, without the gutta-percha circle, very useful when using Dr. Simpson's galvanic pessaries.

By taking merely the loop and stem, and on the end of the latter fixing the galvanic pessary with a little heated gutta-percha, it can be retained perfectly in its position in the uterus, while, at the same time, that organ can be kept at any desired angle by bending the stem; the loop end of the wire being fixed to the bandage, as before suggested.

As an illustration of the usefulness of these instruments, I append a short account of two cases. First, a case of anteversion.

E. T., aged 24, a servant girl, had been incapacitated from work for some years in consequence of dysmenorrhœa, with stricture and anteversion of the uterus. Intra-uterine galvanic pessaries were introduced; but by no possible means could they be retained, the uterus expelling them as soon as the support of the finger was withdrawn. The gutta-percha platform support, india-rubber-balls, and a sponge, were all tried in vain. They failed to keep the pessary within the uterus. Dr. Simpson's wire pessary was now tried, and it gave complete relief; but after a few weeks it caused so much irritation that I was obliged to withdraw it, when I found the uterus had become very much enlarged. In a short time the uterus was discovered to have resumed its former anteverted position, and I then began to use the loop pessary; but it gave little or no relief. Some modifications of the instrument I now show were afterwards tried, with encouraging results; and she has now been wearing one of the present form for some months, with complete relief. A day or two ago, I passed a sound, nearly straight, up to the fundus of the uterus.

How far she may ultimately recover it is impossible to say. At present, menstruation is not quite regular, and she suffers from frequent disordered function of the liver, but can walk about with perfect ease and comfort.

The next case which I shall relate is one of retroversion.

M. C., a young woman, 25 years of age, had suffered severely from dyspepsia, for seven years before coming under my care,—vomiting a large quantity of mucus, and a small portion of her food almost daily, for nearly all that period, according to her own and her mother's statement. Her menstruation was irregular, painful, and not free; and she suffered from pain and weakness in the back, along with other uneasy symptoms which usually accompany this condition. After careful inquiry, I came to the conclusion that the uterus was the source of her distress, and warned her friends that local treatment would most probably be required, but that, in the mean time, medicine could be tried. Bismuth, nitrate of silver, nux vomica, lime-water, &c., were tried, with temporary good effects, but at every menstrual period all her symptoms were greatly aggravated. On examination, per vaginam, the uterus was found retroverted, and the passage somewhat constricted. The introduction of a galvanic pessary greatly relieved her dyspeptic symptoms within twenty-four hours, though a little local irritation was experienced. Under the use of the pessary for a short time, and without medicine of any kind, menstruation became quite regular and natural, and the functions of the stomach were completely regained. I now thought we could possibly dispense with the pessaries altogether, and withdrew the one she wore; but within twenty-four hours vomiting returned as bad as before, accompanied by all her other distressing symptoms. The gutta-percha loop pessary was now tried, but with only very partial relief; then some modifications of the present instruments, which were found to give relief so long as they retained their proper position. On becoming displaced or withdrawn, however, the vomiting almost immediately returned. She has now been wearing a pessary similar to the one I have shown you for about four months, with perfect comfort, and with relief to all her symptoms. She can walk any ordinary distance, work in the garden, stoop, &c., with ease, and has grown comparatively stout and ruddy on good food, air, iron, and cod-liver oil; and my only anxiety now is to find whether she will ultimately be able to do without the pessary. At present, I fear, she cannot; for about a month ago I withdrew it, in the hope that she would remain well; but I was disappointed to find that the vomiting returned in two or three days, and continued to recur as usual till the instrument was replaced.—*Edinburgh Med Journal, Dec.*, 1865, *p.* 545.

91.—ON EXTREME FLEXIONS OF THE UTERUS.

By Dr. Henry G. Wright, Physician to the Samaritan Hospital for Women.

The distinction of those uterine deviations formerly comprised under the general title of ante- or retro-version into true version (where the whole organ is bent like a pear on its stalk) and of flexion (where the bend is in or above the uterine cervix) has come to be very generally accepted, and is not without its practical utility.

It is to the latter class of cases, where there exists some considerable alteration of the natural angle which the cervix uteri forms with the body of the organ, that the following remarks are restricted; and more especially to those extreme conditions of flexion where the uterus is, to a greater or less extent, folded on itself. Where this displacement occurs backwards (its most frequent direction), the facilities for physical examination presented by the prolongation of the vaginal cul-de-sac behind the cervix allow of its more ready recognition and replacement. Notwithstanding the advantage thus afforded, the diagnosis should be very carefully formed; for there occasionally occur in this situation tumours which present to the touch characters very like those conveyed by a retorted fundus uteri. But with ordinary care, there is rarely any difficulty in arriving at a correct diagnosis.

Retroflexion of the uterus in the utmost degree—with the fundus forced down in the fossa of Douglas and the cavity of the organ bent like a hook—may be considered as a type of extreme intrinsic displacement. In such cases the cervix may but slightly deviate from its normal situation, or even occupy its natural position relative to the vagina; yet the protruding posterior lip of the os uteri, and the elongated linear orifice, curved with its convexity forwards, should at once induce suspicion that there has ensued some disarrangement of the natural order of uterine place.

The suggestion that a uterus so extremely displaced (and affording distinct etiological evidence of results constantly met with only where it is thus distorted) should be treated as for some supposed inflammation, without regard to the deviation of place, is, I firmly believe, just as unreasonable as it would be to treat a strangulated hernia by the application of leeches, without attempting any reposition of the bowel.

The use of some intra-uterine support indubitably owed its origin to the proved inefficiency of ordinary methods of treatment in certain extreme cases of uterine versions and flexions. In the latter cases the fundus could be raised by careful manipulation, the cervix brought well backwards, and the uterus—long unnaturally distorted—be thus replaced, with immediate relief to the patient. But too often this good result was only of temporary duration. The fundus gradually rose and reclined again, and the urging of intestinal action caused it to resume its vicious position. There would thus be successively brought about three distinct conditions of mechanical displacement. 1. The whole organ is first turned on its axis, producing a partial retroversion. Then there gradually ensue the stages of deviation, which are ranged under the head of flexions—viz.: 2. A cornuation of the uterus, the organ being unnaturally *curved* (most frequent in cases of anteflexion). 3. The true flexion where the body is bent on the cervix at a sharp angle. The first stage—that of retroversion—is not commonly met with where the neck is empty, since the conditions under which it most frequently happens are just those which lead the patient to delay seeking advice. The following case illustrates such a state:—

Mrs. ——, aged 27, confined a year, suckling the child for only six months. After the first month she began to move about; but felt so much weight and forcing in the body that she was glad to take her couch again. Carriage exercise increased her suffering, until she had to give up every attempt at exertion, since it induced such increase of pain. The action of the bowels always produced suffering. Latterly some blood usually passed with the motions, and she came to consult me in the belief that her sufferings were due to internal piles. The uterus was large and soft, but not tender —just in that condition so often found after a difficult labour, where the

subsequent involution has been inefficiently effected. The cervix was low down in the vagina and the organ directed somewhat backwards, with a slight reclination of its fundus. Reposition by means of the uterine sound (which entered to a depth of nearly three inches and a half) gave immediate relief. There was no recurrence of the displacement or of the bleeding; the patient became again pregnant, and has felt no further inconvenience.

In even more advanced reclination, if careful replacement be effected just prior to a menstrual period, and great caution exercised throughout that time, permanent relief may sometimes result. For the reparation of uterine tissue, which is determined during each catamenial period, supplies a valuable auxiliary in the treatment of uterine disorders of all kinds, and always requires to be held in mind.

Moreover, in these extreme cases of retroflexion, even where the displacement recurs again and again, it often happens that a cure may be effected by extra-uterine supports—setting aside the necessity for resorting to any intra-uterine appliance, especially in cases where some neoplastic thickening of the posterior uterine wall induces retroflexion by its weight, and menorrhagia by its presence. Here reposition, followed by the adaptation of a fully inflated air-ball pessary, as advised by Valleix, in order to sustain the uterus high up in the pelvis, mechanically prevents recurrence of the displacement, whilst appropriate treatment is pursued for restoring the general health. Notably this was the result in the following cases, which serve also to illustrate the very diverse physical conditions which may produce the same disorder of uterine place.

1. A widow lady, aged 46, of healthy family, strong constitution, and full habit, suffered from such extreme menorrhagia and urging dragging back-pain that she had been almost entirely confined to her couch for three years. The original cause was clearly traced to sudden check of menstruation by exertion and night exposure during a fire. There followed gradually increasing dysmenorrhœa and menorrhagia, her sufferings greatly increasing at the menstrual periods, which were both too frequent and too free. The recto-uterine space was occupied by a rounded tumour, which, by using the sound, was distinguished to be the thickened posterior wall of the retroflexed uterus. This was carefully replaced, and a large air-ball adapted as high up as possible and fully inflated. Great local relief ensued; the health gradually improved under general treatment, though the progress was but slow. The patient has now been upwards of a year without any instrument or recurrence of the suffering, and walks and works and makes long journeys without discomfort.

2. E. D., aged 28, married, very thin and debilitated. A week after birth of her last child (in India, five years ago), she left her bed to attend her husband through a severe illness. Has ever since suffered from dysmenorrhœa and symptoms of uterine displacement; being unequal to any exertion, and presenting the usual complications met with in Indian cases—leucorrhœa, hepatic congestion, constipation, severe sympathetic headaches, enfeebled digestion, oxaluria, &c. The fundus uteri was felt low down behind the cervix, and carefully replaced. The long enfeebled and attenuated organ measured three inches and a quarter, and tended to fall forwards, just as it had previously bent backwards. Here the ball-pessary sufficed to maintain position, whilst the general health was improved by tonic treatment. Two months after, she could walk three miles without inconvenience, the pessary having been left off after the first menstrual period had passed.

But there occasionally come under notice cases where no extra-uterine appliance does any good, and where the ingenious curved supports devised by Dr. Hodge of Philadelphia, and modified by Drs. Priestly and Marion Sims, cannot be borne because of the irritability of the peritoneal pouch that descends behind the uterus; and where the numerous other ingenious devices recommended prove equally inefficacious, the replaced fundus returning to its retroflexed position, and all the old suffering coming back to the patient, on any effort or exertion. It was the occurrence of such desperate cases which undoubtedly led Amussat, Velpeau, and Simpson, to resort to the use of some

intra-uterine means of support, as affording a fair probability of relief. Of the instruments devised, that of Sir James Simpson is the only one worthy of note. Its success in his hands at once attracted attention, and the intra-uterine stalk-pessary was extensively tried. That in a considerable number of cases there ensued very serious results—metritis, pelvic cellulitis, peritonitis, and death, all attributed to its use—cannot be doubted. And individual writers in this country have joined chorus with the speakers at the Imperial Academy of Medicine who so vehemently denounced the use of the instrument during the discussion by that learned body of the use of intra-uterine pessaries. But it must be remembered that Dr. Simpson recorded the results of his own practice without making himself responsible for the results which might arise from defective application of the instrument; and even where its use is most vehemently condemned, there is no attempt to explain the cause of its failure—to ascertain whether it be the stem-pessary that is in fault, or the principle of intra-uterine support that is erroneous, or the operator that is to blame.

The evil results which have indubitably ensued in many cases where instrumental intra-uterine support has been attempted are, I believe, chiefly attributed to two causes:—

1st. The method of using the uterine sound ordinarily pursued to produce reposition is very liable to seriously hurt the uterus.

2d. A firmly supported intra-uterine stalked pessary must produce mischief unless used with very exceptional skill, and is calculated to perpetuate or increase any injury caused during the preliminary replacement of the retroflected uterus.

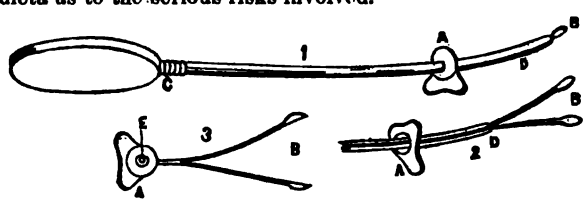
This latter proposition explains itself, when it is remembered that the uterus, whether healthy or diseased, is never absolutely at rest, moving downwards with every inspiration, and with every effort which induces descent of the diaphragm—as coughing, sneezing, laughing, &c. It is influenced as to its position by the condition of the adjacent organs, and by any strain thrown on them. It especially resents continued pressure against any point of its internal surface, and such local irritation is pretty certain to determine expulsive effort. If this effort fail, and strong, unyielding pressure on the delicate intra-uterine membrane be maintained, there ensue results ranging through that series of severe symptoms noted as following the employment of the intra-uterine pessary; for its due adjustment implies that an ivory or metal stalk lies within and holds up the womb; this being firmly maintained in position by a vaginal stem connected with a well-adapted external support. Thus the fundus of the replaced uterus is at every downward movement checked by the presence of an unyielding, inelastic intra-uterine stilette, the point of which always impinges on the same spot. The success of the instrument in the master-hand of Sir J. Y. Simpson has probably been to his so adjusting the length of the intra-uterine stem that it just sufficed to retain the uterus in position, without impinging against the fundus. But at best it must be a hazardous procedure, and the more so when it is remembered that there already exists an unhealthy structural condition in the majority of cases of chronic retroflexion.

Whichever be the kind of uterine displacement, however extreme its extent, and whatever the proposed plan of treatment, the first indication is to produce restoration of the natural position, if this be possible; for there occur cases where it would be eminently injudicious to attempt any interference, on account of adhesions having been formed, or from other causes. This preliminary replacement usually requires the employment of the uterine sound, and if not adroitly done may itself be productive of mischief, as already noted. The general directions given for the use of the instrument in case of extreme retroflexion seem very inadequate. The strongly curved sound is to be introduced until it passes to the depth of the uterine cavity and then turned or twisted on its axis until the fundus is moved forwards. Any one holding the instrument in his hand and thus rotating it, will at once perceive what a powerful leverage is exercised, and how the uterus must be dragged into a new condition of lateral deviation whilst undergoing the half-turn that

is to bring its concavity forwards, with great probability of injuring the delicate tissues pressed on.

The rule for reduction of all disorders of uterine place is that the organ shall be gently caused to retraverse the course originally passed through on its way from the natural to the unnatural position. In a case of extreme retroflexion, reposition may be readily accomplished according to the above rule. The patient is placed on her side, doubled together so that the body lies across the couch and the knees approach the chin. The curved sound introduced with the concavity backwards is gradually passed round the flexure to the full depth of the uterine cavity, whilst the forefinger of the free hand gently raises the fundus, helping, as it were, to fit the displaced part on the end of the instrument within the cavity, so that it may obtain full command of the organ. The next stage is that of replacement, sweeping the fundus uteri steadily upwards, forwards, and somewhat downwards. The intra-vaginal finger is shifted so as to press on the anterior part of the cervix, and prevent the instrument receding. Then the handle of the sound is carried gradually backwards until it lies within the fold of the nates, that portion of the instrument engaged in the cervix constituting the axis. Thus the point of the sound lifts the fundus uteri upwards and forwards. If now, retaining the same axis of movement, the handle be raised, and swept round in a semicircle until it comes down behind the thigh, there is thus acquired the power of continuing the tilting forwards of the fundus uteri until its natural position is attained, without causing any other deviation of place than originally occurred in the progress of displacement. When replacement has been thus accomplished without undue drag or the use of any force, there ensues almost immediate relief—so marked that I have ventured, at the risk of being tedious, to recount the method and meaning of an apparently very simple process.

The cases in which every ordinary method of replacement fails to prevent recurrence of flexion were just those in which trial was made of the stalked pessary of Sir J. Y. Simpson. When this was condemned as hazardous, and the reasons for its falling into disfavour were understood, there remained no other course than to leave these patients to their sufferings, condemned to dysmenorrhœa, sexually inapt and sterile, and unable to make any physical effort or endure the least exertion. I was painfully impressed as to the extent of hidden misery which women have to endure, often inexplicable to themselves, and more frequently of such kind that they do not choose to talk about it, by a case where the patient cheerfully professed her readiness to submit to wearing a stalked pessary, even after full explanation of the professional dicta as to the serious risks involved.



It was with full recognition of the difficulties and hazards attending the introduction of a foreign body within the uterus, so as to exercise mechanical influence on its position, that the instrument now to be described was devised. I consider it essential for an efficient intra-uterine pessary that it should not interfere with the movements of the organ itself, since these must go on. I recognised that only mischief could result from allowing the possibility of pressure against any one point of the intra-uterine surface; that any continued dilatation of the cervix would certainly induce expulsive action, and that the only way in which physical elasticity could be safely counteracted was by elastic resistance.

This instrument for assuring the continuance in place of a flexed uterus

resembles in appearance a short sound, having a guard-plate (A) at such distance from the extremity as to represent nearly the normal uterine length. After replacement of the uterus, the instrument is introduced just as an ordinary sound until the guard comes to touch against the cervix, when the smoothly-rounded end (B) lies free within the cavity of the uterus, the intervening part being bent to the natural curve of the organ. The finger retains the guard-plate *in situ*, whilst the cannula (C, D, Figs. 1 and 2) is withdrawn. The spring pessary expands, as shown in Figs. 2 and 3, lying along each lateral wall of the intra uterine cavity. The breadth of the spring prevents any antero-posterior flexion, and its elasticity antagonises expulsive effort, whilst the movements of the uterus, as a whole, are in no way restricted. The withdrawal of the instrument is accomplished by introducing the cannula until its point touches the hollow on the vaginal surface of the guard-plate. There is a slit on the back of the cannula which receives the metal bar connecting the spring with the guard-plate. Thence it readily glides into the cavity, and collapses the spring, as in Fig. 1. There is no need to employ the speculum either for introducing or withdrawal. It was my original intent that the springs should be plated with gold; but I find that the steel undergoes but slight superficial change, without any erosion of its smooth surface, even when retained for a fortnight. This is the longest time it has been continuously worn; but in this case it caused no inconvenience, and was only removed on account of impending menstruation.

This instrument, like all local appliances, whether mechanical, physical, or chemical, is but ancillary to, and may not be be allowed to supersede, the more important general therapeutical treatment required in all uterine complaints, whether these be lesion of structure, deviation of place, or disorder of function. The success of any new plan of treatment can only be fairly judged after the lapse of a very considerable period, and when the patient has for some time resumed the accustomed routine of a healthy woman's life. I purpose in a further communication recording the results of my own experience of the use of the instrument, of which I have here described the mechanism and purpose.—*Lancet*, *Feb.* 24, 1866, *p.* 198.

92.—A NEW UTERINE DILATOR.

By J. W. Hembrough, Resident Accoucheur at St. Bartholomew's Hospital.

[The uterine dilators at present in use are deficient in their action upon the internal os and upper part of the cervix.]

The accompanying engraving represents the instrument I have had constructed, the advantages of which will, I think, be sufficiently obvious to those who are acquainted with the dilatation now in general use.

Whether dilatation is required for introduction of a metrotome, sponge or laminaria tent, or stem, especially when we consider the almost universally strictured condition of the *internal* os in mechanical dysmenorrhœa, it is obvious that the *internal* os and upper part of the canal required to be dilated to at least an equal extent with the remainder.

The engraving represents the instrument open. It has three joints, a central (B) and two lateral (A and C). To the central one (B) is attached a rod (D), which is included within the two halves when the instrument is closed. By shortening the rod D, dilatation is accomplished to the extent required by turning the screw handle.—*Lancet*, *March* 3, 1866, *p.* 240.

93.—ON VAGINAL RECTOCELE.

By JAMES R. LANE, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mark's Hospital for Diseases of the Rectum.

It occasionally happens, when the perineum has been partially ruptured, or when, without any rupture, the perineum and vaginal outlet have been so much dilated, or have become so much relaxed, as not to afford an adequate support to the pelvic viscera, that dilatation of the rectum and posterior wall of the vagina is gradually induced, and these parts are protruded through the vulva in the form of a tumour, which is at first of small size, but which, if measures are not taken for its relief, may become in course of time as large as, or larger than, the fist. To this condition the term *vaginal rectocele* was applied by M. Malgaigne, in a paper published in 1838, in which he has described it in his usual complete and exhaustic manner. It had, however, been previously noticed by Sabatier, who pointed out the distinction between simple prolapse of the vagina, and the prolapse of the rectum with the vagina now under consideration. It was also well known to Sir Charles Clarke, who, in 1814, gave a clear and accurate description of it under the name of *procidentia vaginæ*, while, somewhat inconsistently, he called the analogous condition in which the bladder and anterior wall of the vagina are protruded, *procidentia vesicæ*. Such a nomenclature could not fail to cause confusion. It is much better, therefore, to adopt the terms proposed by Malgaigne, of vaginal rectocele for the one case, and vaginal cystocele for the other, and to restrict the term prolapse of the vagina to the descent of the vaginal membrane only, not involving the adjacent structures. This latter affection, though described by various surgical writers, is, I believe, of very rare occurrence, setting aside, of course, those cases in which it is associated with the descent of the uterus. Malgaigne, who closely investigated the subject, could only meet with three cases of *true* prolapse of the vagina, and in all three it was partial and confined to the posterior part, which was protruded to a limited extent, without any corresponding dilatation of the rectum. I know of no authentic instance of prolapse of the anterior wall without corresponding descent of the bladder; but then it must be remembered that the cellular connexions between the bladder and vagina are much more dense than those between the vagina and rectum. Indeed the vagina is so closely connected with the urethra and neck of the bladder, that its separate protrusion at this part can scarcely be imagined possible.

It should be understood that in vaginal rectocele there is no displacement of the entire rectum; its posterior surface maintains its normal relation with the sacrum and coccyx; its anterior surface only is involved, and becomes dilated into a sort of pouch, which carries the vagina before it as a covering. When fæcal matters descend into the rectum, they distend this pouch, and the muscular fibres, being weakened by the dilatation, are unable to expel them; while the abdominal muscles, when called in to assist, instead of acting so as to overcome the resistance of the sphincter, expend their force upon the pouch, which they tend still further to dilate.

A rectocele is, in most cases—indeed in all which have come under my notice—of gradual formation. Malgaigne, however, mentions two instances in which it was suddenly produced—in one case by a fall, in the other by lifting a heavy weight. Both these patients were in an advanced stage of pregnancy.

On examination of the parts, protrusion of the posterior wall of the vagina is observed; and if the patient strains, the protrusion will be seen to increase in size. On introducing the finger into the rectum, it can be readily turned forwards into the swelling, thus showing that the rectum as well as the vagina is involved; while, by passing the finger into the vagina in front of the swelling, the uterus is usually felt to be in its natural position, or nearly so.

The affection is one which causes great discomfort and distress: there is a feeling of weakness, of weight, and bearing down in the perineal region,

with obstinate constipation and great difficulty in evacuating the rectum, which sometimes cannot be effected until pressure has been made upon the tumour to squeeze the contents out of it—until, in fact, the deficient contractile power of the rectum has been artificially supplied, and properly directed towards the natural outlet. Leucorrhœal discharge is a very frequent accompaniment.

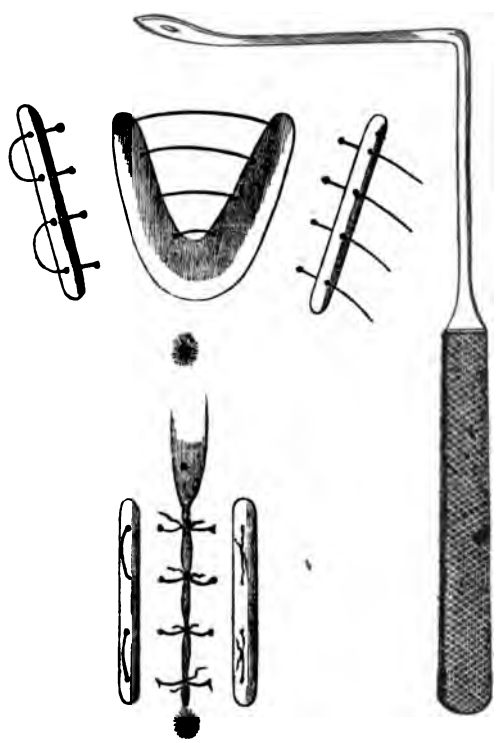
I have stated that partial rupture of the perineum predisposes to this affection. It is otherwise, however, when the rupture is complete and extends through the sphincter. In this latter case there is incontinence instead of constipation; for when the fæces pass into the rectum, there is nothing to prevent their escape, and dilatation therefore can hardly occur. I should have thought the existence of rectocele altogether incompatible with *complete* rupture of the perineum, but for a case recently recorded by Mr. Bryant, in which the two conditions were present in the same person. In this individual, probably the dilated rectum had been present before the laceration of the perineum took place. Out of eight cases of rectocele which have come under my notice, there was partial rupture of the perineum in five; in two there was no evidence of rupture, but the perineum was short and the parts relaxed; in one only was the perineum more than an inch in length.

It is necessary to remark that prolapse of the posterior wall of the vagina might possibly proceed from other causes than that now under consideration. A vaginal hernia, for instance, in which the intestine descends between the vagina and rectum, pushing before it the recto-vaginal fold of peritoneum, might cause a tumour similar in external appearance. Sir Charles Clarke mentions a case in which protrusion of the vagina was caused by the descent of cysts connected with diseased ovaries. It might also possibly be dependent upon an abscess in the recto-vaginal septum. The nature of the case, however, is at once made clear by examination of the interior of the rectum, the cavity of which will be more or less encroached upon in any of the cases just mentioned; whereas in rectocele it will be found to be dilated, and the finger can be at once passed forward into the tumour.

It is only within the last few years that any effectual surgical remedy has been devised for this complaint. The measures formerly recommended were merely palliative in their extent. Sir C. Clarke recommended the evacuation of the rectum by enemata, the use of astringent injections, and, in severe cases, a globular pessary to be worn in the vagina. Malgaigne relates cases in which attempts were made to diminish the calibre of the vagina by dissecting away portions of the mucous membrane covering the swelling, and uniting the edges of the incision by suture; but the result was not encouraging, and he therefore was content with a pessary of a peculiar description to retain the swelling in its place. The removal of portions of the apparently redundant vaginal membrane is not likely to be of much service: it does not remedy the corresponding dilatation of the rectum, and it does not supply the want of support in the pelvic floor which is the essential cause of the complaint. The only effectual mode in which that support can be supplied is by closure of the ruptured or relaxed perineum to a sufficient extent to retain the protrusion; and of late the operation for suture of the perineum has been applied by several surgeons (in the first instance, I believe, by Mr. Baker Brown) to the cure of rectocele, with very satisfactory results. I have myself practised it repeatedly, and it has in every instance fully answered my expectations. Indeed, whatever doubt there may be as to the efficiency of the operation in producing a permanent cure of prolapse of the uterus or of vaginal cystocele, I believe there need be none as to its effect, both immediate and remote, in vaginal rectocele.

The following is the plan of operation which I have adopted for some years past for suture of the perineum, whether for rectocele or for rupture of that part. The principle is that of the quill suture, but in several of its details it will, I venture to think, be found an improvement on the methods in ordinary use. The patient being placed in the lithotomy position, a portion of skin and mucous membrane is dissected off on each side of the lower half of the vulva, so as to form a raw surface, which should be about an inch

and a half in length on each side, the right and left portions being continuous with each other below across the median line. It should be an inch or more in depth antero-posteriorly at the lower part next the anus, but may diminish to about half an inch in depth towards its upper part. It is better first to mark the outline of this raw surface by incision with the scalpel, and then to dissect off the mucous membrane, the thinnest possible layer of which should be removed; but it should be taken away in one piece, and not in small fragments. I much prefer this plan to transfixing the part with the knife, and cutting a sort of flap from within outwards: in the latter way a larger amount of tissue is removed, which is objectionable, and vessels of larger size are likely to be wounded. By proceeding in the way above described, I have never had occasion to place a ligature on a bleeding vessel, nor have I ever met with bleeding sufficient to cause inconvenience, either at the time of the operation or subsequently, Care should be taken that the denuded surface is not situated too far outwards upon the buttock or too far inwards towards the vagina, but just where the opposite sides would naturally and readily come in contact. The deep sutures, which are to hold the quills, are next to be inserted. For this purpose I am in the habit of using a strong needle of rectangular shape, set in a handle, and with an eye near the point, as represented in the figure. This should be entered unarmed, on the left side (the terms right and left refer to the patient, not to the operator), a full inch external to the anterior border of the cut surface; it should be



passed deeply to take hold of as much tissue as possible, and brought out close to the posterior edge of the raw surface. It should then be thrust onwards through the tissues on the right side at a corresponding depth, and made to penetrate the skin at a point corresponding to that of its entry on the left. The eye near its point is now threaded with the wire suture, and

the needle is withdrawn, carrying the suture with it. The needle should be bent at a right angle about three inches and a half from the point, and should be slightly curved from the angle to the point. Such an instrument can be passed with much greater readiness than the needle in ordinary use, in which the handle is in a line with the shaft. I have found the latter difficult to pass, on account of the necessity of depressing the handle against the left buttock in order to carry the point through the integument of the opposite side. This inconvenience is entirely avoided with the rectangular needle, in using which also the surgeon's hand is in a much more convenient position, and the needle is passed through the parts with a simple turn of the wrist.

I am in the habit of using four deep sutures of silver wire, and of fastening them to perforated ivory bars, which represent the quills. Each ivory bar is about the size of a quill, and is perforated with four holes about half an inch apart, but they should be of different lengths for different cases. The ivory bar intended for the right side is ready threaded with two pieces of wire as represented, each piece of wire being looped through the two adjacent holes. This is held by an assistant, and the needle, after it has been passed across, is threaded with one of the four ends of wire and withdrawn; the proceeding being, of course, repeated with each of the other three. The wires are then passed through the holes of the second ivory bar, and being drawn tight, the whole is firmly secured by twisting together the ends, first of the two lower, and then of the two upper wires. By having the wires looped on the one side, no fastening is required on that side; while on the other side the two adjacent sutures are fastened simultaneously. Thus not only is time saved, but a uniform pressure upon the part is more easily secured. A single silver wire of sufficient strength to hold the quill suture securely is somewhat stiff and unmanageable. I have therefore latterly used very fine wire, twisted in three strands in the form of a rope. This gives equal or greater strength, and is at the same time much more flexible and convenient in use. Metallic sutures are greatly to be preferred to those of thread or silk. The latter, from their absorbent properties, produce much more irritation and suppuration along their track, and therefore less favourable to immediate union.

The quill suture serves to hold the deeper part of the raw surfaces in contact, but the cutaneous edges must also be held together by four or five superficial sutures of fine silver wire. These may be most conveniently inserted by means of Dr. Simpson's tubular needle. The operation, as above described may readily be completed in ten minutes or even less. In a case of this kind the sphincter is entire, and its action does not in any way interfere with the apposition of the cut surfaces; it is therefore quite unnecessary to divide it.

The patient is then placed in bed on her side. Sufficient opium is given to prevent the action of the bowels for the first six or seven days, and the urine is drawn off twice or thrice daily with a catheter. The deep sutures should be cut and removed, together with the ivory clamps, at the end of forty-eight hours. Some œdematous swelling generally takes place, but soon subsides after the pressure of the quill suture has been removed. I have never seen any benefit from leaving the deep sutures longer than this; on the contrary, whenever I have done so, I have found that a good deal of additional irritation, and often suppuration, has been set up, without any corresponding advantage. When metallic sutures are employed, there is rarely any suppuration along their track at the end of the second day, or if there is any, it is very trifling, and never in my experience to any injurious extent. The superficial sutures need not be removed till the sixth or seventh day, at the end of which time tolerably firm union will in most instances have been obtained. During this period a nutritious but unstimulating diet should be given, without restricting the patient's inclination as to quantity, and with a moderate allowance of beer or wine; but stimulants in any quantity, as sometimes recommended, I believe to be unnecessary in ordinary cases. The bowels should now be emptied by a brisk aperient, and the opiates discontinued. The catheter also may be dispensed with. In three or four days more, the patient may generally

be allowed to move about, and at the end of a fortnight th[illegible]acculated. He hospital are often able to return to their homes. At the en[illegible]e the wire was there is usually complete cicatrization throughout, the part is [illegible] He saw this and all tenderness consequent on the operation has disappeare[illegible]ose patient

The result of this operation for rectocele is in the highest d[illegible] had proven tory. I have practised it in seven cases, and in all of them [illegible]a common a thick and firm perineum, capable of effectually retaining the [illegible] this fact The sensation of weakness and bearing down is no longer felt, a[illegible]till in a stipation, so far as it depends upon the mechanical difficulty in em[illegible]in this rectum, is relieved. It may be objected, that the restoration of the [illegible]tality does not cure the dilatation of the rectum and vagina, but merely [illegible] their protrusion externally. I believe, however, that it does more th[illegible] and that when the parts are retained in their proper places, they wil[illegible] dually return to their proper dimensions. It must be remembered tha dilatation is not the essence of the complaint, but only takes place for chanical reasons, in consequence of the loss of support in the pelvic flo When that support is again supplied, the dilatation by degrees disappears, at any rate is so much lessened as to be of no practical inconvenience. I several cases which I have had opportunities of examining after the lapse of a year or more from the operation, I have been able to find scarcely any remains of the pouch which was formerly so evident. The protrusion is not likely to be reproduced, for the stress falls upon the posterior part of the new perineum, where it is thickest and strongest, and if the operation is properly done, there is no chance of this giving way.—*Lancet, Feb.* 3, 1866, *p.* 117.

94.—A CASE OF OVARIOTOMY IN WHICH THE PEDICLE WAS TIED AND RETURNED, AND THE LIGATURE REMOVED IN FORTY-EIGHT HOURS.

By Dr. J. H. Aveling, Senior Medical Officer to the Sheffield Hospital for Women.

Mrs. A. W.., aged 45, keeps a small shop and mangle, came into Sheffield Hospital for Women, suffering from an ovarian tumour. She has been married twice, and has two children. The eldest is now twenty-five, and the youngest twenty-two. Her second marriage took place in February, 1864. Her general appearance and complexion are good. There are, however, a few small boils on the abdomen, and slight œdema of the ankles.

The measurements are—1. Girth at the umbilical level, forty-one inches. 2. From ensiform cartilage to umbilicus, nine inches. 3. From umbilicus to symphysis pubis, nine inches. 4. From right anterior superior spine of ilium to umbilicus, nine and a half inches. 5. From left ditto to umbilicus nine inches.

There is too much distension to make out whether adhesions exist or not Fluctuation is very distinct. The uterus is pushed over to the left side, and can only be moved slightly. The length of its cavity is three and a half inches. The catamenia appears every three weeks, but the discharge is small in quantity. It was entirely suppressed for three months in 1864. Finding herself getting stouter, the patient then thought she might be pregnant: but as it hurt "her belly to lift," she concluded that something must be wrong, and sent for her medical man, Mr. Parker, who diagnosed ovarian tumour. From that time to the present she has continued to increase in size. Her breathing is very laboured, and accompanied by a troublesome cough. She sleeps on her right side, but is most comfortable when sitting nearly upright.

August 3rd, 1865, with full consent of the patient, the operation was performed. Drs. Jackson and Keeling and Messrs. Gillott and Sharp were present, and rendered valuable assistance. An incision was made between the umbilicus and the pubis, five inches long, commencing an inch below the umbilicus. As soon as the cyst was reached it was tapped, and the fluid withdrawn without passing the hand into the abdomen. No adhesions exist-

pedicle, which was short and about
needle carrying a double thread.
ight up together on either side,
oil had fitted upon its end a
s were secured after they
to compress the vessels.
the hook of my polyptrite
re was no hemorrhage, it was
nd the wound closed with iron
s of the two coil-clamps were left
ower angle of the incision.
isted of five cysts, communicating with
of a colloid mass about the size of the
The cysts contained twenty-one pints of

4th. Patient comfortable. Has taken food Pulse 84. No abdominal tenderness.

n. Has pain caused by flatulence. Pulse 80. The gatures were unwrapped from the cross-bars, the coils removed, and the ligatures drawn out.

6th. The wound is healed, with the exception of a small aperture the size of a quill, through which the coils passed. The sutures were removed, and plaster put on instead. Pulse 85.

8th. Has had a feverish attack, followed by an eruption which extends over the forehead, arms, chest, and abdomen. She is restless and uneasy. Pulse 95.

10th. The eruption fading. Pulse 80. The slight discharge which has continually escaped from the small opening has ceased.

From this time the patient had not an unfavourable symptom, and after two or three weeks of good living, she was discharged cured.

October 23rd. The patient walked to the hospital this morning, a distance of two miles, and says she is able to attend to her shop, and, now and then, to take a turn at the mangle.

The coil-clamp, which is figured in the preceding sketch, will probably be found most useful in cases where the pedicle is short, as by its use all dragging may be avoided. In the foregoing case two clamps were used. One would probably have been quite sufficient. They may be obtained from Messrs. Hutchinson, Duke-street, Sheffield.

Mr. SPENCER WELLS said that no one method of dealing with the pedicle could be applicable in all cases of ovariotomy. A long pedicle was dealt with so successfully by the clamp that he desired no better method; but it was still doubtful what was the best mode of proceeding when the pedicle is short. Tying it and cutting off close to the ligature, and returning it with the ligatures, also gave excellent results in persons of good condition. But in feeble persons, where copious serous effusion might be expected, and it appeared desirable to maintain an opening for drainage, the plan of the earlier ovariotomists, of leaving the ends of silk or twine ligatures hanging out through the wound, was so unsuccessful that the apparatus of Dr. Aveling would probably prove of great advantage in cases of short pedicle in weak patients. Ovariotomy was an operation still in its infancy, and anything which seemed likely to improve any of its steps was worthy of investigation and trial.

Dr. MARION SIMS said that the clamp was a great improvement on the ligature. He thought Mr. Baker Brown's plan of severing the pedicle by the actual cautery promised great results. He viewed Dr. Aveling's method

with favour. He always used a silver wire, and left it to be sacculated. He said the distal end of the severed pedicle did not slough where the wire was applied. The wire became embedded and hidden in its tissue. He saw this in a case in which he had applied the wire for M. Nelaton, whose patient died five days after the operation. His countryman, Dr. Peaslee, had proven that the stump of the pedicle did not slough even when tied with a common ligature, for he had made three *post obit* examinations, in all of which this fact was verified. He thought the plan of dealing with the pedicle was still in a transition state ; and he hailed with pleasure every improvement in this direction, for from this he expected a still greater reduction in the mortality of ovariotomy.—*Obstetrical Transactions, Vol.* vii., 1866, *p.* 229.

95.—ON CHLORATE OF POTASS IN THE TREATMENT OF OVARIAN DISEASE.

By W. Craig, Esq., Ayr.

Disease and enlargement of the ovarium have long resisted all kinds of treatment, and many medicines have been used in attempts to absorb the tumour, but hitherto unavailingly. Extirpation for some time past has been the favourite method of treatment, but it is attended with a large expenditure of life—at an average, nearly every second case. It is very desirable, then, that a mode of treatment could be discovered which would prevent such an expenditure of life, and this usually when the patient is in the most hopeful and interesting period of her existence.

In the following cases the cure has been effected by the administration of chlorate of potass. I could form no opinion regarding the nature of the tumours, other than that they were ovarian.

In his work on the Science and Art of Surgery, Mr. Erichsen states, that "medical means exercise no influence in curing, and but little if any in retarding the progress of ovarian tumour."

Dr. Charles Clay, of St. Mary's Hospital, Manchester, makes the following statement in the London Medical Review :—"I conscientiously believe that neither medical treatment, external or internal applications, pressure, nor galvanism, are of the slightest benefit. They neither cure nor palliate the disease. All such attempts, then, are fallacious, and only throw obstacles in the way of any benefit that extirpation of the tumour offers ; increasing the difficulties of that operation, if not defeating it altogether."

The treatment in the following cases is an exception to the above statement, as they were treated and cured by means of a very simple medicine, viz :—Chlorate of potass. It may be that one only of a species into which the disease is divided may be of a nature to be removed by this medicine, and, consequently, the others may remain uninfluenced by it ; but, during the life of the patient, unless after the operation, it is not always possible to learn the exact nature of the tumour; but whatever be its character, it can do no harm to the patient to allow her to have the benefit of a trial of this medicine, as it has a favourable action on the functions of the body, irrespective of the action on the tumour.

Chlorate of potass, as is well known, is a medicine that can he used with the utmost freedom. I do not pretend to offer an opinion as to its *modus operandi* in this disease. The circumstance of this salt having in combination a large quantity of oxygen, which is held feebly by the potass, and is left free in the system, may cause it to operate beneficially on the constitution, invigorating and improving the animal functions, so as to enable them to throw off these morbid growths.

Case 1.—Miss S., of Ayr, is of middle height, sallow complexion, and apparently of sound constitution. The tumour is on the left side, rising out of the iliac region. It is about the size of a child's head of a month old. The patient states that the tumour is sore when pressed, also during defecation and micturition. She menstruates regularly. It is free in its attachments and

rolls from side to side as the patient turns in bed. It is about five years since she first observed the enlargement, and it was about a year after this when she first applied for medical advice. She had the counsel of many medical men, and took many medicines, but received benefit from none of them. She was under the treatment of one medical man during the twelve months immediately before coming to me. This gentleman used many medicines and numerous external applications, all without any beneficial effect. He then, with the consent of other medical men, resolved to perform the operation, but he died when preparing for it.

When she came to me I immediately put her upon a saturated solution of chlorate of potass—a dessert-spoonful thrice daily. She stated that she had only taken the medicine two or three weeks when she felt a gradual improvement in her general health. The tumour gradually diminished in bulk till, at the end of ten or twelve months, it disappeared. After the tumour had been so far reduced as not to be felt through the parietes of the abdomen, it could be felt in its greatly reduced size lying close to the uterus. About this time it was about one and a-half inch in diameter. Subsequently, the tumour has disappeared completely, with the uneasiness and symptoms depending on its bulk, and she has since continued in her usual health.

Case 2.—Miss C., from London. The attention of this lady was first called to her complaint when taking a bath in June, 1861. At this time she felt a swelling rising from the right side of the pelvis. It was then about the size of an egg, and moved from the side to the middle of the abdomen. There was no pain in the tumour when the body was at rest, but in quick walking and some other forms of bodily exertion, it seemed as if bound by a light network all over the lower part of the right side. There was frequently a dull pain in the iliac region, and more rarely a sharp, stinging pain; but when at rest, or in ordinary walking, there was no pain. The patient states that her health was very sensibly affected by it. "I lost strength and tone, and became listless." The tumour grew rapidly from June to September, but after this the enlargement was slow in its progress. The tumour till now was always movable, but subsequently it became more fixed.

She came expressly to Scotland to consult an eminent practitioner, and was under his treatment two months of the summer of 1862, and nearly as long in the same season of 1863.

It was in the autumn of 1863 that she consulted me. On examining the tumour I noted no particular induration of its texture, but its size was about that of a large fist. Her former adviser had used many medicines and appliances with but little effect. The patient was twice cupped and leeched over the tumour, and the skin was twice painted over with a preparation which acted like a blister, and was also painted many times with combinations of iodine. She had tonics from the beginning, and daily a solution of bromide of potass. She also passed an electric current through the tumour for half-an-hour daily, and this was done during two years. The only effect of the treatment hitherto applied was to make the tumour "more compressed or harder." Immediately on her application to me, I commenced the administration of the saturated solution of chlorate of potass in dessert-spoonfuls thrice daily, and with what effect I shall allow the patient to tell. "I will add, that about three or four months after I had seen you, and taken the medicine you prescribed, the swelling disappeared as it came, silently and suddenly. I continued your medicine, and the occasional use of the electric battery until a few months ago. I use neither now; and as I said before, I have no swelling; none whatever; none."

I saw this lady in London a short time ago, and I could not discover a vestige of the tumour.

Case 3.—Miss H., from Glasgow, has had for a considerable time a tumour on the left side in the left iliac region. It was about the size of a large fist closed. She had used the chlorate of potass for two or three months before observing much diminution of the tumour, and at this time left off the use of medicine. She was induced, however, to commence the use of the medicine

again, and she states that, during the last few weeks, whilst using a renewed supply, the tumour has become much smaller, and gives less uneasiness.

A fourth case, in Ayr, presented herself with a small tumour in the left iliac region. The tumour had been observed for many months. It was painful, more especially when it was pressed. The size could not be well estimated, as the abdominal parietes were thick. I commenced with the chlorate of potass, but the patient soon became impatient and would not persevere, and has failed to continue the medicine.

So few cases go but a small way to establish the efficacy of this medicine in the cure of ovarian tumours; but, in view of the declaration of experienced and practical men who have seen much of this disease, and who maintain that medical means exercise no influence in curing, and but little in retarding the progress of ovarian tumours, it is right to produce facts, in however small numbers, when they show that medicine is not so inefficacious as represented by some writers on this subject.

It may be admitted that, before publishing my experience of the efficacy of chlorate of potass in the treatment of ovarian disease, I ought to have been able to present a larger number of successful cases; but in a small town, amongst a rural population, there is less chance of meeting a satisfactory number of cases than in the crowds of large cities.

Though the number of examples be small, the success attending the treatment ought to encourage a trial of a medicine that can be used with the greatest freedom, and could always be tried—provided there is no contra-indication—before having recourse to such a formidable operation as that of ovariotomy. *Edinburgh Med. Journal, Nov.*, 1865, *p.* 427.

96.—ON THE TREATMENT OF OVARIAN TUMOURS.

By I. Baker Brown, Esq.

On numerous occasions I have secured adhesions, especially to the omentum, by silver wire, cutting off the ends.

On September 9th, 1862, I secured a mesenteric artery, which was bleeding freely during operation, by silver wire, the patient recovering.

Since the introduction of Mr. Charles Clay's adhesion clamp, I have divided all adhesions and arrested all hemorrhage by actual cautery, and now divide the pedicle by the same method. The success attending this practice will, I believe, cause it to supersede all other methods of securing the pedicle in ovariotomy.—*Medical Circular, June* 14, 1865, *p.* 398.

97.—ON IODINE IN THE TREATMENT OF UTERINE LEUCORRHŒA.

The treatment of leucorrhœa is a constant subject of difficulty and vexation to the medical practitioner. Although the use of various astringents will often effect improvement, yet this is seldom lasting, and the recurrence of the symptoms is a continual source of annoyance. We have lately observed a plan which is being pursued by Dr. Murray at the *Great Northern Hospital*, and which promises to be a very useful addition to our means of treatment in this very troublesome condition. Dr. Murray first ascertains, by means of the speculum, that the discharge proceeds from within the uterus. He then introduces a small short-haired brush (much like that used for washing phials) by a screw-like motion, so that the thick, phlegm-like layer on the uterine wall is swept off with every turn of the brush. When this reaches the fundus he steadily withdraws it, charged as it is with the mucous deposit. Its place is then taken by a gum-elastic catheter, with several apertures, through which is injected a lotion consisting of one part of the compound tincture of iodine to two parts of water. The uterine wall is thoroughly washed with this. The muscular contraction which follows this

injection is remarkable, the tube being tightly grasped, so that its reintroduction at the time is extremely difficult. Dr. Murray has reason, after an experience of many cases treated by this plan, to feel highly satisfied with its success.—*Lancet, Jan.* 6, 1866, *p.* 7.

98.—ON DYSMENORRHŒA, METRORRHAGIA, OVARITIS, AND STERILITY, DEPENDING UPON A PECULIAR FORMATION OF THE CERVIX UTERI; AND THE TREATMENT BY DILATATION OR DIVISION.

By Dr. Robert Barnes, Obstetric Physician, and Lecturer on Midwifery and the Diseases of Women and Children, St. Thomas's Hospital; Examiner in Midwifery, Royal College of Physicians, &c.

[The opinions have been various as to the existence of, and the nature of narrowing of the cervix uteri: some think the contraction exists most frequently or always at the os internum; others at the os externum.]

The condition that prevails most frequently seems obvious enough. If a woman has suffered long from painful menstruation and metrorrhagia, and has been married some years without pregnancy, it may be predicated with great confidence that the neck of the uterus projects in an abnormal degree into the vagina; that its form is conical; that the os internum is a small round orifice, barely admitting the uterine sound; and that probably there is deviation of the cervix backwards or forwards, or to one side. This form of cervix has been aptly described by Ricord as the "*col tapiroïde.*" It is, no doubt, congenital, and may be traced back to imperfect development. It is sometimes associated with imperfect development of the body of the uterus and of the ovaries; but this association is certainly not a necessary one. In the majority of instances the ovaries perform their function, irregularly, it is true; and pregnancy frequently occurs after the error of formation of the cervix is corrected.

Another circumstance to be noted is the rather frequent association of this peculiar form of cervix with a short vagina and general defective development of the pelvic organs.

The vaginal portion may project into the vagina from half an inch to one inch, an inch and a half, or even two inches. This excessive projection into the vagina may be due to a preternatural hypertrophic elongation of the lower part of the cervix; but I believe it is commonly due to the reflection of the fundus vaginæ taking place at a higher point of the cervix than usual. In the ordinary construction, the cervical canal communicates freely with the vagina by an open transverse fissure, inclining, indeed, to a circular form in the virgin. The form of the cervical cavity is thus a flattened cone or funnel, of which the base is open. The vaginal portion projects as a flattened semisphere scarcely half an inch into the vagina, the vagina being reflected off from the cervix a little above the level of the os externum. In some cases the vaginal portion is a cone, the apex projecting nearly an inch in the vagina, and the vagina being reflected off nearly on a level with the os uteri internum. Instead of the natural free communication between the cervical cavity and the vagina, the os externum is so contracted as to form a sensible obstruction. Indeed, sometimes the ordinary uterine sound can be passed only with difficulty; and I have known the occlusion to be complete at times, requiring some little force to break down a thin membranous septum formed at the orifice. As soon as the os externum uteri is penetrated by the sound, it is usually found that the point enters into a sufficiently capacious cervical cavity. This cavity, in fact, is exactly spindle-shaped; it narrows again towards the os uteri internum, which is, as Dr. Henry Bennet has shown, naturally a small opening. In cases of protracted suffering from the dysmenorrhœa attending the peculiar form of cervix under consideration, I have, however, generally found that the sound passes through the os internum without difficulty. It is by this observation that I have come to the conclusion, already mentioned, that in some cases the excessive pro-

jection of the vaginal portion is simply due to the vagina being reflected off from the cervix at a higher level than usual. In such cases the introduction of the sound to the fundus uteri shows the entire length of the organ to be normal.

The seat of the obstruction, then, I believe to be almost invariably at the os uteri externum. The obstruction is due chiefly to the small round os itself; partly to the pointed, elongated form of the lower part of the vaginal portion, and partly to an unusual rigidity of structure of this part, which impedes the expanding action natural to the healthily formed os uteri.

With this coarctation of the os externum uteri, and undue projection of the cervix into the vagina, we might almost reason out most of the consequences which observation has detected. Some of them I will briefly describe. The narrowing of the vaginal orifice of the womb will, as has been often described, cause obstructive dysmenorrhœa. This is, indeed, sometimes so marked as to resemble retention of the menstrual fluid from complete occlusion. There is a partial occlusion, and the same symptoms, only less in severity, follow. The ovarian excitation of menstruation causes engorgement of the uterine mucous membrane; blood is effused; it cannot escape readily, and whilst fluid; it tends to accumulate in the uterine cavity and in the cervical cavity, and sometimes to form coagula. The blood so gathering and pent up in the uterus causes efforts at expulsion, betrayed in spasms or uterine colic. These pains are constantly likened to labour-pains; they recur at distinct intervals of from five to fifteen minutes, are extremely severe, and are commonly partially relieved on the discharge of a quantity of blood or coagula.

This is often the character of menstruation in girls having this formation, from the very establishment of the function; but sometimes menstruation only assumes this painful character after the lapse of some time.

The next consequence is periodical congestion beyond the normal degree of the ovaries and uterus. Preternatural congestion seeks relief in hemorrhage. The mucous membrane of the Fallopian tubes and uterus pours out blood copiously. Hence we have metrorrhagia or menorrhagia, in addition to dysmenorrhœa. The patient is distressed by the recurrence of the menstrual flow after intervals of two or three weeks only, and by its lasting for a week or ten days.

A secondary consequence—I am persuaded not an infrequent one—is an oozing of blood from the abdominal end of the Fallopian tubes and from the surface of the congested ovaries. This is a form of retro-uterine hæmatocele of which I have seen several instances in young girls. Symptoms of shock announce the outpouring of blood into the peritoneum, or in the cellular tissue of the broad ligaments; intense pain in the abdomen and pelvis, fever, announce the reaction and peritonitis. As in almost every instance of retro or peri-uterine hæmatocele, a simultaneous escape of blood takes place externally.

Attendant upon repeated congestions, ovaritis may follow, entailing plastic adhesions between the Fallopian tubes and ovaries, and limited pelvic peritonitis. These adhesions may become a cause of sterility, and will, I believe, sometimes induce atrophy of the ovaries.

All these results may occur in single women. When women so suffering marry, the symptoms set forth are often much aggravated in severity, and new evils add to their distress.

1. *Sterility* is an almost constant condition. Occasionally one meets with examples of the conical cervix with small os with the history of a childbirth some years previously, and more frequently with an account of having suffered abortion. Strict inquiry will generally throw doubt upon the reality of the abortion. The profuse flooding and discharge of clots interpreted by the patient as evidence of abortion are often nothing more than the metrorrhagia characteristic of the uterine obstruction. Where impregnation takes place, it usually does so in young persons early after marriage, that is, before the further disorders, which we shall presently notice, have had time to be developed.

2. *Cervicitis interna et externa* and *endometritis* are amongst the first consequences of marriage. The periodical engorgements of menstruation easily pass into subacute and chronic inflammation, chiefly affecting the mucous membrane lining the cervical cavity and the vaginal portion. The secretions resulting from this inflammatory action find difficult exit from the cervical canal. Hence there is increased, often constant, pain. Conjugal relations become almost intolerable. It is common now to find a highly vascular state of the cervical mucous membrane; the small portion seen just inside the minute os is deep-red, angry looking. There is loss of epithelium round the os. The introduction of the sound easily causes bleeding. At first the discharge is chiefly albuminous; afterwards it becomes muco-purulent and even sanguineous, and by its abundance entails serious constitutional debility.

Not infrequently, vaginitis follows, chiefly affecting the upper part of the canal. This I have several times found so intense as to produce vaginismus, and render sexual relations impossible.

3. Whether or not the inflammation described be produced, *deviations of the projecting vaginal-portion* are very common. The most ordinary form is flexion or curving of the cervix forwards. Sometimes it is driven backwards. Lateral deviation to the right or left is often observed. In fact, the excess of vaginal portion is equivalent to a foreign body in the vagina. It is thus constantly liable to injury in the shape of inflammation and displacement.

I believe flexions of the cervix upon the body occur far more frequently in this way than as the result of a fibroid body growing in the wall of the body of the uterus, as represented by Dr. Marion Sims. The deviation really belongs to the cervix, and not to the body. Whenever deviation takes place, the prospect of impregnation is still further impaired. The minute os externum is thrown aside, and there is stricture by angulation at the seat of the os uteri internum.

I have notes of a case which shows in a striking manner the severity of the symptoms sometimes produced. A young lady had been married two years without pregnancy. Since marriage she had suffered from metrorrhagia, and several attacks considered to be, and treated as, peritonitis. During the last six months she has had a constant sense of swelling with pain in the left ovarian region; vomiting attends the pain. This has been relieved by leeching. When I saw her, metrorrhagia had continued six weeks without cessation. Great prostration was present, with irritative fever, reminding me of pyæmic puerperal fever. I found a small conical cervix, with an os so minute that it required considerable pressure to introduce the uterine sound; deviation of the cervix to the left; defined tumefaction and pain in the left ovarian region. I inferred that the constricted os externum, impeding the flow of blood from the uterus, led to the formation of coagula in the cavity; that these coagula were broken up by decomposition; that absorption of septic matter took place, causing constitutional symptoms, and possibly cellulitis in the left broad ligament; that the tumefaction in the left broad ligament might also be due to hemorrhagic effusion. I split the cervix with my instrument. The metrorrhagia, which had persisted for six weeks, and was abundant at the moment of the operation, ceased in a few days, and the pain abated.

The treatment of these cases has given rise to much discussion. It is convenient to divide the subject under two heads:

(A) The general treatment of the effects of the mechanical obstruction.

(B) The direct treatment adapted to remove the cause.

(A) Depending, in great part at least, upon a mechanical cause, any plan of action that is not adapted to remove this is not likely to do more than mitigate the symptoms. One class of remedies will be called for to control or moderate the hemorrhage, and to counteract the consequent anæmia. A considerable amount of success may attend the use of nitric and hydrochloric acids, cinchona, iron, Indian hemp, Chian turpentine, cinnamon, digitalis, and the other styptics and tonics in general use. I have not seen much benefit from bromide of potassium. The pain and colics will demand special remedies; opium,

belladonna, Indian hemp, are often serviceable. Special treatment will also be necessary to relieve the local inflammations. Vaginitis and aggravated cases of cervicitis externa require, above all things, *rest*, in the fullest acceptation of the term—rest, functional as well as physical. To attain this end, repose in the horizontal position is important. Possibly this, aided by the application of pledgets of cotton-wool soaked in glycerine and tannin, by injections of various astringents, or of plain water, by occasional cauterization with nitrate of silver, will suffice. But in more severe cases, where a large surface of vaginal mucous membrane is inflamed, spasmodic contraction of the muscular coat is apt to follow. The effect of this is very distressing. The rugæ of the vagina are preternaturally enlarged; the contractions of the muscular coat cause chafing of the opposed prominent rugæ; hence abrasions of epithelium, purulent discharge, sometimes blood, and the most intense pain. The essential condition of recovery from this state is *rest*. This I accomplish by getting the patient to wear a vaginal support, made of india-rubber, of a somewhat cylindrical form, which is inflated with air after introduction. I have not found the division of the sphincter, recommended by Dr. Marion Sims, to be necessary. The form of vaginismus to which I am referring is, indeed, different in origin and nature from the cases described by Dr. Sims; the vaginismus here is strictly the result of inflammation. The action of the vaginal rest I employ is, first, to keep the irritable and inflamed walls of the vagina apart; the prominent rugæ, no longer chafing against each other, have an opportunity of getting well. Secondly, the easy mechanical support afforded to the walls of the vagina quickly subdues the morbid contractility of the muscular tissue. Very soon the inflammation and irritability subside simultaneously. By this means I have cured several very aggravated cases. In one instance—that of a lady who had been married sixteen years without being able to endure conjugal approaches, and in whom an examination was not possible without the administration of chloroform—complete relief was afforded, ending in pregnancy and the birth of a living child. The application of this principle and means of procuring local rest is much more extensive. I resort to it in cases of extreme engorgement and inflammation of the cervix, following upon labour.

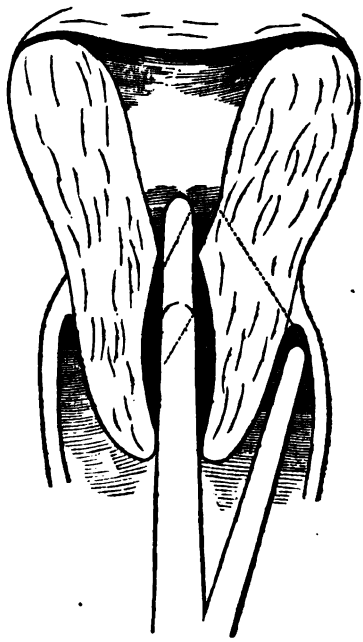
It remains to consider the treatment as directed against the cause of the disorders under discussion. The importance of this treatment must be admitted when we reflect upon the severity and dangers of the disorders that call for relief, and the hopelessness of giving more than partial and temporary relief by means directed simply against the consequences of a physical obstruction.

The principal means of overcoming the coarctation of the os externum uteri are dilatation and incision. It would be superfluous to enter at any length on the description of the numerous contrivances that have been devised for dilating the cervix; but it is of great moment to determine the amount of safety and the efficaciousness which attend dilatation. If it were proved to be more safe and efficacious than incision, then it ought to be preferred. Evidence is conflicting. I will not venture to offer an opinion as to the relative value of the facts and arguments adduced by others. I cannot, however, estimate lightly the circumstance that men of great experience, sagacity, and resources, like Simpson and Marion Sims, have abandoned dilatation in favour of incision. They have found dilatation neither safe nor efficacious. Hemorrhage, pyæmia, cellulitis, peritonitis, have undoubtedly followed dilatation; and it is certain that in many cases, however good the dilatation effected by bougies or tents may appear at first, it is not of long duration. I suppose there is no dilatation by instruments more powerful than that effected by pregnancy and labour, yet even after giving passage to a full-grown child the peculiar cervix under consideration will sometimes completely resume its old vicious form. The sea-tangle-tents lately come into use certainly possess advantages not before attained, and so strengthen the case for dilatation. But, admitting that dilatation can be completely attained, are all the indications, all the requirements of the case, answered? Will dilatation remove the congestion, the inflammation, the deviations

which attend the conical cervix? Here I believe dilatation fails, and incision, or splitting the cervix, is an operation possessing a marked superiority in results.

I will briefly advert to the instruments employed. There is first the well-known *bistouri caché*, or metrotome of Professor Simpson. In fig. 1, is

Fig. 1.



represented this instrument in action. It is well adapted to answer its purpose of dividing the cervix, but requires great care in use. After introduction into the cervix, the blade is detached from its guard in such a manner as to open at once to the full width at which it may be set by the screw in the handle. The incision it makes, if not very carefully controlled by the hand, will be liable to extend deep into the substance of the cervix at its upper part, near the junction of the body of the uterus. There is no doubt that incisions so made have actually gone through the substance of the cervix, and wounded the plexus of vessels outside.

Incisions so free in extent, and involving the higher portion of the cervix —its most vascular part—may be expected to cause considerable hemorrhage. This is a great objection—one all the more serious in that such free incision is altogether unnecessary. It will be seen that objection lies partly against the rationale of the operation, and partly against the instrument.

Another instrument has been devised on the same principle as Dr. Simpson's, but having two blades, which diverge and cut both sides of the cervix simultaneously. There is no advantage in cutting both sides at once; on the contrary, the operator having to use an instrument constructed to do its work like a machine, is deprived of the opportunity of guiding and moderating his incision. The single-blade of Dr. Simpson is less open to this objection. It may be made to work according to the judgment and skill of the operator.

Dr. Greenhalgh has contrived an ingenious modification of the double-bladed metrotome (described 'Obstetrical Transactions,' Vol. V.). The blades do not diverge to the full width at once, but gradually, forming an incision of

the form of an ellipsis. The incision being narrow at the upper part avoids to some extent the danger of cutting deeply into the substance of the cervix. The division of the projecting part of the cervix made by it does not extend to the angle of reflection of the vagina. I used this instrument for some time. Owing to the extreme fineness of the blades, the incisions made are so clean as to favour hemorrhage; and unless great care is taken to keep the lips of the wound apart by tents, its reunion is very apt to take place. It is, moreover, open to the objection stated against the old two-bladed metrotome, namely, that it is too much of an automatic machine; not enough of a surgical instrument.

Feeling this objection, and that an instrument should be absolutely governed by the intelligence of the operator, and not so made as to work after its own fashion, I contrived an instrument to cut like a pair of scissors, which I think is free from all the objections applying to the instruments or machines just described. One blade, strong and somewhat probe-shaped, passes through the narrow os into the cervix, whilst the other blade, slightly concave on the cutting edge, so as better to seize the rounded surface of the cervix, is applied at any point of the vaginal portion the operator chooses. Then, the part to be divided being exactly included between the blades, is cut through. Thus we are sure of cutting as much as we want, and no more. There is this great advantage in cutting from without inwards instead of from within outwards, that *we are cutting towards safety and not towards danger*; we cannot cut deep into the substance of the cervix on a level with the os internum; the incision partakes of the crushing character. These two circumstances afford better security against severe hemorrhage or other danger. When one side of the cervix is divided, the other is treated in like manner. The instrument is most simple, inexpensive, and not likely to get out of order. The wound made by it is less liable to reunite by first intention. It *gapes* better than that made by the knives. With this instrument, made for me by Messrs. Weiss a year ago, I have operated many times at the London Hospital and in private and consultation practices. Its action has almost always been entirely satisfactory. It produces a good os tincæ. (The instrument is represented in action, fig. 2.)

It afforded me sincere gratification to see in the Lancet of the 1st of April last, a paper by Dr. Marion Sims, in which it appeared that he had for some years before adopted the same principle of proceeding as myself, and was in the habit of using a similar instrument. Our distinguished *confrère*. however, fixes the cervix by means of a vulsellum before cutting. I have not found this to be necessary. My experience entirely corroborates that of Dr. Sims as to the safety and efficaciousness of this operation.

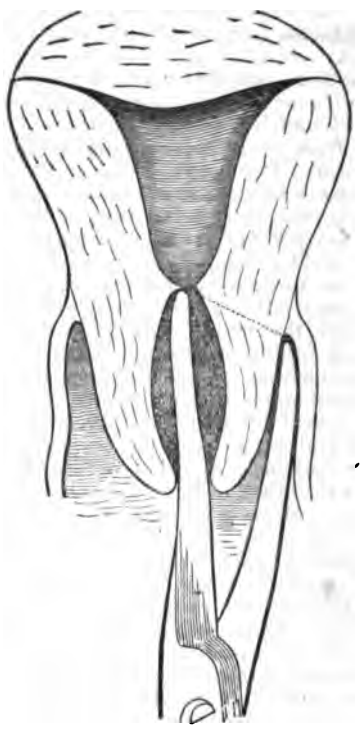
I will conclude with a brief *résumé* of the advantages attending division of the cervix, as I have performed it, over dilatation. I believe that dilatation is preferred by some physicians who have seen or heard of severe hemorrhage after division by the single or double knife, on the ground that it is safer, and entails less responsibility. I doubt whether dilatation be safer than division as performed by Dr. Marion Sims and myself. I have never seen hemorrhage at all alarming; it is usually stopped in five or ten minutes by applying pledgets of cotton-wool soaked in cold water; and it is rare to find any secondary bleeding of moment. To guard against this, and at the same time to obviate the tendency of the wound to unite, I press a bit of oiled lint between the lips of the wound, and lightly plug the vagina with oiled lint to keep the dressing *in sitû*. I need hardly say that rest in bed for two or three days is enforced.

Any troublesome bleeding may be quickly arrested by iced water, by perchloride of iron, or by firm plugging.

As to the permanency of the new os tincæ I am not able to speak with certainty. There is no doubt a tendency to contract; but I question whether the contraction ever proceeds to the extent of reducing the os to its original minuteness; nor is the risk of contraction so great as after simple dilatation. It may be necessary to repeat the operation after the lapse of some years; but I believe this will rarely happen.

Mr. BAKER BROWN differed from the author as to the seat of stricture; he believed it to be in the cervix itself, generally accompanied by narrowing contortions, and reflections of the canal, the results of inflammation. He thought stricture, either at the internal or external os, was exceptional. Mr. Brown described his mode of operating, which was as follows. - Having

FIG. 2.



placed the patient in the lithotomy position, he introduced a bent speculum, and then, grasping the anterior lip of the os uteri with a pair of vulsellum forceps, and holding it up in the median line, he introduced Simpson's hysterotome, first having arranged the blade so as to determine the depth of the incision to be made. He cut from within outwards completely through the os externum and neck, as far as possible, without wounding the vagina; having done this on the right side, he reversed the instrument and did the same on the left. He had no fear of hemorrhage when the subsequent steps which he adopted were fully carried out, namely—inserting oiled lint well into the cut surfaces, and then plugging the whole vagina with lint or cotton-wool, so as to prevent any admission of air which might loosen the pledgets of lint in the wound. Mr. Brown had always found Dr. Simpson's instrument to answer exceedingly well; and had tried, but did not like the double hysterotome. He, however, thought it highly important that the operator should see what he was doing, and therefore objected to Dr. Simpson's method of operating, with the patient on the side, and using the finger as the only guide to the os uteri.

Dr. GREENHALGH stated that he had listened with great attention to the president's paper, and was extremely surprised to hear that he considered the seat of stricture in cases of dysmenorrhœa to be at the external os uteri, and that the neck of the uterus was small and tapering. A large experience had convinced him (Dr. G.) that although such cases are occasionally met

with, still in the great majority the seat of stricture will be found at the internal os uteri, the neck of the uterus being somewhat enlarged, and not tapering; consequently he deemed it essential that the internal as well as the external os uteri should be divided. The drawings sent round were evidently ill-developed virgin uteri, in which the operation is rarely or never required. After division, Dr. Greenhalgh introduced his bilateral expanding stem, which keeps up steady dilatation and prevents contraction of the parts. Much had been said about hemorrhage, but he could positively affirm that, out of nearly one hundred cases of division of the os and cervix with his metrotome, in one case only was plugging had recourse to, and in this instance the patient was extremely plethoric; moreover, the uterus was much congested, and she was the subject of severe pruritus, and on the eve of a menstrual period; the operation was perfectly successful. Dr. Greenhalgh considered the great advantages of his method were the extreme exactitude, quickness, and almost painlessness with which the incisions can be made, and that without any exposure of the patients during or after the operation.

Dr. ROUTH fully confirmed Dr. Greenhalgh's view. For his own part, he believed in by far the majority of cases the obstruction was at the *inner* and not the *outer* os; although he did not deny that in some cases of conoid cervix it was present at the external os. He agreed with Dr. Gream in believing that Dr. Sims's plan of operation would occasionally leave a deformed cervix for life; and he did not think it was necessary to cut through the entire cervix. The instrument Dr. Greenhalgh had invented obviated all danger from hemorrhage. The same was true of his (Dr. Routh's) instrument, which he, however, preferred, because of the bend, and, therefore, more easy of application in *flexion* cases. A little bleeding was salutary. In most of these cases there was a complication of congestion, which the very incision by the subsequent hemorrhage relieved. But there was no doubt that such incisions, however freely made, had a tendency to contract again. Hence it was necessary to keep the cut made patent by some internal uterine pessary, and for some time, it might be for months, so as to allow it to become properly lined with mucous membrane and incontractible. He knew several persons now walking about London with these. In other cases their removal had been followed by conjugal relations and pregnancy, though previously sterile for years. Of the use of sponge-tents and other modes of artificial dilatation, in these cases, he spoke disparagingly. He had seen cellular abscess and death follow their use. They should be used with the greatest caution. He also believed cases of dysmenorrhœa were more common than was generally supposed. Not only was the seat of obstruction more frequently at the *internal* os than the *external*, but indeed, in many cases the external os was patent and abnormally so, as shown by Dr. Henry Bennet. And there were many, and by far more numerous cases of dysmenorrhœa which were in no way due to stricture at either os. As these cases were not, however, referred to by Dr. Barnes, he did not allude to them further.

[Dr. SAVAGE laid before the Society an injected preparation of a virgin uterus.]

He believed the preparation to be an excellent specimen of a uterus which had never been impregnated. It appeared to him to warrant the following deductions:

1. Except in cases of irregular vascular distribution, which happen far from seldom, the vaginal portion of the cervix could be divided without risk of serious hemorrhage; division beyond this would endanger the lower cervical arteries.

2. An incision laterally deeper than $\frac{1}{8}$th of an inch at any part of the canal of the cervix or isthmus would be unsafe.

3. An internal os uteri surrounded by a ring of muscle (attributed to Kölliker) as asserted by Mr. Wells, does not exist. The isthmus is surrounded by muscular fibres subjacent to the mucous membrane; but the complete identification, so to speak, of the latter with the rest of the uterus, and the

extremely oblique decussations of the former could not contribute to the formation of a so-called inner os. The uterine isthmus could never form a stricture.

4. That the instruments so vaunted for the cervical dilatation of the cervix, most fortunately for operator as well as patient, did little else than divide half through the vaginal portion of the cervix, and this even not efficiently. The complete division of the vaginal part of the os required the scalpel or scissors.

5. That the least bending of the isthmus, where only a bending could occur, would cause an obstruction, without the least constriction or narrowing organically of that part of the uterine canal, such obstruction not being remediable by cutting.

Dr. Savage said experience amply confirmed these deductions. The easier introduction of a sound through the isthmus after surgical dilatation, as ordinarily performed, would be no proof that the so-called triangular incision, commencing with the division of a constricted isthmus, had been made; mere division of the vaginal portion of the cervix was constantly followed by relaxation of the isthmus, whereby could be easily introduced sounds of a size not before admissible.

He believed that obstruction at the isthmus was generally caused by uterine curvature. The canal of the cervix beyond the vaginal portion was never contracted. Obviously a very slight bending would cause obstruction, owing to the antero-posterior coaptation of the corresponding sides of the isthmus. He quite agreed with Dr. Sims, that no straight instrument would relieve an obstruction thus produced. Dr. Greenhalgh's hysterotome was the best in the automatic list of such instruments, but it was half an inch too short, could not be trusted to cut the vaginal part of the os completely through, and could not be made to pass a curvature; but suppose the uterus was straight, the passage of any such instruments through the isthmus showed that division was not required.

Dr. Sims's denunciation of mechanical dilatation of the uterine cervix and isthmus, Dr. Savage could not comprehend; Dr. Gream, a physician of immense experience, had strongly testified to its safety. He (Dr. Savage) was under the mark in stating he had employed it at the Samaritan Hospital, and elsewhere, many hundreds of times, and in this respect would amply confirm Dr. Gream's assertions; besides being quite as safe, to say the least, it was quite as efficacious as surgical dilatation, especially when performed by the new instrumental inventions. Dr. Sims's process certainly left nothing undone; but he (Dr. Savage) had for years been in the habit of dealing with the worst of such cases as Dr. Sims had described nearly every week at the Samaritan Hospital, and never had failed in overcoming such obstructions with the sponge-tent. It might fairly and properly be urged against surgical dilatation performed in any of its various ways—it was useless to do too little, very dangerous to do too much, and just enough left the uterus permanently mutilated. The operators were not the best authorities, as, unknown to them, their failures marched off to other practitioners. Dr. Gream, and indeed every man of note in this department, speak of case after case rendered worse by an indifferent operation, or exposed to all the dangers of abortion after an efficient one. Dr. Sims speaks of furious hemorrhage occurring at the first stage, and the fatal cases are notorious and far from few.

Dr. MARION SIMS agreed with the President (Dr. Barnes), and with Mr. Baker Brown, and Dr. Savage, in saying that in the great majority of cases requiring surgical interference, the canal was found to be narrowed in the lower portion.—*Obstetrical Transactions*, vol. vii., 1866, *p.* 120.

99.—ON IMPROVEMENT OF THE BLOOD IN CHLOROSIS, AND ON THE ELIMINATION OF CERTAIN POISONS.

By Dr. C. E. Brown-Sequard, F. R. S., &c.

It has been shown that chlorosis is a nervous affection, and that the deficiency of iron in the blood of chlorotic patients occurs only after this affection has existed some time, and is an effect instead of a cause. It has been further shown that iron serves to cure that affection only on account of a peculiar influence which causes the food to be transformed more easily into blood, and perhaps, also, of a direct improvement in the nutrition of the nervous centres. Other metals, which are not normal constituents of blood-globules, especially manganese and silver (particularly the oxides of these metals), sometimes act better than iron in cases of chlorosis and anæmia. He who will read attentively the remarkable papers of Bouchardat on the pugilist's training, and on the rapidly curative effect of exercise and good food in diabetes, will soon be convinced of the immense value of purely hygienic means in improving nutrition and secretion. It is quite natural, therefore, that gradually the idea is being abandoned that whenever a principle is diminished in quantity in the blood or in the tissues, the best treatment consists in trying to increase its amount by giving as a remedy the deficient substance. The selection of a remedy as a tonic or a stimulant ought, therefore, to be quite independent of any such chemical view, and we ought to let a good diet and all the principal rules of hygiene take care of the composition of the blood, remembering, however, that it may be useful to the patient to employ the kinds of food which we know to contain, in easily absorbable forms, iron, phosphorus, or some other substance which seems to be in insufficient quantity in the blood or in the tissues.

As regards the elimination of mineral poisons which have produced functional nervous affections, I will only say at present that the discovery of Melsens, which shows the value of large doses of the iodide of potassium for the expulsion of lead and mercury from the system, has been universally recognised. It is important to remember that the dose of that salt must be very large for its successful influence. The statements of Melsens and N. Guillot on this point have received positive confirmation from researches by Dr. W. Budd, of Bristol, and Prof. Easton, of Glasgow. My own observations agree with the conclusions of these able physicians. The elimination of lead and mercury takes place chiefly by the urinary, salivary, and cutaneous secretions. Consequently saliva must be spit and the skin very frequently washed, so as to avoid a partial absorption of these poisons after they have been secreted.

I will not speak of the elimination of morbid poisons (such as those of diphtheria, rheumatism, gout, syphilis, leprosy, &c.) having produced nervous affections, because nothing of great importance has been found recently that applies to all, or almost all, those poisons. The only fact worth mentioning is, that the iodide of potassium may serve for the elimination of all but one of them.—*Lancet, March* 10, 1866, *p.* 247.

100.—GLANDULAR TUMOUR OF MAMMA, WITH CYSTS; INJECTION WITH COMPOUND TINCTURE OF IODINE.

By William Martin Coates, Esq., Surgeon to the Salisbury Infirmary.

[The patient was 36 years of age and unmarried. The left breast was much larger than the right, and was tender. In the centre of the affected portion, which measured two inches and a half in its diameters, there was to the touch a feeling of elasticity giving an impression of deep-seated confined fluid.]

On introducing a grooved needle at the softest point, a clear fluid escaped.

On the 13th of August I passed the perforated needle of Wood's syringe,

with piston down, into the elastic part of the tumour, and by withdrawing it (the piston, see Art. 102, p. 223) removed five drachms of clear fluid, and threw in two drachms of compound tincture of iodine. Very little immediate pain was caused by this proceeding. On the 14th there was some little tenderness and increase of size. These effects soon subsided, and in the course of a month the mamma was not larger than the right, all hardness having disappeared.

On December 25th, 1864, I was consulted by the same lady for another cyst, situated directly below the nipple of the same breast, with the same increase of glandular structure around. I applied the same remedy, with the same happy result; and to-day (Dec. 19th, 1865) I carefully examined the part, and found it free from enlargement, hardness, tenderness, or abnormal elasticity. There occurs at each menstrual period a sensation of fulness and tenderness, subsiding as the period passes away.

The interesting points in the foregoing case are, the bloodlessness, simplicity, painlessness, and rapidity of cure of the cysts, when compared with removal of the tumour, or the laying it open and stuffing it with lint, the treatment recommended by the most eminent authorities. Secondly, the subsidence of the glandular hypertrophy a few weeks after each injection, leading to the inference that the iodine acts in this disease in the same manner as in cases of hydrocele, in which I have frequently seen the enlargement of the testicle, so common in this disease, gradually subside after the treatment by injection of the tincture of iodine.

Wood's syringe enables us to treat cases in which iodine injections are useful in a much less painful manner than by the hydrocele trocar; and if the needle has a rather larger bore, it forms an excellent mode of exploring for fluid, as, by withdrawing the piston, considerable traction is exerted upon the liquid contents of a tumour.—*Lancet, March* 3, 1866, *p.* 226.

101.—CURE OF A LARGE RANULA BY INJECTION OF COMPOUND TINCTURE OF IODINE.

By William Martin Coates, Esq., Surgeon to the Salisbury Infirmary.

[The patient was 10 years of age. The ranula was large.]

June 11th, 1865. I injected from within the mouth, by means of Wood's syringe, fifteen minims of compound tincture of iodine into the tumour. On withdrawing the needle, a drop of the fluid peculiar to ranulæ oozed from the puncture.

12th. A little hardening of the swelling, but no pain.

21st. Tumour rather smaller. On this day I injected thirty minims of the tincture from without by means of Wood's syringe.

22nd. There is considerable hardening of the ranula, and some tenderness on handling it.

From the last note the tumour daily diminished, and on the 15th of July it could not be either seen or felt.

I cannot find by reference to those works on surgery in my possession that the cure of ranula has been attempted by the injection of iodine. I therefore have assumed that the idea is an original one of mine. It is so simple, so painless, so free from danger and annoyance, that I have ventured to place it among my interesting cases. It is true that one case is too small a basis on which to found a treatment; but as there must be a first case, and as ranula (according to my experience) is not a common affection, I publish it in the hope that others may give it a trial; and perhaps in the future, those suffering from this disease may escape, through my means, incision, excision, the cautery, the seton, the opening it in its whole length and stuffing it with lint, or the platina tubes of Dupuytren—all of which must result more or less in failure, pain, bleeding, or distressing salivation.—*Lancet, March* 3, 1866, *p.* 226.

102.—ON A CASE OF SPINA BIFIDA CURED BY INJECTION OF A SOLUTION OF IODIDE OF POTASSIUM.

By W. Martin Coates, Esq., Surgeon to the Salisbury Infirmary.

[The tumour was about the size of a large walnut, transparent and flaccid, and was saturated at the lower part of the last lumbar vertebra and the upper part of the sacrum.]

The opening into the spinal column was about half an inch long and a quarter wide. It was easily felt. The child was otherwise well formed and healthy. There was no paralysis of the lower extremities. When the child cried or struggled the tumour became very tense. I gave a desponding prognosis. The tumour progressively enlarged, becoming daily less flaccid when at rest, and more tense during exertion or crying.

About a month after the birth of the child, I read, in an extract from the Boston Medical Journal, that "two American physicians, Drs. Brainard and Crawford, had treated seven cases of spina bifida by iodine injections, and that five were cured, one dying seven months after of chronic hydrocephalus. They used from a quarter of a grain to four grains of iodine, and from three-quarters of a grain to twelve grains of iodide of potassium, dissolved in from one drachm to several ounces of water. The injections were repeated as often as necessary, the strength of the solution being gradually increased. The punctures were made in sound skin, at the side of the tumour. No more of the fluid was withdrawn than was replaced by the injected solution. After the operation collodion was applied, and for months after the tumour had disappeared."

I proposed to operate on Rosa L. A consultation with Mr. James Paget was decided on. He entirely coincided with me in recommending the operation, as without it the case was hopeless. The infant, on its return to Salisbury, caught whooping-cough; in consequence of which the operation had to be put off.

On the 8th of January, 1860, the tumour was so tense, and the covering had become so thin at one point, that I determined to lose no more time. I operated in the following manner, chloroform being very cautiously administered by my partner, Dr. Blackmore:—Instead of an ordinary trocar, I used Wood's syringe, holding half a drachm, with a pointed perforated needle. Having screwed on the needle, and pressed home the piston, I thrust the needle through a healthy part of the skin at the side of the tumour into its cavity, drew out, by withdrawing the piston, half a drachm of clear fluid, unscrewed the needle, and emptied the syringe; pressed the piston home, screwed it on again, and again withdrew half a drachm of fluid, and so on until I had taken out two drachms. The tumour was then very flaccid. I injected, by reversing the process, two drachms of the following fluid:—Of iodine, ten grains; of iodide of potassium, twenty grains; of distilled water, one ounce. The tumour was painted over at once, and daily afterwards, with contractile collodion. The thumbs of the patient were immediately convulsively drawn into the palms, and the toes of both feet pointed. The infant looked very ill, and refused the breast during twenty-four hours. She then rallied, took the breast, and from that hour had no unfavourable symptoms. At the time of the operation the covering of the tumour was so thin that the colour of the solution of iodine was very apparent through it.

From this time the cure proceeded uninterruptedly. The tumour became flatter, its covering grew first mottled by deposition of white matter in its substance, then evidently, though gradually, more dense, until when quiet it (the tumour) did not project more than an eighth of an inch from the surrounding skin, and all its covering, except about the size of a pea, became thick and opaque. The child is, and has been since two months after the operation, well, and runs about as vigorously as other children of her age.

As will be seen by the perusal of the above, the original idea did not emanate from me. The idea of curing spina bifida by the injection of a solu-

tion of iodine originated with Drs. Brainard and Crawford, American physicians. The modification of the operation I introduced was the using Wood's syringe and needle instead of the small trocar. By this, my plan, a very small puncture is made; there is much less chance of wounding nervous tissue; and so small a puncture, if care be taken, could not admit atmospheric air. The puncture healed immediately.

I used chloroform, not only to save the child pain, but to prevent shock, recollecting the experiments described by Dr. Claude Bernard, in which it was found impossible to destroy the spinal cord in frogs without killing the animals, until chloroform was given them, and then they survived the experiment.

The peculiar convulsive action bringing the thumbs into the palms, and causing pointing of the toes, is interesting, as being very similar to the effects of centric or eccentric irritation of the spine.

The case of Rosa M. L. is strongly favourable to the view that spina bifida is essentially a local dropsy of the arachnoid membrane of the spinal cord, and that the fluid, carrying with it the membranes of the cord, either prevents ossification at the point of its exit, or causes absorption of the tender bony covering of the spine by continued and increasing pressure.

If this hypothesis be accepted, the alterative action of the iodine on the lining membrane of the cord is explained, and it would account for the failure of puncture, incision, or excision in these cases. By the injection the disease itself is attacked; by the simple puncture, &c., the effect of the disease is attempted to be removed, but the diseased action is neither altered nor controlled, and danger is run of arachnitis of the cord.

I believe this is the first case related of the kind by an European operator. The injection of a strong solution of iodide of potassium and iodine into a sac communicating directly with the spinal cavity seems indeed a bold proceeding; but, according to my experience, there is a marvellous tolerance of iodine in the animal tissues. Besides, this disease seems otherwise beyond our art.

The iodine injection appears to do in these cases that which nature does in rare instances. It arrests the abnormal secretion of serum. That being done, the covering of the tumour contracts and thickens, so as to protect the membranes of the spine at that point from external injury.

Another mode of radical cure by an effort of nature is by adhesion of the arachnoid at the opening in the spinal column; but it is evident that the abnormal and progressive secretion of serum must first have been arrested, otherwise death must have ensued by pressure on the spinal cord and brain. In such cases of isolation of the swelling, it may be as safely removed as an ordinary tumour, and this, I believe, has been done. It is, however, a hazardous proceeding, as with thickened integuments it is impossible to ascertain that there is not some slight communication with the membranes of the spinal cord, and then arachnitis of the cord is very probable.—*Lancet, March* 3, 1866, *p.* 225.

103.—PRACTICAL REMARKS ON THE LOOSENING OF THE ARTICULATIONS OF THE PELVIS.

The morbid relaxation of the articulations of the pelvis is but the exaggeration of a physiological fact, the object of which is to facilitate the passage of the fœtus. When the ligaments are much relaxed, at the close of pregnancy, the patient is often entirely unable to take walking exercise, and the circumstance may give rise to errors of diagnosis. It may, therefore, not be uninteresting to advert to M. Trousseau's opinions on the subject, such as they are represented in the Union Médicale by M. Peter, formerly the Clinical clerk of the learned Professor at the Hotel Dieu.

Before offering any remarks on two cases observed in his wards, M. Trous-

seau briefly related the history of two ladies in whom he detected for the first time the symptoms of this condition of the pelvis.

At the age of 36, Mrs. X. was delivered of her third child. The accouchement had been in every way regular, but through motives of precaution, on which it is unnecessary to dwell, M. Trousseau required her to remain in bed for an entire fortnight, and to lie on a couch for an equal period. She was then permitted to rise, but when she attempted to stand she experienced a severe pain in the back and pelvis, and for more than a month was unable to walk round the room. Slight leucorrhœa being present, the Professor ascribed the symptoms to a trifling amount of metritis, a frequent consequence of parturition; he, therefore, merely prescribed a few injections. One day, however, the patient having endeavoured to walk in his presence, he was much struck by her attitude; she *waddled*, dragging each leg with difficulty, and bending to the right or left as she advanced one or the other foot. She found it impossible to stand on one leg, and if she endeavoured to do so she fell, and complained of severe pain in the back and hips. Mrs. X.'s extraordinary debility suggested the idea of an affection of the spinal cord, but, on a careful investigation of the state of common sensation and mobility, it was ascertained that the integument was in full possession of all its functions, and that every movement was performed with ease, provided the patient was in the horizontal attitude. It then occurred to M. Trousseau that the symptoms might arise from relaxation of the articulations of the pelvis.

Pressure over the pubes and the sacro-iliac joints occasioned considerable pain. The Professor at once bound the pelvis as tightly as possible in a circular bandage, and the patient, to her great delight and surprise, was immediately able to walk firmly, and without pain. A laced jean belt was then constructed, to support the pelvis and hips, and as soon as this bandage was ready she resumed her usual domestic occupations. After an interval of six weeks the use of the belt was discontinued, and a complete cure was effected.

The second patient was a young woman of 25 who presented similar symptoms; and M. Trousseau, enlightened by his former experience, at once pronounced the case to be one of relaxation of the pelvic articulations, and prescribed a bandage which was worn for two months, and finally consolidated the loosened joints.

At the Hotel-Dieu, a stout young woman, who had recently been confined, was found incapable of standing while her bed was being made. She was carefully examined; the uterus was in a perfectly healthy condition, and when lying down she performed with ease every movement of the limbs, common sensation being at the same time preserved in its full integrity. Even in bed, however, the patient complained of pains in the pubic and sacro-iliac regions. When she moved about she complained rather of a distressing sense of weakness than of actual suffering; she could not stand upright unless her hands had some support; and if she attempted to walk she dragged her legs, and was compelled to rest after every three or four steps, requiring always the assistance of an arm or the prop of the back of a chair, and thus progressed from bed to bed, grasping the iron curtain-rods. No pain was elicited by pressure over the articulations of the pelvis; no swelling and no redness were present. But the joints were perceptibly loosened, and the fact was apparent when attempts were made to test the separate mobility of the ossa ilii.

M. Trousseau then showed that in an instant the power of free locomotion might be restored to this woman. A circular bandage was tightly applied around the pelvis, and she at once walked with entire ease. She remained in the wards during the time required for the construction of a jean belt, and she has since entirely recovered her natural vigour and agility.

On the 12th of July, 1864, a strong, healthy young woman of 24, whose labour had been most favourable, was extremely alarmed, on the ninth day after her delivery, to find herself utterly unable to stand or walk; whenever she attempted to do either she experienced, in the lower extremities, a sense of extreme weakness, a pain which she compared to a bar across the pubic

region, and also a distressing feeling of weight in the small of the back. These uncomfortable sensations also recurred in bed on any sudden effort to turn round. Trusting that she might recover her strength and that the pain would subside under the influence of rest, she returned to her bed for several days, but when she again ventured to rise found she had made no progress whatever. She then determined on applying for admission into hospital twenty-three days after her accouchement.

On examination, the patient was found unable to stand or walk without great pain, and after every effort of the kind she fell back on her bed. The organs of generation were vaguely indicated by her as the seat of the pain, which in reality existed in the symphysis pubis only; here a marked interval was detected between the articular surfaces, sufficiently wide to admit the tip of the forefinger, and, in addition, the cartilage was obviously softened. The exploration having proved extremely distressing, no attempt was made to cause the ossa ilii to move separately, the diagnosis being otherwise fully established.

Cases of this description have been brought forward by many authors. MM. Debout, Courot, Moutard-Martin, for instance, have expatiated on the good effects, under similar circumstances, of tightly-secured circular bandages, and M. Dévilliers has demonstrated in the most peremptory manner, that in recent cases a cure may sometimes be effected by entire repose and astringents. Professor Stoltz, who has also adverted to the subject, opines that the affection often yields to rest and to the tonic virtues of the waters of Baden-Baden. This is, however, a question well deserving of the practitioner's most serious attention, because the loosening of the pelvic joints gives rise to symptoms which have too often been deemed indicative of uterine disease and depletion, and antiphlogistic treatment injudiciously resorted to. We should, however, observe that, as a consequence of the puerperal state, inflammation may sometimes exist as a complication. In this case, the most carefully constructed bandages are not so efficacious as entire repose; but even then, puriform arthritis may arise, and death supervene, as in the case of the late lamented Princess Mary of Orléans, who died at Pisa of an affection of this kind.—*Medical Circular, June* 21, 1865, *p.* 403,

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104.—ON THE MORTALITY OF CHILDBED AS AFFECTED BY THE NUMBER OF THE LABOUR

By Dr. J. Matthews Duncan, Lecturer on Midwifery, &c., Edinburgh.

[In this paper the two following questions are considered: Does the number of a woman's pregnancy regulate in any degree the mortality to be expected from lying-in? Does the age of the child-bearing woman regulate in any degree the mortality accompanying this function? The following are the conclusions arrived at.]

1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.

2. The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.

3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.

4. As the number of a woman's labour increases above nine, the risk of death from puerperal fever following labour increases with the number.

5. If a woman has a large family she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.

The laws, although they merely state coincidences, have very important practical bearings which are too self-evident to require description. They have also important philosophical bearings, which were alluded to in the commencement of this article. The most important, perhaps, of these relate to

puerperal fever. These also I shall not enter upon farther than to say, that the attendance of puerperal fever specially on primiparæ, and women who have borne large families,—its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies,—its subjection also to the law of the duration of labour,—do not appear to me to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, contagious, epidemic. Another point under this head I shall merely mention. Authors, comparing the mortalities of lying-in-institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that, unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly compared with one another.

The well-known protraction of labour in primiparæ may to some appear a sufficient cause of the increased mortality of first child-bearing. But mere prolongation of labour for a few hours cannot, in my opinion, be regarded as any satisfactory explanation of the causation of this increased mortality. In one set of Johnston and Sinclair's cases, the labours of primiparæ are called purely natural, and they are compared with similar purely natural cases in multiparæ; and the mere addition of a few hours to the length of labour in such primiparæ is not a sufficient cause of their mortality being quite as great as that of similar multiparæ. Denman alludes to "a vulgar and pernicious error which makes no distinction between the slowness and the danger of a labour." It would be to fall into this error to explain the increase of mortality merely by increased length of labour.

It must be held as proved, that according as labour increases in length, so the mortality accompanying it increases; and this is true not only of the whole mortality but also of the special mortality from puerperal fever. This law, although it must have weighty bearings on the mortality of primiparæ with their long labours, cannot be regarded as to any great degree throwing light on it; for we find new increments of mortality after the ninth labour, when we have no reason to believe that labour is more prolonged than in labours preceding the ninth, and in which the mortality is less. In other words, we have the number of the labour denoting increase of mortality where there is no evidence of accompanying increase of its duration. The law of its duration, then, does not enable us to explain the variations of mortality in different labours.

To completely exclude the influence of the law of duration would be very desirable; but we see no present prospect of doing this, except by processes of reasoning. Without such, it could only be done by comparing a series of labours of different number, but in all which the duration was the same.

It must be remarked that, while the law of duration certainly has important bearings on the data and arguments herein adduced to show the influence of the number of the labour, and while the extent of these bearings is undecided, it is at the same time equally sure that the law of the number of the pregnancy has important bearings on the data and arguments adduced to show the influence of the duration of labour, and the extent of these bearings is undecided. The mutual influence of the data and arguments in these demonstrations must be great, and it remains for future observers to accumulate materials for either showing the amount of these influences or for a separate demonstration of the laws by data which do not intermingle them in their conditions.

It is worth while to remark, that, restricting for a moment our regard to the great mortality of primiparæ as it exceeds that of multiparæ, taken together, we have a set of cases—those of Johnston and Sinclair—where the deaths were from puerperal fever, and in which the average increase of duration of labour in primiparæ above that observed in multiparæ was 4 hours. In multiparæ the average duration was 8 hours; in primiparæ 12 hours. Looking at this increased duration, and the correspondingly increased mortality in pri-

miparæ, with the light thrown on it by tables published by various authors to demonstrate the law of duration, it appears to me that the increase of mortality in primiparæ is greater than these tables appear to give as the increase corresponding to a rise in duration from an 8 hours' labour to a 12 hours' labour.

These various remarks I have made with a view to keeping the demonstration of the influence of the number of labour on childbed mortality in its proper light, to keep it separate from other laws or supposed laws with which it may be confused. I have alluded, with this view, to the causation of the variations of mortality according to the number of the pregnancy. It is no main part of this paper to enter on this subject, but a few words may not be out of place. It would be foolish to imagine that any injurious influence or the reverse could spring from the mere number of the pregnancy. A woman in a first may and often does have as fortunate a delivery as in any other. To ascribe to the number of pregnancy any potency would be to fall into the error of those students of the duration of labour who ascribe great potency to the mere addition of length to a labour. In the case of the law demonstrated in this paper, and in the case also of the law of the duration of labour, it appears to me that the causation of the variations of mortality is to be looked for in the introduction of complications. I here use the word complications in a much wider sense than is generally ascribed to it, wishing it to imply injuries or injurious tendencies far slighter than those ordinarily classed as complications of labour. I have no doubt that all of these, however minute or slight, have their weight in giving proclivity to a fatal termination of the childbed. Puerperal fever may have its root in an otherwise insignificant perineal laceration, as well as in a phlebitis or endometritis.

In primiparæ, as labour goes on, complications occur which are not nearly so liable to attack a woman in her next subsequent labours. These have their origin in various sources, chiefly in mechanical difficulties, and these often so slight as not to take the case from the category of purely natural, in an arrangement where the labour is alone taken into consideration, to the exclusion of the childbed.

Multiparæ are specially and increasingly liable to complications of a different kind connected with constitutional diseases, and with local infirmities of the uterus.

This introduction of complications forms also the main explanation of the law of the duration of labour. Indeed, in a rough way, it may be held that the statement of duration is a statement of the increase of complications ; for it is known that, as labour lengthens out, so complications increase in frequency. Without these complications duration would be of small importance, as the profession has generally held. Their introduction is present evil and the seed of future disasters. Tables have been framed to show the increasing introduction of complications as labour is prolonged, but I only refer to them. They are quite insufficient, so far as I know them, because they are founded only on an enumeration of those of the graver sort. Further, the introduction of complications is not ruled exclusively by the duration of labour. Many are rather connected with precipitate parturition. The complications which probably contribute largely to produce the increased fatality of labours after the ninth are not all included, or capable of inclusion, in any statement of duration, being present before and after the process.—*Edinburgh Medical Journal, Sept.*, 1865, *p.* 209.

105.—ON LIEBIG'S FOOD FOR INFANTS AND INVALIDS.

By Dr. Arthur Hill Hassall.

In the preparation of this food, the two principal objects at which Liebig aimed were—first, to produce a food which should resemble human milk in the relative proportions of its heat-giving and flesh-forming constituents ; and, second, to reduce it to the state most easy of digestion and assimilation.

It should be clearly understood, however, that the formula given by Liebig,

although it furnishes an article having about the same relative composition as human milk, is yet of twice its strength, or, to use the words of Liebig himself, it contains "the double concentration of woman's milk;" and therefore there is reason to believe that in some cases this food will prove too rich for the infant's stomach, and will require dilution.

It appears to me that the great merit of Liebig's preparation consists in the use of malt flour as a constituent of the food; this, from the diastase contained in it, exercises, when the fluid food or soup is properly prepared, a most remarkable influence upon the starch, quickly transforming it into dextrin and sugar, so that, in the course of a few minutes, the food, from being thick and sugarless, becomes comparatively thin and very sweet. That the action of the diastase on the starch is very considerable is amply proved by the following analysis:—

Uncooked Food.

Albuminous matter	9·25 grains per cent.

Dried Cooked Food.

Albuminous matter	15·84 grains per cent.
Fatty matter	8·49 "
Sugar or glucose	37·73 "
Sugar of milk	10·90 "
Dextrin and starch	27·04 "
Total	100·00

It will be observed, by an examination of the above figures, that a very large proportion of the starch has become converted, in the course of the preparation of the food, into sugar.

Correct and ingenious as are the principles upon which this food has been designed, yet the directions given for its preparation are certainly open to considerable improvement. Thus Liebig directs that the malt should be ground in a common coffee-mill and the coarse powder passed through a sieve. This necessitates the subsequent straining of the food—a tedious operation,—in order to remove the bran and remaining particles of husk. And further, that the food should be put upon a "gentle fire" previous to its being finally boiled. Now, a gentle heat may mean almost any temperature nearly up to the boiling point; and, since the action of the diastase is destroyed at about 150° F., the temperature ought never to be allowed to exceed that degree.

I recommend, therefore, that the malt should be well freed from husk and finely ground; that the wheat flour should be lightly baked; and, finally, that a thermometer should be employed in the preparation of the food. Indeed, in some samples recently submitted to me by Messrs. Savory and Moore, I find that the first two points noticed have been attended to, and that they use malt freed from husk and finely ground, and the wheat flour baked.

The effect of baking the wheat flour is to partially cook the starch entering into its composition, so that less heat is required in the preparation of the liquid food. I find that a temperature ranging between 140° and 148° is amply sufficient to effect the complete transformation and solution of the starch corpuscles, and, indeed, to cook the food sufficiently.—*Lancet, July 29, 1865, p. 135.*

ADDENDA.

106.—ON THE CITRIC, ACETIC, AND CARBOLIC ACIDS IN CANCER.

By Dr. John Barclay, Banff.

In the beginning of September of last year, a paragraph was pointed out to me in a newspaper setting forth the wonderfully beneficial effects which had attended the use of citric acid in a case of cancer; but I did not think of trying it, until a patient labouring under a large and excessively painful and cancerous tumour of the neck, behind the angle of the jaw, which from its size, situation, and the extent of its attachments, held out no hope of its successful removal by the knife, requested me to allow her to make a trial of the acid, as she said her clergyman had strongly recommended her to do so. I at once assented, thinking it could have little effect either for good or for harm. But when I called in a few days after, I was somewhat surprised to find that since she had applied a lotion composed of a drachm and a half of the acid, dissolved in eight ounces of water, she had had almost no pain in the growth. This I was the more astonished to hear, as nothing of the anodyne class seemed to have afforded the slightest relief from the pain before, with the exception of the hypodermic injection of morphia, and even this, she said, did not remove the pain so effectually as the citric acid lotion; and besides, the relief from the latter was much more permanent. This treatment was continued for some weeks, with the effect that the patient improved considerably in looks, health, and spirits; and to show if this was really due to the change of remedy, I ordered her to discontinue the wash for a week, during which period of omission the pain returned with as great severity as before, compelling the patient to resume the application, which again brought relief along with it. It now occurred to me, remembering the solvent power of acetic acid over cancer cell walls, to try what effect that acid would have when applied to the tumour. By this time the skin had begun to give way, and a sore to develope itself on the surface of the growth. I therefore ordered vinegar to be applied, and was glad to find that this controlled the pain quite as well as the citric acid had done before. Dr. Manson, the Senior Surgeon of Chalmers' Hospital, in this place, who had used the citric acid lotion with equally beneficial results in another case of cancer (this time of the breast, and totally unsuited for excision from its extent, adjacent glandular enlargement and the length of time it had existed), now changed it for the vinegar application, and he had no reason to be disappointed, for he found that its anodyne power was equal to that of the citric acid, and also that it possessed advantages which the acid formerly used by us possessed in a very much more slight degree. In the case of the wounds in the breast, which were numerous, and all partaking of the characters of cancerous sores in a most marked degree, it was noticed that after the application of the citric acid, the "thick, serrated and everted edges" did not seem quite so thick, serrated and everted as before, but looked thinner, softer, and with less induration around them. But, and as I hoped would turn out to be the case, the application of the acetic acid lotion, gradually increased in strength from that of common vinegar upwards, produced in this way a much more marked effect, for the edges began to thin down much more rapidly, granulations of a seemingly healthy character arose in the centre of the wound, all fetor disappeared from the discharge, and even attempts at cicatrisation began to

take place, sufficient in several of the smaller ulcers to close them in altogether.

The acetic in varying degrees of strength had now been used for a month in both cases, and looking back over this period, and over the month during which the citric acid had been used, to the condition of both patients previous to the use of these remedies, we were of opinion that not only had their state of health, and the appearance of the disease in each, not become worse, but that both had most sensibly improved. Both patients ate and slept much better than before, and were able to go about their usual household occupations with ease and comfort. The tumour in the neck had become decidedly less, there were attempts at a skin-forming process at several points on the edges of the sore, and pain in it was reduced to a minimum. As for the case of the breast, the woman had so much improved in health, that it would have been difficult to recognise in her the same thin, cachectic-looking creature of three months back, for her cachectic look had wholly disappeared, and no one by looking at her now, could have supposed that she laboured under a disease of such a serious nature, and which had progressed so far. The tumour itself was no larger than it was three months before, perhaps rather smaller, and several of the smaller sores had healed over entirely.

And now it was resolved to try the effect of carbolic acid in the above cases, and this was commenced on December 28th of last year. About this time, two other cases of cancer applied for advice. The one was an extensive tumour of the neck of the uterus, and implicating the whole of the vagina, accompanied by very great pain, and a most profuse and exceedingly fetid discharge. Indeed, so fetid was the discharge, that no one could stay even for a short time in the room with the patient. The other was an enormous schirrous tumour of the breast, of very rapid growth. It had been in existence only four months, and already it extended from the floor of the armpit almost to the sternum. The subject of it had been in one of the largest of our provincial hospitals, and had got nothing, either there or anywhere else, which gave her any relief from the extreme pain and the horrible fetor of the discharge. The carbolic acid in the form of a very dilute lotion* was ordered in all four cases, with the following results:—

In the case of the tumour behind the jaw, the lotion was about as effectual in relieving the pain as either the citric or acetic acid lotions. Applied in this weak state, its solvent effect was much the same as that of the citric acid; but applied in a more concentrated form, the effect was a most vigorous eating away of the tumour, and with much greater rapidity than by the two acids formerly used. But there were very feeble attempts at skinning under the use of the dilute carbolic acid in this case, and none of course when the strong acid was employed.

In the case of the mammary tumour, which had been treated before by the citric and acetic acids, the report was that the pain was as effectually controlled by the carbolic acid as by either of the other two; that under its use the "thick, serrated and everted edges" disappeared much more rapidly than with the other two; and that when the weak solution was employed, cicatrisation was seen going on over many of the sores, wherever the cancerous excrescences were eaten down to below the level of the surrounding skin. In the case of the disease of the uterus and vagina, the effect was equally striking; whenever the weak lotion was employed the pain almost entirely disappeared, and with it the horribly offensive discharge. The poor woman, from wishing herself dead, began to have her spirits raised, to eat and sleep well, and now no fetor was perceived by those in the room with her. And the general improvement in her appearance was beginning to be visible when a severe attack of hemorrhage nearly carried her off, since which time her progress towards recovery has been very slight.

A like result was obtained in the fourth case, that of the large mammary tumour. The pain, instantly on the application of the carbolic acid lotion, disappeared as if by magic, and the fetor of the discharge was very much

* ℞. Acidi carbolici, ʒ ss—ʒ ij.; spiritus vini rectificati ℥ xx.

lessened. The tumour was extirpated a few days after the application of the acid, and the case has gone on well since.

Two or three more cases of cancer of the breast had the carbolic acid lotion applied, but the patients live at a distance, and no report has been received from them.

I may mention that, on treating cancer-cells under the microscope with acetic and carbolic acids in varying degrees of dilution, I found that in about equal strength the carbolic acid dissolved the cells much more rapidly and effectually than the acetic acid, and caused the nucleus also to disappear almost entirely when applied in a concentrated state.

From the above experiments with the three acids, then, it appears that they have about an equal effect in removing pain in cancerous growths; that the carbolic acid has a powerful effect in correcting the offensive fetor of cancerous discharges; and that they all have a solvent effect on cancerous tissue—the citric acid least, the acetic next in degree, and the carbolic most powerful.—*British Medical Journal*, *April* 21, 1866, *p.* 409.

107.—ON THE USES OF THE MEDICINAL SULPHITES AND HYPOSULPHITES IN ZYMOTIC DISEASES.

The extent and value of Dr. Giovanni Polli's researches, experimental and clinical, as to the uses of the medicinal sulphites and hyposulphites in zymotic diseases have not been duly appreciated in this country. With the cholera impending, and typhus still prevalent, and the cattle plague not yet "stamped out," we shall do good service by calling special attention to the results of Dr. Polli's labours as stated in his latest papers on the diseases caused by a morbific ferment and their treatment, communicated to the Royal Institute of Lombardy, at Milan. His first paper appeared in the eighth volume of the *Memoirs* of the Institute, and described researches into the chemistry of morbific ferments and their treatment by anti-zymotic chemicals. Whatever be the theory as to the cause of the so-called zymosis (and either Liebig's albumenoid, or Pasteur's animalcular, or any other theory may be adopted), the practical facts, according to Polli, are, that there are two morbid elements in the blood in these zymotic diseases; the one is of the nature of a ferment or excitor of change, the other is the material capable of fermentation. When either of these is absent from the blood, zymosis is impossible; but when both are present, then such a reaction takes place that zymotic phenomena result. Now, the ferment may be rendered innocuous, although it may not be readily destructible itself, by destroying the fermentable material, or by at least so changing its composition that fermentation is effectually hindered. Anti-zymotic agents are already numerous, the majority of which seem to act both upon the ferment and the fermentable material, and would therefore seem *d priori* to offer all that is needed.

But the very qualities upon which their anti-zymotic powers depend, render them dangerous in practice, because they are for the most part efficacious in proportion as they are destructive to the vital processes themselves. A safe anti-zymotic was, therefore, still needed. After a series of experimental researches, Dr. Polli affirms that he has found in the sulphites and hyposulphites of potass, soda, and magnesia, medicinal agents which do not act upon the ferment, nor upon the vital processes, but upon the fermentable something without which the ferment is innocuous; and in this way they constitute safe and efficacious anti-zymotics. In the memoir under review he indicates the diseases in which they may be administered either for prevention or cure—their doses, modes of administration, and in short entire natural history, as therapeutical agents of their class.

The fundamental fact of his researches is this:—He selected a number of dogs; a proportion of these he dosed with the sulphites, while the remainder were left without. He then killed them all, the weather being warm. He found that the urine, the blood, the viscera, and the flesh of the sulphited

dead dogs resisted putrefaction for many days, while the same elements of the unsulphited animals were already putrid. The obvious conclusion from so striking a fact is, that as the sulphites prevented putrefactive fermentation in the dead body, they would *à fortiori* prevent it in the living. He accordingly instituted a series of comparative experiments on the living. He selected two or three dogs as nearly equal in size and state of health as possible, and inoculated them with the nasal mucus of a glandered horse. One of the dogs had had previously administered to it from four to five scruples or more of the sulphites of soda or of magnesia daily for some days; another was treated by sulphites as soon as inoculated; a third was left without treatment, either prophylactic or curative. The result was that the third animal suffered much more severely than the other two. The like results followed after injecting pathological ferments into the veins, such as fresh and putrid pus, and putrid blood. Encouraged by these results, Dr. Polli proceeded to test the effects of the sulphites and hyposulphites on men in health and disease. He found in man, as in dogs, that they are not ordinarily decomposed in the stomach, but pass unchanged into the blood, or at least appear in a brief period unchanged in the urine, and only after a while as sulphates; that they may be traced in the organism for from 24 to 36 hours after being taken; that they slightly increase the renal secretion, and are mildly cathartic in large doses.

The clinical researches extend over a series of local and general forms of zymosis, and have been conducted chiefly by Italian physicians. That the salts have been extensively used is deducible from the fact that three chemical houses in Milan have sent out more than 1000 kilogrammes (2204 lbs. avoirdupois) of the sulphite of magnesia—a salt which Dr. Polli says can have no other use in the arts and pharmacy in Italy than those he has indicated. We will note the uses in the order Dr. Polli arranges them.

External Uses.—In all suppurations, sloughing, and ulcerations, the solutions are useful in soothing pain, removing fetid odours, and exciting healthy action. The sulphites of soda and potass are the best; they neither stain nor corrode the linen, as the solutions of metallic oxides, and being without smell are not offensive, like the chlorides, phenic acid, &c. They have the advantage, also, of being highly soluble. In gangrene and sloughing, foul ulcers, and suppurations of all kinds, a more or less concentrated solution of the sulphite of soda is the best. This salt is soluble in four times its weight in water, but it may be advantageously used in the proportion of one part salt to five to ten of water. The solution may be applied as a wash, or lint or a compress wet with it, or it may be injected into sinuses and sloughing cavities. The solution may also be mixed with glycerine, and the powdered salt may be sprinkled on the part affected. The sulphites of lime and magnesia have a somewhat caustic action, and are, therefore, best adapted for indolent ulcers. The immediate effect of the salts is to arrest putrid fermentation. In cases of purulent absorption (pyæmia) and septicæmia the internal use should be combined with the external. Dr. Polli recommends the sulphite of magnesia in thirty-grain doses every two hours.

2. *Pyæmia* and *Septicæmia*, and all the class of fevers, with putridity and purulent absorption, such as hospital fever, puerperal fever, anatomical inoculation, and the like, have been treated by the sulphites with advantage. According to the clinical observations of Professor Burggraeve, of Ghent, it is necessary in those cases to give the sulphite of magnesia, so as to saturate the organism rapidly with it, and cause the fluids of the body—urine, saliva, sweat—to contain it. Half-drachm doses every one or two hours may be given with this purpose. Reports from twelve Italian physicians testify to its beneficial effects in ameliorating all the symptoms of 18 cases of purulent infection. Dr. Polli recommends it to be given as a prophylactic in surgical cases, bad cases of labour, &c. From four to six drachms of the sulphite of magnesia or the hyposulphite of soda may be given during the first 24 hours in divided doses, and from six to nine drachms during the next 24 hours to prepare a patient for an operation or a labour. If, when the patient is attacked, there is excessive diarrhœa and vomiting, these must be relieved,

otherwise the system will not be saturated, or else clysters of the solution must be administered. A little caustic magnesia added to the solution of the sulphite of magnesia causes it to remain better on the stomach. For the diarrhœa opium may be combined. After a while, the remedy being retained will take effect in relieving the diarrhœa, and the opium may be discontinued.

3. *Endemic* or *Miasmatic Fevers* are numerously reported on by Dr. Polli's correspondents; in some instances in a tabular form, as to hundreds of cases of quotidians, tertians, and quartans treated by the sulphites. Comparative trials of quinine and sulphites were made by some physicians both for the prevention and cure of intermittents. The results of those observations are very striking. The sulphites were found to cure a larger number of cases than quinine; to cure more completely and with less tendency to a return of the fevers, and to cure radically cases that were utterly rebellious to quinine. In others of this class, the conjoined administration effected a cure when neither cured singly. The sulphites do not, like quinine, cut short the fevers; they gradually diminish the intensity of the symptoms. The sulphite of magnesia is the most efficacious, and may be taken to the extent of an ounce or an ounce and a-half in the twenty-four hours in divided doses, either in a cup of water, or in sugar, or any confection. It may be taken during the accession as well as during the remission. A change for the better, if the remedy is likely to be efficacious, is manifested in from three to five days. All acids and acidulous fluids are incompatible with the sulphites, and should therefore be forbidden. To prevent malaria taking effect, Dr. Polli recommends a solution of the hyposulphite of soda to be taken in any vehicle which serves to mask the bitter taste of the salt. A prophylactic mixture (the "*liquore solfitico antifebrile*") of about a litre or $1\frac{3}{4}$ imperial pints, containing fifty doses, may be thus made:—Hyposulphite of soda, 7721 grains; water, 9765 grains; tincture of essence of anise, 2316 grains. Two small table-spoonfuls to be taken every morning fasting in a cup of water. It may act on the bowels for the first day or two; but this effect ceases in a while, and the remedy is borne perfectly well, although a good deal of sulphuric acid is formed in the intestine. When the remedy is not effectual in a few days, in a case of intermittent, quinine or iron may be combined. Its prophylactic value is increased in anæmic persons in the same way.

4. *The Exanthemata and Typhus Fevers.*—Dr. Polli suggests the use of the sulphites in yellow fever, and prints thirty-seven reports from numerous correspondents of their value in measles, scarlatina, small-pox, erysipelas, and typhus. The general results reported are to the effect that they mitigate the intensity of the febrile exanthem, render the confluent and malignant forms benignant, shorten the course of the milder forms, and in all cases accelerate convalescence. In typhus gravior with fetid dejections and putrid symptoms, they are found useful. Dr. Polli does not venture to assert that they will prove available in the cure of cholera and plague; but he thinks there is reasonable ground for concluding that they will be highly useful as prophylactics in destroying the fermentable material upon which the malignancy and fatality of those pestilences chiefly depend, and thus rendering an inevitable attack mild. They may have no effect on the ferment, but they will deoxidise the fermentable stuff. In all these zymotic diseases the same principle applies.

5. *Syphilitic Infection* and constitutional syphilis have been efficiently treated by the sulphites, but facts are not given. Pellagra, too, and tuberculosis, it is thought, may be benefited.

6. *Cattle Plague and Epizootics.*—Dr. A. Colaprete, di Campo di Giove (the Abruzzi), has used it in the cattle-plague, and cured a third of the animals attacked. And Dr. Polli refers to other instances in which the sulphites have been administered in epizootic aphtha of cattle.

7. Finally, Dr. Polli speaks favourably of the uses of the sulphites to anatomists and others. Corpses or viscera can be preserved for many weeks in a solution of sulphite of magnesia or of soda, without change either in color or density, or otherwise, and without acting upon the scalpel or other anatomical instruments when dissection is made; in these respects being

superior to alcohol, arsenic, corrosive sublimate, or the sulphates and chlorides of iron, zinc, &c.

Some hints as to the administration of the sulphites may be useful to those who may be induced to try them in the various diseases in which Dr. Polli recommends them. They appear in the urine twenty minutes after being taken into the stomach unchanged, but are gradually changed in the system into sulphates. The saliva and sputa contain them. They are not decomposed in the stomach under ordinary circumstances, but when they are, there is a manifest production of sulphurous acid gas. When this is the case a little magnesia must be added to the sulphite to neutralise the acids of the stomach. The sulphites of soda and magnesia are tolerated in large doses if dissolved freely in water. A concentrated solution is apt to lie heavy on the stomach and cause a frontal headache; the more diluted, as a general rule, the better. They are decomposed by all the vegetable acids, however weak.

Hence their beneficial influence will be counteracted if the patient takes citric, tartaric, malic, or oxalic acids, or food or drinks containing them. This is important to remember when administering them in fever, inasmuch as lemonade, imperial, apple tea, and the like, are all incompatible drinks, and destroy their efficacy.

The hyposulphite of soda is much more purgative than the sulphites of magnesia and soda, which are commonly more diuretic than cathartic in their action. There are individuals, however, whose intestinal canal is very sensitive to their action, in which cases a carminative and sedative are required to be combined. The purgation is, however, most commonly without pain or discomfort. The stools of those taking the sulphites lose all cadaveric or nauseous fetor. Upon the whole, the best mode of administering the sulphite of magnesia is in the form of powder, because of the large quantity of water it requires for solution, and because in that state it is most easily altered by the air. This sulphite has the least taste, and is the most active of all, and since it undergoes in the dry state the least change, it is to be preferred to all the others for medicinal purposes. From fifteen to thirty grains may be prescribed in powder, to be taken either in water or other vehicle, or it may be covered with gum or dextrin, and sugared and taken as troches or jujubes. The sulphites are better than the hyposulphites when a rapid curative action is required; but the latter may be advantageously substituted for the former when prophylaxis is aimed at. The great aim in administering them curatively is to saturate the system with them; and for this purpose four to five drachms daily must be the minimum quantity for an adult. Three or four times that quantity of sulphite of magnesia has, however, been administered with advantage; and a third or a fourth more (five to seven drachms) of the sulphite of soda will be borne well. Their long-continued use is apt to induce œdema and diseases of debility by their deoxidising qualities; otherwise, they have no influence on the system.

The numerous researches made by Dr. Polli and his correspondents merit careful consideration and trial by the Profession in England. Our experience, so far as we have tried the salts, is in favour of Dr. Polli's statements. It must be carefully remembered, however, that the sulphites cannot be fairly tested without strict attention to the conditions under which they can be reasonably expected to be available, and with such precautions as are required by a consideration of their chemical qualities and medicinal properties. —*Med. Times and Gazette, May* 5, 1866, *p.* 475.

108.—DR. HASSALL'S FLOUR OF MEAT.

[This material consists, in the concentrated form, and with the exception of the water, of the whole of the constitutents of the meat previously freed from bone and visible fat; and is prepared at such a low temperature that these constitutents have undergone no change whatever.]

Furthermore, in consequence of the nearly impalpable character of the

powder, no mastication is required, and no greater effort in swallowing than is necessary in the case of any liquid, It is impossible but that such a material should possess high dietetic value, and indeed the view originally entertained by me of its importance has by further experience and experiment become greatly confirmed.

It is, however, not only necessary that an article of diet should possess intrinsic merits, but it must also be pleasing to the eye and palate. I am glad to state that these requisites are entirely fulfilled, and that with the condition in which the several preparations are *now* manufactured I have to express my complete satisfaction; while I unhesitatingly affirm that no article of preserved food hitherto brought out contains an equal amount of nutriment, and embraces such a combination of favourable qualities, as does my flour of meat. The samples first issued had a certain dryness or roughness, and there was a deficiency of the gelatinous element of the meat, which, moreover, had a disposition to sink in the liquid as a sediment. These objections, entirely due to *mechanical causes*, have now been completely obviated.

But the principal object of my present communication is to point out certain uses of the meat-flour, in addition to those referred to in my previous letter:—

1. A spoonful of it added to ordinary beef-tea or soup will greatly enhance its nutritive properties.
2. When taken as a sandwich, or sprinkled over or mixed up with vegetables, it is often retained by the stomach when solid meat could not be taken, or, if taken, be rejected.
3. In all cases in which food has to be administered by means of the stomach-pump, as in many cases of insanity, when made into beef-tea, soup, or cocoa, it will be found a most valuable nutriment.
4. A similar remark applies where nourishment has to be conveyed into the system by enemata.
5. In all cases in which mastication is impeded from any cause—as from disease of any kind affecting the action of the jaws, or from defective teeth—it affords a ready means, as yet unequalled, of giving nourishment.
6. It is most valuable as a nutriment after parturition.
7. It is found most serviceable in many cases of dyspepsia.
8. There is no form of nourishment short of the solid meat itself so suitable in diabetes. It is scarcely necessary, however, to remark that the meat-flour should be used pure, and that neither starch nor sugar should be added when preparing from it beef-tea or soup.
9. It promotes nutrition, and so hastens recovery, in low fevers.—*Lancet, April* 28, 1866, *p.* 469.

109.—ON THE CONCENTRATION AND PRESERVATION OF MEAT.

By Dr. Arthur Hill Hassall.

Of every four pounds of the flesh of fresh beef or other meat, freed from bone and visible fat, nearly three-fourths, or three pounds, consist of water, the remaining pound containing the whole of the constitutents of the meat—viz., the albumen, fibrin, gelatine, interstitial fat, creatine, creatinine, sarcin, the various phosphates, and other salts of the blood, both organic and inorganic, &c.

It long since occurred to me that, if I could succeed in removing the whole of the water of the meat without essentially altering its composition, I should obtain a material but little prone to change or alteration by keeping, and possessing a high dietetic and medicinal value. For some years, but more especially during the past few months, I have been engaged in conducting a series of experiments with this object, and, I am happy to be enabled to state, with complete success.

Into the details of the process it is unnecessary here to enter at length.

I may state, however, that the meat is dried without the loss of a particle of any one of its constituents, for the most part at a temperature below the coagulating point of albumen; and that the product so obtained is then ground and dressed into a very fine powder, constituting, in fact, "*a Flour of Meat.*" This powder, when prepared from beef, is of a light-brown colour, of a very agreeable taste, and one pound of it represents about four pounds of the flesh of meat, or six pounds of the leanest joints not deprived of bone and fat; and the purposes for which it is more especially adapted are—

1st. For the speedy preparation of beef-tea.

2nd. With the requisite vegetables and flavouring substances, for the speedy preparation of soups.

3rd. Combined with farinaceous matter, it forms a highly nutritious food, well adapted for children, the dyspeptic, and invalids.

4th. Mixed with cocoa, it furnishes a highly nutritious breakfast beverage.

5th. With other suitable ingredients, it forms an admirable meat biscuit.

Recent investigations have fully established the fact that clear beef-tea and soups contain exceedingly little solid matter, and that they are, in fact, destitute of all the nitrogenous and protein principles of the flesh, excepting gelatine, which is present chiefly after prolonged boiling, and, consequently, they are capable of affording in themselves, notwithstanding their agreeable flavour, scarcely any nourishment.

The beef-tea and soups prepared with my concentrated meat, beef, veal, a mixture of these, or any other suitable kinds of meat may be partaken of in two forms, either in the clear state, when they will resemble somewhat closely the clear beef-tea and soups as now prepared, but they will differ from them in their greater strength, and in the rapidity with which they may be prepared; or, as I would more particularly recommend, they may be consumed while still containing the powder of the meat suspended in them.

The saving of time in the preparation of beef-tea is obviously a matter of considerable importance when invalids are concerned, and it is of no less consequence to the cook in the case of soups, the length of time required in their preparation being a very great obstacle in most families to their frequent consumption. The various vegetables and seasonings required for soup are likewise prepared in the same manner as the beef or other meat, and these are added to the meat in certain proportions, so that the cook has nothing to do but to allow the mixture to simmer for a few minutes, when the soup is ready for table, the solid materials being strained away if it should be desired that it should be clear, but allowed to remain if greater nourishment be required.

It is almost impossible to over-estimate the importance of the concentrated meat to invalids. By it they are enabled to receive into the stomach the whole of the constituents of the meat, no mastication being required, and no greater effort at deglutition than is needed for swallowing a liquid; and the material is, moreover, presented to the stomach in the form most easy of digestion. It is not too much to say that it will doubtless be the means of saving many lives; and I am happy now to be able to announce that the several preparations and combinations to which I have so briefly referred in this communication are now being largely manufactured, and that they will shortly be submitted to the profession and the public for their approval and judgment.—*Lancet, Feb.* 17, 1866, *p.* 185.

110.—ON THE PRESERVATION OF MILK.

By Dr. J. Scoffern, Kensington Park.

[As by far the greater portion of milk used in cities and large towns is obtained from the country, any means of preventing the putrescent changes to which it is liable during hot weather are especially valuable.]

Premising that the agent used is sulphite of soda, let me state that some years ago I was much interested in the employment of sulphurous acid

and the sulphites in processes of sugar manufacture. In the course of my investigations, I desired to obtain a specimen of cane juice from the West Indies unchanged, and easily accomplished the desideratum by causing a specimen of juice to be impregnated with sulphurous acid. It bore the voyage from Barbadoes to Cork unchanged, as best evidenced by the fact that the normal weight of sugar was extracted from it. So strong an evidence in favour of the anti-fermentive qualities of sulphurous acid was impressive. Therefore when lately consulted by a London purveyor of milk relative to the troubles he experienced from fermentive and putrescent causes, the experiment with sugar-cane juice naturally came to mind. For many reasons, some of which will be obvious to your readers, uncombined sulphurous acid would in the case of milk have been ineligible. Sulphite of lime was first used, but with indifferent success. Sulphite of soda has answered perfectly. Necessarily winter is not so favourable a time as summer for establishing a crucial test. In the opinion, however, of the milk purveyor, who has tried the experiment, the results are conclusive. Judging from previous and collateral experience, I am induced to believe that one teaspoonful of saturated aqueous solution of sulphite of soda added to every gallon of milk will preserve the latter from change, for many weeks at least, in the hottest days of summer.

My medical brethren need not be reminded that sulphite of soda in the proportions noted is quite harmless, and indeed imperceptible. It is not new to therapeutics as a valuable agent in pyrosis and certain forms of dyspepsia, though in these cases I find the sulphite of lime to present advantages. Collaterally I may state my belief that the sulphites of soda and of lime are amongst the best, if not the very best, of all disinfectants. I would suggest their use in the cattle disease, not merely as external disinfectants, but therapeutically administered to affected animals.—*Lancet, April* 28, 1866, *p.* 475.

111.—A CASE OF RESUSCITATION, AFTER DROWNING, BY A COMBINATION OF THE PLANS OF DR. SILVESTER AND DR. MARSHALL HALL.

An interesting example of resuscitation by perseverance in artificial respiration is furnished by the following case, for notes of which we are indebted to Mr. W. H. Ellis, house-surgeon. It contains, besides, a useful hint upon the importance of careful observation of a patient for some time afterwards, owing to the tendency which exists for the circulation to be at first imperfectly performed, and probably accumulation of black blood upon the brain thereby occasioned.

E. C., aged twenty-five, artificial flower maker, was brought to the Great Northern Hospital about half-past twelve on the morning of Jan. 28th, 1866, having been taken out of the Regent's Canal in the Caledonian-road. When first called, Mr. Ellis was told that the woman was dead, and to all appearances she was so, no pulse or respiration being perceptible. However, with the assistance of some gentlemen from St. Bartholomew's Hospital, who were in the building at the time, the wet clothes were quickly removed, and artificial respiration commenced after the plans recommended by Drs. Silvester and Marshall Hall: that is to say, at the time the arms were brought down in contact with the chest according to Silvester's method, the body was half rolled over according to Marshall Hall's plan; at the same time measures were taken to keep the patient's mouth open and her tongue well drawn forwards, and endeavours made to excite respiratory action by the application of ammonia, &c., to the nostrils. After persevering in the above methods for between ten and eleven minutes, the operators were rewarded by symptoms of returning animation, the patient making a very feeble attempt at an inspiration. The process being continued for a few minutes longer, she was sufficiently restored for brandy, ammonia, &c., to be administered. After this she was removed to bed and wrapped in blankets. Hot-water bottles

were then applied to the thighs, axillæ, and feet, with mustard poultices to the abdomen and calves of both legs. Friction with hot flannels was now resorted to in order to keep up the very feeble and intermittent pulse, and this being continued for an hour, she was left in charge of a nurse, with instructions to continue the administration of beef-tea, stimulants, &c. At about two A. M. the nurse reported that the woman was dead. This proved to be a false alarm; but she was found in a state of the most perfect collapse, both respiration and circulation apparently being at a standstill. Artificial respiration was again resorted to, and after a few minutes she gradually revived. From this time till four A. M. she was kept sitting up in bed, constant friction being applied to the trunk and extremities; for no sooner did she lie back in bed than her pulse was imperceptible, and respiration seemingly gone. After four o'clock she improved rapidly, the respiration and circulation evidently becoming stronger and more regular; and about half-past five she was allowed to sleep, as she was getting warm and breathing well. About nine A. M. she seemed quite well. She left the hospital two days afterwards, not having had a bad symptom.

The chief difficulty experienced in applying Dr. Silvester's method was in overcoming the extreme rigidity of the muscles.—*Lancet, May* 5, 1866, *p.* 483.

112.—ON THE ACTION OF FUNGI IN THE PRODUCTION OF DISEASE.

By Dr. Tilbury Fox.

[There are many questions connected with the action of fungi in the production of disease which are most interesting as well as important. Thus it is questionable whether these so-called parasites are vegetable or not in nature, and whether they are distinct species, or only varieties of one species. It is now admitted by all that the fungi are really of a vegetable nature, and that their germs are derived from the exterior.]

The mode of entry of the Fungus into the system.—There is no difficulty in accounting for the access of germs to living bodies, for these germs are freely distributed and disseminated in the air. The best illustration of this fact may be noted in the experiments of M. Bazin (Gazette Méd. de Paris, July 30, 1864), which consisted in passing currents of air over the head of a favus patient, and thence over the open mouth of a jar containing ice. The ice cooled the air, causing the deposition of moisture, in the drops of which the achorion sporules were detected. The same thing may be shown by holding a moistened glass slip near the head of a patient, and just rubbing his scalp freely. Of course, actual contact is much more effectual in the implantation of germs. But, without delay, let us suppose that the sporular elements find their way to the human surface; how get they deeply into the tissues? In various ways probably. Let us take a general sketch. The greater the degree of moisture and heat, the better is the chance of entrée.

First of all, the fungus elements may enter by fissures or natural orifices; for example, in ordinary ringworm the sporules lodge themselves at the opening of the hair follicles, and presently get beneath the epithelial scales. We shall see, directly, how. A great many experiments have been made at different times, upon this point, in the case of plants. De Bary (Die gegenwartig herrschende Kartoffelkrankheit, ihre ursache und ihre Verhutung, Leipsic, 1861) found that, in terminal filaments of potato mould, so-called zoospores were formed, which bud, protrude filaments forming a mycelium which has the power "of penetrating the cellular tissue in twelve hours, and when established there it bursts through the stomata of the leaves." This "boring" operation is quite likely to occur, especially where the structures are diseased; as, for example, the muscardine in silkworm, in diseased mucous surfaces or epithelial changes; here the entrance by continuity is

easily accomplished by the growing filament. It has been supposed that mycelium may get within the shaft of the hair in some part of its course, in this way. I do not believe it. If a fungus finds an entrance, it is either through a cut end, a distinct fracture, or, what is usually the case, the soft growing root. A good deal of doubt has been expressed as to whether the spores could find their way into the interior of plants through the stomata. It seems pretty clear that the latter are not sufficiently large for the occurrence. There can be no question, however, that, in a large number of instances, the spores send out little processes, which get into the plant through the stomata. Here the recent experiments of De Bary help us again. They are noticed by Mr. Cooke in his admirable popular work on microscopic fungi. The observer took a large number of common garden cress plants, placed their roots in water containing zoospores, and though the former became covered with these latter bodies, yet not a jot of evidence of penetration occurred. De Bary, however, found that if the cotyledons or seed-leaves are watered with fluid containing zoospores, that slender tubes put out by the zoospores, enter the stomata, the terminal ends enlarge, branch out, and become the centres from which a ramifying mycelium is produced, which presently shows itself externally. De Bary tested 105 plants in like manner and under similar circumstances, with water free from zoospores, and without the production of any sign of rust in these. De Bary concludes that plants are not infected by spores entering through the roots or leaves, or through the medium of the seed leaves of cotyledons. But it is probable that the fluid contents of the spore cells may be absorbed and give rise to disease. The Rev. Mr. Berkeley, several years ago, found that the germs of bunt, placed in contact with seeds, infected them, without there being any evidence to show that any spore or mycelial thread had effected an entrance. It seemed as if the granular fluid contents were taken up by the plant and caused mischief. It is possible that minute threads might have penetrated the seeds nevertheless. There is, however, no difficulty in supposing the granular contents of spores (sporules) capable of reproducing the typical spore. But, in the next place, there cannot be a doubt but that in the human subject the germs of the fungus find their way to the roots of the hairs, and are carried bodily upwards into the shaft in the process of growth, developing as they go, till at last they degenerate and break up the fibrous structure in which they are. By analogy we should quite expect that such a thing is possible, and indeed, of frequent occurrence in the case of the tender roots of plants; and this is more likely to happen when the contents of the original spore (which is as large as the spongiole cell) happen to be discharged by bursting. Moreover, it is quite clear that the germs of parasites enter at a much earlier period than we are apt to imagine, and lie dormant, brooding mischief till the favourable opportunity arrives. De Bary proved this in the case of the white rust (cystopus) which hybernates as it were in the sub-epidermal structures during the winter, till the spring arrives. In addition, the fungi "make head," so to speak, into structures in virtue of the chemical action which they set up. This is best seen in the hard structures of animals. Carbonic acid is given off at the terminal cells. This dissolves the lime of the shell and allows the parasite to effect an entrance most easily. The experiments of Wittich, quoted by Robin, all tell in the same direction. Panceri has come to the conclusion, however, that, in the case of the egg, the minute germs effect an introduction through minute microscopic holes which exist in the shell. *Lastly*, traumatic lesions afford an easy channel for the conveyance of fungi to deep structures. This is what happens in the mycetoma or fungus foot of India.

We have then, as modes of entrance—(1.) That through natural orifices; (2.) That in which the growing force forces the mycelial thread beneath the layers of the superficial tissues; (3.) That in which processes shoot out from the spore and enter by such openings as stomata; (4.) That where the cell contents are absorbed; (5.) That in which the spores are carried bodily inwards by growing parts; or (6.) dissolve away the opposing structures by chemical action; or (7.) enter by traumatic lesions. In each and every

instance the germs of parasites are derived *ab externo* and not generated *spontaneously*.

There are some special circumstances that deserve comment. It has been asserted that microscopic entophytes have been discovered in close cavities utterly cut off from communication with the external air. But these instances are open to grave objection; fungi have been found in the fluid of the ventricles of the brain, which, however, was allowed to stand all night exposed before it was examined. Again, it is asserted that in the kidneys the like has been found. This is open to exactly the same objection. The case of the egg parasite has been explained away by Panceri; and it has yet to be shown, supposing the urine has ever during life contained fungus elements, that air cannot enter the bladder. The case of germs of vegetable nature in the blood current presents some difficulty; but even here the most considerable caution is needed. We know that fungi spring up with enormous rapidity; and it must be proved that those spores and mycelia are present at the moment of death, nay, during life, before we can give credence to any theory which asserts that they have been present and introduced during life, and not by a communication with the external air. It is still a question whether the endosmotic action of the villi may not be able to account for the presence of cryptogams in the blood current. As far as the facts of vegetable parasitism go, we are bound to deny any such occurrence. And, upon analogical grounds, I venture to assert that the entozoa found in muscle, which have lately caused no little sensation, are not vegetable in nature. Should they be proved so, it will entirely alter the whole subject of vegetable parasitism; for we are justified at present in asserting that there is probably no known instance of a growing plant in any situation not in direct or possible communication with the air. I am bound to say that Dr. Thudicum believes in the vegetable nature (see Report of Medical Officer of Privy Council for 1865) of the rinderpest entozoa (?).

I pass to the consideration of the part played by fungi in diseased states. Two theories the most opposite in intention have been held by writers and others, so opposed that really the conclusion is forced upon one that both *must* be wrong and a middle belief correct. Whilst one batch of inquirers affirms that parasites are accidental, another contends that they are the essential cause of those diseased conditions found in "association" with their growth. Ehrenberg, in speaking of organised parasites at a time when the exact nature of many of them was indistinctly recognised, said, "that there is more cause for wonder at the limitation of their effects by the actions of living bodies they inhabit, than at any morbid effects they appear actually to produce." It must first of all be noted that there are certain conditions which are peculiarly favourable to the growth of vegetable parasites. The latter are ubiquitous, capable of resisting the action of heat, cold, and decomposition, have a tremendous and rapid power of increase, and will remain for a very long time in a state of inactivity; yet, notwithstanding all this facility, there are certain states of organisms against which they fail; which will somehow resist their inroad and attacks; and it is now clear that though parasites may for the moment get a temporary hold, yet they will not flourish upon a typically healthy surface. This is a fundamental truism that must be observed in reference to therapeutics. For rusts and mildews prevail in direct ratio to the wetness of the season, or after drought, as in the pea or hop; damp itself is very favourable, and where there is much drought the vigour and the circulation of plants are diminished very considerably. When plants are very ripe also, there is a less degree of vitality present, in consequence of the cessation in great extent of the circulation and vital connexion between the fruit and the stem. The same thing holds good in every instance where animals, plants, or men are attacked. We may instance the case of muscardine. The experiments of Claude Bernard also showed that frogs kept in captivity got out of order, and aphthous conditions arose. A healthy frog brought near its diseased fellows "set contagion at defiance," but unhealthy frogs were at once attacked by the vegetation flourishing on the aphthous surfaces of others; and the case of favus in man, or scab in sheep,

of which an account may be seen in the Gardener's Chronicle for April 24, 1864, is illustrative of the fact under notice.

There is always a certain resistant power about all healthy living beings; and a certain amount of fungus, however it acts, may be present without giving rise to what one can possibly call disease. In young life, of course, one would expect that fungi would obtain a hold more effectually than in old life; and it is very remarkable that the white rust before referred to, according to De Bary's experiments, should effect an entrance into the system of the garden cress, by attacking the young leaves or cotyledons. The young and tender stage becomes an easy prey; and this is exactly what we find in the human subject, the young being most liable to ringworm.

Taking all things into consideration, it is clear that parasitic disease, or—as I have named it generically *tinea*,—cannot be explained by either of the conflicting theories I have referred to, but consists of three distinct components, which must be recognised, if the physician would cure his patient well and quickly.

1. A certain state of soil: in speaking of polymorphism of fungi, I noticed that each fungus appeared to require each its special kind of pabulum.

2. The access of air, and the presence of heat and moisture,—the conditions necessary to support the life of fungus. And,

3. The introduction from without to and action upon the body of the vegetable germs.

The first and second will be passed over without comment: my remarks are specially intended to define the action of the parasite in the production of diseased states. Now fungi are not "accidental" and unimportant, but act in several distinct ways when once they take hold and grow upon the surface. This is important; if we insist upon some *one modus operandi*, we shall assuredly find our position utterly untenable. They act then (often in more than one way in the same instance be it remembered),

Firstly, mechanically.—If you simply rub into the surface some of the fungus elements, in many cases you get what we know as irritation. This is seen in the ordinary herpetic ringworm of the surface, where the mycelial threads range over the skin beneath the epidermis and lead to erythema, &c. A very remarkable case is recorded by Dr. Kennedy, of Dublin, in which a quantity of flax powder was inhaled by a lad who became attacked with measles and peculiarly severe local dyspnœal symptoms, evidently dependent upon the direct mechanical irritation exerted by the fungus elements. In the case of mildew of plants the same thing is seen, the threads of the mycelium grow and force asunder the tender structures near it. Now, it is this mechanical action exerted by the growing force which is at work, especially in ringworm. The fungus finds its way to the sub-epidermal space, from thence to the hair folicle, irritating and interfering mechanically with the growing parts, enters into the hair, and by its increase and development simply splits up the hair shaft, appropriating also its juices, and rendering it all the more brittle, and therefore the more easily destructible. To declare in such a case that the parasite is accidental in any sense of the word is to turn a deaf ear to the plainest voice of facts; but this very action can be isolated. I have performed a good many experiments at different times with diseased hairs out of the body, and occasionally it is possible to get a hair containing spores, which spores will germinate and actually produce the splitting up of the hair, and the other changes that are observed in ringworm—in fact, *to produce the lesion of ringworm out of the body.* In those instances in which the mycelium abounds, the epithelium seems to suffer particularly. On the mucous surfaces there are no such structures as hairs which form a lodgment, so to speak, for the fungi, and hence no marked results are visible. The cells of the tissues are invaded and destroyed, the mycelial phase abounds and ramifies in the secretion, and not in the tissues themselves; but there is the same *capability* of damaging when parasites attack only the internal surfaces. If we would wish for examples of the enormity of the force exerted by a growing fungus, we have only to confine some of the more ordinary varieties and see the result. Now, it so happens that no other agent can produce in dis-

ease the same kind of action as that exerted by a growing fungus,—such as splits up the hairs in the way in which this is observed in tinea; and it is this state of things which I regard as the pathognomonic lesion of ringworm, viz., the mechanical action of the parasite upon the hair and epithelium, in connexion with other minor changes.

One word as to definition. I use the word tinea as the generic term, and particularize each variety by the terms favosa, tonsurans, sycosis, vesicolor, circinata, &c.—the tinea signifying especially the diseased state of the hair and epithelium. Now, take the case of sycosis, which means inflammation of the follicles of the chin and lips. It may not be caused by a parasite; but undoubtedly cases are sometimes caused by a fungus, and these I called tinea sycosis. Again, tinea circinata means the parasitic herpes circinatus, and tinea decalvans the baldness produced by the fungus (microsporon Audouini), as distinguished from alopecia, non-parasitic baldness, the result of many different causes. The term tinea is very distinctive.

Secondly, Fungi act by inducing local chemical change.—They absorb oxygen and give out carbonic acid, and, as has been before observed, they hereby secure to themselves the power of penetrating calcareous structures. In addition, a large amount of gas is evolved as in cases of sarcinal disease. Moreover, they lead to fatty degeneration. If any one will take the trouble to examine carefully some of the old stubs in favus, he will notice a certain amount of fatty changes going on in the cell structures. Remove a hair of this kind loaded with sporules, and get the latter to germinate, and the fatty alteration goes on at a rapid rate, till after a time a large quantity of crystalline fat is produced. Now, this will not happen unless the fungus germinate; but happening, it is worked out in accordance with the views lately put forth by Dr. Bence Jones, and was expressed in precise terms in my book on parasitic diseases. It has been remarked by many observers that fat is always present in considerable amount in connexion with the development of fungi. M. Signol believed that fat very much favoured the development of bacteridia. Perhaps the very best exemplification of the association of fatty change with parasitism is that afforded by the case of the madura foot, where the oily matter is so very abundant. The tissues degenerate, and the crystalline fat is so varied and peculiar as to have actually misled observers into the belief that it was a form of fungus. Now, it becomes a question whether fat assists the development of fungi, or whether the latter attract fatty matter, the fungus forming a centre of attraction for crystallization, or the fatty change be the result of cryptogamic growth. I adhere to my original belief, and Dr. Carter is of the same opinion, that the fatty change is coincident with and a consequence of the growth of fungi. Nitrogenized and other matter becomes fatty in this way very readily. Of course, under such circumstances the fungi become a centre of attraction for the fat. It is a chemical action entirely, as far as the degeneration is concerned; a process of oxidation which the fungus induces under favourable circumstances in connexion with the performance of its own vital functions.

Thirdly, Fungi act as conveyers of poison.—This is a mode of influence which has been altogether disregarded by observers. If the endogenous pus cell can convey the noxious poison of an acute disease, why may not the elements of a fungus act in a similar capacity? Recent research has shown that all fungi exhibit great transportability. Now, what action have the cells afloat in the air of hospitals during the time of epidemics, such, for instance, as cholera (see Dr. Thomson's Observations at St. Thomas's in 1854); may they not take the virus of a hospital gangrene from one patient to another, acting the part of a fomes in the very same way, comparatively speaking, that man himself does? Suppose we inoculate with fungus elements, it is clear that in some instances symptoms ensue (as in Dr. Kennedy's and Salisbury's cases) before the onset of local symptoms. Again, the fungus elements would appear to be most active in their early stage, that is to say, when the poison produced simultaneously with their development is in its freshest and most active condition. Again, respirators in epidemics have been found to be efficacious. And, lastly, direct experiments, upon plants especially, have

shown that disease may be produced by the contact of fungus elements, when there is not a particle of evidence to prove that sporules, spores, or mycelial threads have entered the organism of such plants, but where there is the greatest probability that the granular and fluid contents may be the poisonous compound which, when absorbed, gives rise to the subsequent malady. It is not unlikely that in catarrh and influenza especially such a conveying property may be at work. We have the strongest possible amount of analogical evidence in regard to animal life, comprehended in all the details of the "animalculæ theory of disease,"—a doctrine that may be pooh-poohed by some, but which must erelong be fairly discussed. One might give a great deal of very interesting matter under this head. Those who are interested in the subject should read Sir Henry Holland's article in his Medical Notes and Reflections, 4th edition, I think, on the Animalcule Theory of Life, and to Dr. Daubeny's essay in one of the volumes of the Edinburgh Philosophical Journal, some few years back. The occurrence of epidemics, be it noted, moreover is often associated with the peculiar prevalence of various moulds and mildews,—a source of terror and superstitious horror in bygone time, which gave rise to the idea of a raining of blood. Plutarch refers to such an occurrence in the plague of Rome. Hecker, in his work on the Epidems of the Middle Ages, also associates it with the disasters of 789 and 959. The spots were actually observed on garments, and called lepra vestium; *signacula* was another term. In 1502 and 1503, it again frightened everybody. Agricola was certainly one of the first to give a rational explanation, he attributing it to the appearance of a lichen. The fungi attacked walls, bread, cheese, meat even, and garments, in Venetia, in 1819, and also articles of food, and garments, and all fruits, during the years following to 1829. And is there not something similar observable now-a-days? Have we not had some very remarkable and severe epidemics, and have not fungi been remarkably abundant on vegetation? I will not theorise, but merely just draw attention to the coincidence. The particular action of fungi now under notice will perhaps be better appreciated in connexion with that now to be described.

Fourthly, which looks upon these organisms as developers of poison, and comprehends Dr. Richardson's forsaken theory of zymosis,—a doctrine that appears to me most satisfactory. It has been suggested at different times by one and another observer that the fungi themselves induce change *actually in the circulating current*, sufficient to account for disease, either by setting up a kind of fermentative action in the blood, giving rise to the production of a specific compound,—a poison, in fact, just in like way to that which happens in ordinary fermentation, or setting up change by catalysis,— a wonderful enigma. Others affirm that no poison is produced in the body itself, but that the fungus helps out its increase when once introduced into the system. For my own part I cannot believe that any very important change could be induced by the growth of fungi in the blood current. The presence of air is so very necessary; and not only mere presence, but such as is implied by a direct communication between the growing vegetation and the external air. Outside the body, or in the cavities which communicate with the air, many very important and frequent changes are induced without a doubt.

Dr. Salisbury is a careful observer. He declares, and as far as I know holds to his opinion, that a form of disease, if not identical, at any rate very like measles, results *under certain circumstances* from the inoculation of the fungus of wheat straw. Dr. Kennedy has given confirmatory evidence. Does the fungus *per se* produce the result, or is it a conveyer, or is it the producer of the poison outside the body in the musty straw?

Dr. Richardson, quoted by the late Dr. Barker of Bedford, records the onset of erysipelatous mischief from a like cause. In France the most serious inflammatory mischiefs of veins and lymphatics have followed wounds inflicted with instruments used to cut off the diseased vine-shoots. Dr. Collin, the medical inspector of the waters of the St. Honoré, Nievre, records even fatal results. MM. Demartes and Bouché of Vitrany have also investigated this subject, and conclude that the oidium *can* produce such mischief. They suggest some sort of coincidence between the special development

of the oidium and the occurrence in greater frequency of inflammatory disease. It is to be hoped that the French Academy will, now it has taken note of the subject, enter into a full investigation of it. It is true that ill effect does not always follow experiments with the oidium. MM. Speneux and Letellier failed to produce anything beyond a little redness and irritation by inoculating people with the rasping of leaves diseased by the oidium (Pract. Jour. of Med. and Surgery, Nov., 1864); and MM. Leplat and Taillard on the one, and M. Wertheim on the other hand, came to opposite results by injecting fungus elements into veins of dogs and other animals. There can be no questioning that some fungi are more hurtful than others, and much depends upon the concomitant conditions. The arunda donax, the large red reed of the south of Europe, is attacked by a black rust, and those who cut the reeds suffer from very violent headaches; it is affirmed by M. Michel that the spores produce a papular rash on the face, with much swelling, and a good many serious general symptoms (Yearbook, 1861-2). It would seem that the fungus, *per se*, is not sufficient, but that there is something in addition which is intimately connected with the vitality of fungus. This would seem to be taught by the case of bacteridia. Whatever they be, no injurious results happen unless the medium itself in which they exist contain some peculiar virulence of its own, just as in the case of inflammatory attacks caused by oidial inoculation. The power of vegetable organisms to induce transformation, which must of course be accompanied by distinct chemical change, has been well exemplified by an experiment of M. Lemaire, who took some beans, placed them on a moist sponge, and found that bacteridia soon sprang up, before germination, being succeeded by monads and vibriones; and the like happened after the soil used had been heated to a red heat. Now, if a small quantity of phenic acid (which has the property of suspending infusorial development) was added, the germination came to a standstill until the phenic acid evaporated, when it recommenced. M. Pasteur's experiments on acetic fermentation tend to the like result; and M. Trècul's observations lead to the belief that the change induced in solutions by fungi, as in the case of alcoholic fermentation, depends upon the performance of the nutritive act of the vegetable cell. The fact is, the fungus, when growing, necessarily decomposes the medium, and induces chemical change, whilst the result depends upon the composition of the material acted upon. In like manner, it is conceivable that the fungus of wheat straw acts upon the juices of the stem, producing some subtile compound; bacteridia do the same in *sang de rate*, and the oidium in the vine disease.

It has been supposed that the poisons of measles, influenza, cholera, nay, asthma, and some other acute diseases, may be produced in the way indicated; but it must be remembered that two or more of the modes of action already noticed may be conjoined; that is to say, a fungus may act mechanically as a conveyer and developer of poison at the same time, and in one case. But not only acute but chronic diseases are produced. I refer to the large class of instances in which vegetable parasites induce slow changes of deleterious nature in articles of diet, giving rise to "ergotism." Bad grain, bad potatoes, bad rice, bad maize, are illustrative. The late Russian epidemic, the Irish fevers, pellagra in Lombardy, gangrene in sheep and beasts, ergotism in horses, have all been regarded as taking origin especially from the play of ergotized foods. In the group of *chronic* maladies the material acted upon by the fungus is a solid. The access of air is not so perfect nor so free; the moisture is considerably less; all of which tends in great measure to account for the difference of the quality of the resultant morbid product. The productiveness of grain so infested is considerably lowered. Sir H. Davy proved this long ago. He found that diseased wheat yielded from 21 to 65 per cent. of nutritious matter against the 95 per cent of the healthy grain. It has been suggested at various times that the degeneration of rice by parasitic action gives rise to the formation of products which occasion very severe symptoms of intestinal irritation, resembling dysentery, and that œdema of the leg often follows. And it is not unlikely that the many peculiar ulcerative conditions of the lower extremities are favoured by

the quality of food and induced in like manner. The diminution in the productiveness of the silkworm affected by muscardine affords a capital instance of analogical occurrence. The statistics issued by the Chamber of Commerce of Turin show that although formerly about some 650,000 myriagrammes of cocoons were produced in the country, in 1864 there were 525,000, and last year 283,000 only. I have been paying some little attention to the case of mildewed cotton,—hunting after *illustrative* facts,—and I find that the germs of mildew are really present in the cotton in its rough state, as sold in the market before it reaches the manufacturer. It of course is possible that the processes through which it passes in the hands of the latter may destroy all the vitality of the fungi, but this is not certain; but if it really does, still the fact of the presence of mildewed germs in vigour would imply the existence of a certain degree of deterioration in the actual fibre itself, perhaps induced by the bad cultivation or growing of the original plant,—a point of no mean interest to the merchant. It would make the fibre less able to resist the action of the size and other agents used in the manufacture into stuffs.

I have spoken of things going on outside the body, and then introduced to it; but within a recent time, certain facts have come to hand showing that under special conditions, though good food be taken into the stomach, yet, in the digestive tract, changes of objectionable character may be induced by the agency of fungi. I have to quote Dr. Salisbury again as my authority. He believes that chronic diarrhœa in the army is caused in this way (see the Report of the Surgeon-general of Ohio, in the Amer. Jour. of Med. Sciences, 1865). Wherever there is a poor amylaceous diet, and there be retention of the food, the torula, almost always present, grows, and in so doing induces fermentative changes, with the evolution of gas,—the production of intestinal irritation and diarrhœa,—the torula vegetating into a myceliated "algoid" mass, which may be observed in the fæces; and it appears that its amount is in direct relation to the severity of the disease; the production of sugar being rapid and detectable in the mucous tissues. The green stools of children are so produced, and Dr. Salisbury thinks also that semi-paralytic symptoms ensue. The case of sarcinal disease is on a par entirely; deranged digestion, detention of food, the presence of penicillium, and the evolution of gas with the formation of sarcinæ; vomiting is the result of gastric, as diarrhœa that of intestinal irritation. The stomach in the former, and the intestines in the latter, getting semi-paralysed, at least losing tone and getting relaxed. In both cases there is the mechanical action of the fungus and the induction of chemical changes within the body. The case of diseased foods is one of surpassing consequence, and deserves all the attention we can afford to it.

The quality and character of the poisons or products of this fermentative act are matters of no little interest. Dr. Richardson has lately deserted the zymotic, and given his adhesion to a new theory, which regards the poisons as of an alkaloid character,—basing this position upon the supposed isolation of the pyæmic poison; however, further experiment is needed to establish the truth of the new doctrine. If the poison of so-called zymotic diseases be chemically inorganic, how comes it that nothing of the kind can be obtained by chemical analysis? The diffusion and spread of disease is opposed also to such a view. There is a power of increment about these viruses which is very marvellous and peculiar. There is also a vital principle or act which is very distinct. Another feature worth notice is this, that the effect of the poison does not seem to be, as is the case with mere chemical agents, proportionate to the dose, so to speak, but to the peculiar virulency, which varies as much as the state of the nutrition of the organism acted upon. The viruses certainly, as to their characters, vary considerably, and are not *definite* in the way that we would expect if they were of an alkaloid nature. Independently even of these kinds of influence already noticed, fungi, in the fifth place, would seem to possess inherent noxious qualities in some cases. Just as insects have the power of producing special poisons, so may fungi in a much less degree. The anamita muscaria affords a resinous principle, which chemists isolate. In other cases—for example, the mushroom—there

is evidently an alkaloid, as MM. Sicard and Schoras have shown (Journal de Pharmacie, 1865); but the action of it is different from that of viruses altogether.

Now I have mentioned five different ways in which fungi may act; and these may be summed up as follow, being divided into those which are direct and indirect. Directly, they may act mechanically, or by inducing local chemical change; indirectly, by bringing about changes in substances out of the body, which are brought to influence the latter; by setting up a kind of fermentative action in part due to the oxidation consequent upon the nutritive changes in the plant, or by giving rise to products having an acute or a chronic action, and whose nature is at present a matter of doubt.

And now I am prepared to meet the hypothesis that parasitic disease has nothing essential to do with the development of parasites. Mr. Hunt takes the boldest view in the ranks of the opposition, declaring that the causes of parasitic disease are four, and four only—uncleanliness, atmospheric impurities, deficient exercise, and contagion. I take my stand upon the mechanical action of fungi, and the induction of fatty changes, and defy any one to shake me the least from my footing. Mr. Hunt states that the above four conditions "poison the blood, producing not only their immediate effects in the form of parasitic skin diseases, but laying the foundation probably of more serious disorders, manifested in after life by the presence of lumbrici, ascarides, tape-worm, pediculi, fungi, hydatids, tubercles, and perhaps cancerous germs, in the various organisms." What does this mean? That these varied mischiefs have not each their proper cause, but arise from one and the same influence. This is surely either subversion of the logical definition of cause—unconditional sequence—which is so tenaciously upheld and received as our only true belief. I grant that the four states lower the nutrition of the system, and make it more fit for parasitic growth; this is only one item of the total. The true state of the case I take to be this. That there is a necessary nidus, which is exalted by some into the position of the supreme disease, to the negation of any and every effect produced by the fungus itself, which finds the soil congenial,—a soil associated with bad living, and bad hygiene of all kinds; the fungus growing acts in the various ways already detailed; in ordinary cutaneous affections, the effect upon the hair and epithelium (mechanical and chemical) being *pathognomonic*. Parasitic disease, then, is a composite affair, consisting of mal-nutrition, a growing parasite, and certain effects of such growth.

There is yet one category of facts that needs a word or two of comment, viz., the comparative pathology or the intertransmission of parasitic (vegetable) maladies. In addition to what I have given in my work, a good deal of information has been accumulating. It is now admitted that the transmission of the common ringworm of the surface from animals to man is very common. I am informed upon good authority that this is of very frequent occurrence in Australia, the milkers of cows especially being largely affected. Professor Gerlach (abstract in Ed. Vet. Rev., vol. ii.) has noticed it in dogs, horses, and oxen, and in man, but the sheep and pig seem to offer exception. Dr. Frazer (Dub. Quart. Jour. of Med. Science, May, 1865) contributed a paper, "Remarks on a Common Herpetic Epizootic Affection, and on its alleged frequent Transmission to the Human Subject," containing cases. This gentleman quotes Mr. Brady and Mr. Whitla, in reference to other instances. Dr. Fehr has noticed in Switzerland the transmission from cattle to man. I can confirm by my own experience the truth of these statements. I do not mention any old cases, such as mice, affecting man; but my friend Dr. Allchin informs me that he has seen the transmission of mange from a cat to a child.

Now, I might argue just in like manner in regard to the animal parasites. The two classes of cases are mutually illustrative of each other's *modus operandi*. I take the case of scabies. The acarus demands a suitable soil. It has been pretty well shown, in animals especially, that acari will not grow on all surfaces, but only on those whose hygienic condition we have reason to know from the circumstances that have been at play is not that of health.

The limit of variation is by no means made out in the case of the animal parasites. The relations of acari on bodies generally is being canvassed, especially by German writers. The mode of entry has an analogy also. There is the same difference of opinion as to whether the acari are accidentals or *veræ causæ*; but there is plenty of evidence to indicate the iraction as mechanical irritants, and as the possible developers of irritant products. But these points I cannot now enter upon.

The matter of the action of fungi is a large and wide one; already we see enough to show that the studious inquirer will be amply repaid if he tread carefully the somewhat now uncertain path before him, and the promising indications of success are many.—*Edinburgh Med. Journal, April*, 1866, *p.* 875.

113.—OBSTRUCTION OF THE BOWELS, SUCCESSFULLY TREATED BY THE USE OF THE STOMACH-PUMP.

By Dr. Ebenezer Fleming, Stranraer, Wigtonshire.

[The patient was 59 years of age. Previous to the present illness he had three different attacks of obstruction of the bowels, in one of which, after the desired relief, gout appeared in the great toe of his left foot. In November, 1865, obstruction again occurred and did not yield to any aperients tried, nor yet to injections. The pain was very severe and paroxysmal, and vomiting constant; pulse 72, of good volume. Abdomen much distended. Dr. Dickson, of Newton Stewart, and Dr. Fleming saw the case together.]

We agreed first to inject a quantity of soap and warm water, by means of the long tube attached to the stomach-pump. The tube, measuring a yard long, passed freely and without pain its whole length, but from the distended state of abdomen, the injected fluid caused considerable uneasiness. It was retained. A small quantity of frothy mucus was passed. No other result. The following pills were prescribed, to be used during the night :—℞. Sub. mur. hyd. gr. xij., ext. hyosy. gr. xviij. M. et div. in pil. xii. One every three hours. Abdomen to be well fomented and rubbed with ung. hydr. mit.

26th. 12 noon. Dr. D. White, of Whithorn, who had attended Mr. S. in some of the former attacks, was now associated with us in the case. No vomiting since last night; abdomen more distended; other symptoms unchanged. We agreed again to pass the long tube, and throw into the colon a pint of infusion of salts and senna, with ℥ss. ol. terbinthinæ. Retained, but no result. 4. p.m. Distension of abdomen much increased, so as to impede respiration; pulse continued good. Hab. pulv. jalapæ ver., ℈ij. 6 p. m. Repeated last injection, with addition of tinct. assafœtidæ, ℥ij. The ordinary enema apparatus was on this occasion used. After an ineffectual attempt at the night chair, the pulse suddenly increased to 132, and became feeble; respiration 48; abdomen distended to its utmost limit; outline of colon distinctly perceptible. The symptoms now became more and more alarming. We thought of putting patient into a warm bath, and dashing a pitcher of cold water over the abdomen, but he became so depressed that we could not attempt it. Dr. Dickson suggested that the long tube should be passed, with the hope of drawing away flatus. At our subsequent deliberations this gave the cue to our agreeing to pass the tube into the colon, and applying the stomach-pump, to try whether it would relieve the distension. 10 p.m. The long tube being attached to the pump, as for emptying the stomach, I again passed it its whole length without difficulty into the colon. The first result of the syringe was to bring away flatus, then semi-fluid fæces until a wash-basin was filled. Dr. White supported the tense abdomen, and he felt it distinctly subsiding under each action of the syringe. Complete relief was immediately experienced. Habent ol. ricini ℥j. During the night the bowels were two or three times very copiously relieved, and all the severe symptoms subsided. Next day gout appeared on great toe of left foot (as on a previous attack), for which he was treated by Dr. Dickson.

A letter, dated 6th inst., from Mr. S., now residing in a distant part of the

country, informs me that he is keeping well, but that he requires to take great care, and adds, "My left foot still looks gouty, and swells slightly in the evening."

Remarks.—The progress of the case corroborated our opinion that the cause of the attack was gout seizing upon some part of the bowel, causing primary contraction, but more extensive dilatation. Dr. Wood, of America, in his first volume of Practice of Medicine, refers to such cases of obstruction of the bowels, and considers them less dangerous than other forms. In this particular case, however, the symptoms became so alarming, and the ordinary treatment so inefficient, that we all felt that at any moment fatal inflammatory action might set in. In our experience or course of reading, none of us had ever seen or even heard of the stomach-pump being used in this way; but it is very probable that other practitioners have applied it similarly, although I have not been able to find a published case. Before reporting this case, I intended to have more experience of this application of the operation. Such cases, however, do not often occur, in a country practice; and should similar cases be treated on this principle by other practitioners after the failure of the ordinary routine of treatment, I should be much obliged by receiving brief reports of the result. In mechanical occlusion of the bowel, whether caused by intussusception, twist, internal hernia, tumour, gallstone, or structural change, this operation would most probably fail; but in many cases of obstruction we cannot determine the real cause during life; and other means having failed, the stomach-pump might be applied as herein recommended—viz., to inject a warm fluid as a solvent or diluent, allow it to remain two or three hours, reverse action of pump, and eject—with the hope that in the worst forms of obstruction it is possible, and in the simple forms probable, that it will be successful.—*Med. Times and Gazette, April* 21, 1866, *p.* 414.

114.—ON THE SPHYGMOGRAPH.

By the Editor of the Lancet.

There are few things more interesting in the recent history of medicine than the remarkable ingenuity with which physical means of research have been brought to bear upon the hidden secrets of the body in health and disease. What the ophthalmoscope has done for the eye, and the laryngoscope for the larynx, the stethoscope and percusser have done for the hidden organs of the chest. The thermometer, in the hands of Wunderlich, Aitken, and Ringer, is beginning to tell us its own story in the diagnosis and prognosis of disease; so that, for example, in the treatment of fever the thermometer will at the fourth day establish an almost absolute diagnosis between typhus and typhoid fever. And a further instrument of precision, of remarkable beauty and wide range of usefulness, was on Wednesday evening last, brought prominently under the notice of English physicians by Dr. Anstie, at the Medical Society of London. The sphygmograph of M. Marey is an exquisitely designed instrument, by the aid of which the pulse is armed with a pen, and at every beat writes its own diagram, and registers its own characters. In this diagram each part of every revolution, or "beat" of the heart is recorded, so that the relation of the systole and diastole is inscribed in every curve, and the state of arterial tonicity on the one hand, and the impulsive power of the heart on the other, are automatically compared. The finger is substituted by an instrument of precision, which replaces impressions by recorded facts self-analyzed. Dr. Anstie and Dr. Sanderson have for some time been pursuing a series of clinical and physiological observations by this instrument, and on this occasion Dr. Anstie discussed, by the aid of pulse-charts drawn to large scale from the automatic register, some of the results of a clinical investigation by the aid of Marey's sphygmograph in the history of typhus and typhoid fever, and of the ephemeral fevers which occasionally simulate them. He seems to conclude that in both typhus and typhoid fever it appears to be the universal rule that the pulse is affected in a way which indicates great

reduction of the arterial tonicity. This indication is given, on the application of the sphygmograph, by pulse-curves, which present a nearly vertical line of ascent, an acute summit, a sudden descent, and a very marked dicrotism, or double-beat. The pulse, thus strongly marked, continues, *at all hours of the day*, during the whole disease, up to the period of true convalescence. The prognosis seems to be always favourable as soon as the pulse has returned to that normal type which indicates the restoration of arterial tonicity, even though for some time it remain rapid. On the contrary, there appears to be always danger so long as the peculiar pulse remains. If, for instance, after the end of the third week in typhoid, or the second week in typhus, there is not a considerable alteration of the pulse-form in the direction of the normal type, dangerous sequelæ may be feared.

It contrasts with the peculiar type of pulse which, even from an early stage, distinguishes typhoid and typhus, the pulse in the fevers which we style "febricula" is not materially altered from the normal type, except as regards rapidity, even when much heat of skin and other decidedly febrile symptoms are present.

It is believed by Marey, and Dr. Anstie's experience so far seems to confirm it, that while typhus and typhoid fever (and also contagious erysipelas, purulent infection, low pneumonia, acute rheumatism, and delirium tremens) present the pulse of low arterial tonicity, the so-called true exanthemata—scarlatina, measles, small-pox, &c., show no such affection, at any rate in their early stage. This, if true, establishes a very important point in diagnosis, besides indirectly affording excellent suggestions for the pathological investigation of fevers.

These "points" are not stated in a dogmatic manner by Dr. Anstie. They are results which appear fairly to issue from an extensive investigation (more especially of typhoid fever). But it is certain that the co-operation of a large number of careful and painstaking observers is necessary in order that the history of the pulse-form may be traced as completely as, for instance, Wunderlich has traced the history of temperature-changes in various fevers, which was only effected after 700 complete observations carefully conducted. At present, besides himself and Dr. Sanderson, to whom, indeed, the idea of the investigation was due, Dr. Anstie knew of none who has taken the matter up in this country. He determined, therefore, without loss of time, to direct the attention of English physicians to the immense field of fruitful observation which lies open to the investigation of observers who are willing to devote time and patience to the development of M. Marey's brilliant invention.—*Lancet, Nov.* 25, 1865, *p.* 599.

115.—THERAPEUTICAL NOTES.

There are symptoms met with in old persons which are often the precursors of apoplexy, and which remind one of those that frequently portend insanity, such as dizziness in the head, confusion of ideas, and general embarrassment of the mental faculties. Dr. Headland, in treating such cases in the out-patient department of the *Charing-cross Hospital*, finds that half-drachm doses of the solution of bichloride of mercury (L.P.) if given three times a day for three or four weeks, have a useful influence in removing these sensations. In the chronic bronchitis of the old, where there is great dyspnœa, accompanied by a dry state of tubes, he finds the tincture of stramonium, given in doses of ten minims, combined with other expectorants, very useful. In cases of excessive secretion, he finds no objection to the use of opium in half-drachm doses of the compound tincture of camphor. On the contrary, great relief results from the exhibition of this drug. In tænia, the result of his experience is that the oil of turpentine, combined with castor-oil, three drachms of each, is a better remedy than the oil of male fern. The castor-oil prevents those unpleasant head symptoms which are apt to arise if the turpentine is given alone. Turpentine he also finds very valuable in epilepsy. He gives it either

in half-drachm doses three times a day, or in a single dose of three drachms at occasional intervals, combined with an equal quantity of castor-oil. The results of treatment of lepra and psoriasis, with five-minim doses of the liquor arsenicalis three times daily, and the external use of the liquid pitch ointment, he finds very satisfactory indeed. The arsenic can be continued for months without any deleterious effects. If such should arise, Dr. Headland suspends this medicine, and gives bicarbonate of potash for a short time, when the symptoms of poisoning—rash, irritation of the bowels, &c.—subside. In chorea, he has used nothing which has a better effect than iron. In chlorosis, with suppressed menses, he finds the old Griffiths' mixture the best treatment, combined with half-ounce doses of the compound decoction of aloes in the morning.—*Lancet, Jan.* 6, 1866, *p.* 8.

116.—ON THE PHYSIOLOGICAL OPERATION OF THE PREPARATIONS OF IRON.

By Dr. A. Sasse.

Dr. Sasse, after making some remarks on the astringent properties of the preparations of iron, used both externally and internally, proceeds to discuss their chemical operation on the body. He considers them to be, like the blood-corpuscles, carriers of ozone, and therefore capable of replacing the corpuscles. These bodies change the oxygen taken into the blood by respiration into ozone, and the common oxygen of the atmosphere is just as useful for breathing as so much nitrogen; it must first be decomposed into the negative electric oxygen or ozone, and the positive electric oxygen or antozone, and the former operates as an immediately oxidizing agent. The ferruginous preparations only share with the blood-corpuscles this property of changing common oxygen into ozone, of giving off this ozone to substances inclined to oxidation, and of again taking up oxygen, until all are decomposed or eliminated from the body. The iodide of potassium paper is coloured blue when brought in contact with a solution of a persalt of iron, or with blood mixed with water. From this similiarity in reaction, Schönbein has concluded that peroxide of iron is composed of protoxide of iron and ozone. Ferruginous preparations resemble the blood-corpuscles also in this respect—that they change the positive electric antozone into ozone. Since the preparations of iron act altogether like the blood-corpuscles in relation to oxygen, ozone, and antozone, it will readily be understood that the former, when carried into the blood, may replace the loss of the corpuscles. It matters little whether the iron is taken into the blood in substance, or as an oxide, or as a proto- or persalt; for when it has once entered the circulation, it continually takes up and gives off ozone, and thus becomes alternately a proto- and a persalt united, until it is expelled from the body.

It may perhaps be asked whether in the living body the ferruginous preparations act as oxidizing agents. Such is probably the case, and Botkin has shown that iron, even in healthy persons, raises the temperature above the normal standard. This elevation of temperature often occurs rapidly, and at the same time more urea is excreted, these phenomena all showing an increased oxidation. The same conclusion may be drawn from the similarity of the appearances observed (by Demarquay and Leconte) from the breathing of oxygen and from the operation of iron. Peroxide of hydrogen also, which contains antozone and has lately been recommended for the promotion of tissue-metamorphosis, may be compared with the preparations of iron, since antozone can be easily converted into ozone. Since also iron increases the temperature of the blood, it stimulates the action of the heart.

Thus, concludes Dr. Sasse, we have in iron a means of improving the tone of the system and promoting the process of oxidation. As this metal, therefore, favours the nutrition and the activity of the body, it is contra-indicated when the process of oxidation is already too energetic, as in inflammatory diseases, or when an increased flow of blood is apprehended. In tuber-

culosis of the lungs, according to the author, iron ought to be avoided, notwithstanding some recent recommendations to the contrary.—*British and Foreign Medico-Chirurgical Review, Jan.*, 1866, *p.* 233.

117.—SINAPINE TISSUE, OR MUSTARD PAPER.

(Prepared by Mr. Cooper, Chemist, Abingdon-terrace, Kensington, W.)

This is a thin, glazed, innocent-looking paper, but any one who touches it with the tip of the tongue will soon become conscious that it is coated with something of a very pungent character. Wetted with cold water and applied to the skin, it produces in about ten minutes a good deal of superficial irritation. Between it and a mustard poultice there seems to us this difference, that its effects are more persistent, and not quite so circumscribed, and that the sensations it produces are not the same. In fact, if we had to give a name to it, we should rather have called it *capsicine* than sinapine tissue. Be this as it may, it is so extremely portable, convenient, and clean, that persons who are in the habit of using mustard poultices or other ready forms of counter-irritation—as for coughs, sore throat, deafness, &c.—may easily carry a supply in their card case. Probably a hot poultice *over* a bit of this paper would answer well in cases of pleurodymia, &c.—*Medical Times and Gazette, Feb.* 10, 1866, *p.* 162.

118.—ON THE SUBCUTANEOUS INJECTION OF MORPHIA AND ATROPINE.

By Dr. C. E. Brown-Sequard, F. R. S.

Six or seven years ago I was led by the knowledge of the antagonistic effects of morphia and atropine on the brain to inject these two agents together, so as to reap the benefit of the good effects of the one added to those of the other against pain. The doses I at first employed were half a grain of the sulphate of morphia to one-sixtieth of a grain of the sulphate of atropine. For reasons to be mentioned hereafter, I now employ from one-half to two-thirds of a grain of the sulphate of morphia with one-twentieth or one-twenty-fifth of a grain of the sulphate of atropine. Great advantages are obtained by this combination: 1, an addition of the good effects against pain of the two remedies; 2, the possibility of employing safely, or at least without great cerebral disturbance, large doses of these narcotics.—*Lancet, March* 10, 1866, *p.* 249.

119.—ON THE ABSORBING POWER FOR MEDICINES POSSESSED BY CERTAIN PARTS OF THE BODY.

By Dr. C. E. Brown-Sequard, F. R. S.

The interesting experiments of Dr. B. W. Richardson, by which he thought he had found a valuable mode of producing local anæsthesia, have led Dr. Aug. Waller to a very important discovery. He has found that certain substances, such as atropine, strychnine, morphine, and the tincture of aconite, when mixed with chloroform and applied to the skin, are absorbed very rapidly; while, if alcohol, instead of chloroform, is mixed with the above substances, absorption is much delayed or even altogether prevented. The possibility of introducing rapidly into the blood the most active narcotics and other remedies, without having to perform the operation of hypodermic injection, will prove of great value in those cases in which an operation, however slight, is much dreaded, and in cases in which it may be found necessary to keep the patient for a long time under the influence of the treatment (for instance, in tetanus, in hydrophobia, in persistent neuralgic pains, &c.).

Within the last ten or fifteen years several facts of importance, as regards the selection of the proper surface of absorption of certain remedies, have been clearly made out. I will only mention the following:—1. Prof. Bernard and other observers have ascertained that woorara is hardly absorbed by the mucous membranes of the mouth and stomach, while it is very quickly absorbed when put in contact with the subcutaneous cellular tissue. 2. Mr. W. S. Savory has ascertained that strychnine is absorbed so much quicker by the mucous membrane of the rectum than by the stomach, that a dose only one-fourth of another will act with greater energy in the rectum than the much larger dose in the stomach. 3. I have found that ointments of belladonna and opium employed against neuralgic and other uterine pains act with greater rapidity and much more benefit when pushed up on a small lint ball in the rectum than in the vagina, showing that absorption is more rapid by the mucous membrane of the rectum than by that of the vagina. Not long ago the method consisting in applications of narcotics on the skin, deprived of its epidermis by a blister, was rather thrown into the shade, owing to the superiority of the hypodermic method. I protest, however, against the abandonment of a method which has the double advantage of counter-irritation and rapid absorption of a narcotic, and which may sometimes prove more useful than subcutaneous injections. In a case of crural neuralgia which had not been markedly benefited by hypodermic injections of morphia and atropine, I have obtained a quick amelioration and ultimately a cure from a series of blisters (by the hartshorn ointment) and applications of sulphate of morphia on the denuded dermis.

Of two other methods of administration I will only say a few words. One of them is the inhalation of pulverized fluids; the other the use of medicated cigars or cigarettes. Hyoscyamus, stramonium, belladonna, and many other remedies, may be employed with advantage by one or other of these means, not only in asthma, but in hooping-cough, in laryngismus, and also in tetanus, in hysterical aphonia, &c.—*Lancet, March* 10, 1866, *p.* 249.

120.—CHLOROFORM AS A MEANS OF DETECTING BILE IN THE URINE.

A new method for the detection of bile in the urine is proposed by M. Cunisset, and is given fully in the January number of the Journal de Chimie Médicale. He places in a test-tube forty or fifty grammes of the urine to be examined, and adds to it four or five grammes of chloroform, and then shakes the mixture. If the urine contains bile it immediately assumes a fine yellow colour, and on allowing it to rest, the chloroform falls to the bottom of the tube, drawing with it the fatty matters of the urine coloured by the yellow biliary substance.—*Lancet, March* 3, 1866, *p.* 238.

121.—AN ANTIDOTE FOR PRUSSIC ACID.

An antidote for prussic acid has been proposed by Messrs. T. and H. Smith. It consists of magnesia and perchloride of iron, and its effect depends upon the formation of the compound known as Prussian blue. It is recommended to give the patient first the magnesia (one or two drachms), made into an emulsion, with water; and then to give, in water, a solution of sixteen minims of perchloride of iron, and twelve grains and a half of green vitriol.—*Lancet, Nov.* 25, 1866, *p.* 604.

122.--ON ALCOHOL.

By Dr. Henry Munroe, Hull.

[The question as to the action of strong alcohol on digestion is of less consequence than as to its action in a very dilute form, as it exists in a glass of pale ale. The following experiments of Dr. Munroe are very interesting and important.]

Not having had the advantage of making any experiments with pure gastric juice obtained from the stomach of a living human being, as in the case of Alexis St. Martin, who had an opening made in his stomach by a gunshot-wound,—or as in the case of the Esthonian peasant, Catharine Kütt, who had for three years a gastric fistula,—from which openings gastric juice was easily obtained—my experiments have all been performed with gastric juice artificially made, and with meat digested out of the body.

The following experiments, out of the many performed, seem to be the most conclusive.

Experiment 1.—Four ounces of gastric juice having been poured into a wide-mouthed phial, half an ounce of raw beef, cut up into small angular pieces, was then introduced. The phial was placed for ten hours in a sand-bath at the heat of 100° Fahr., being every two hours briskly shaken to imitate the motions of the stomach. After the first two hours, the fluid became opaque and cloudy; the surfaces of the bits of beef assuming a milky white appearance. In about four hours, the muscular fibres of the beef on the surface, and at the angles especially, began to separate and loosen. In about six hours, the pieces of beef were much lessened in size, and the fluid presented a more cloudy appearance, like thin broth. In about eight hours from the shaking of the phial, the pieces of beef were easily broken into shreds of muscular fibre, floating about in the fluid. In about ten hours, these muscular fibres became softened down and were no longer discernible having become completely dissolved, the mixture presenting all the characters of soup.

Experiment 2.—Four ounces of the artificial gastric juice were poured into another phial, and I introduced half-an ounce of raw beef also cut into small pieces. I then added two drachms of rectified spirits of wine, and then placed the phial in the sand-bath. After two hours, little or no alteration had taken place in the fluid, which seemed to have no action on the beef. In four hours, the fluid was very slightly opaque, the beef remaining the same in appearance. In six hours, a slight albuminous coating was seen on the surface of the beef. After eight hours, there was no change to the sight; but the pieces of beef *felt more solid* during the operation of shaking the bottle. In ten hours, there appeared only the slightest change upon the surface of the beef, the substance of it not having been acted upon at all. On the phial having been set aside to cool, there was seen at the bottom of it *a deposit of pepsine*, which was not observed at the beginning of the experiment, and which did not appear at the bottom of the phial used in Experiment No. 1.

Experiment 3.—Instead of making the gastric juice with four ounces of water, I used that quantity of Allsopp's bitter ale. Half-an-ounce of beef was introduced into the phial as in the other experiments, and the phial was placed in the same sand-bath. In two hours, there appeared little or no change in the fluid or beef. In four hours, the fluid was rendered slightly cloudy, and the surfaces of the beef slightly coated with albumen. In six hours, the muscular fibres of the beef seemed somewhat separating and loosened. In eight hours, no further change had taken place. In ten hours, the beef presented a similar appearance to the beef in the phial containing alcohol—very little acted upon by the gastric juice. The phial on being set aside, and cooling, *a deposit of pepsine* appeared, as in the preceding experiment. On reference to the experiments performed out of the body by Dr. Beaumont, with the gastric juice obtained from the stomach of St. Martin, I find that the process of digestion was performed in somewhat less time. But when a piece of meat tied to a string was suspended in the stomach, it became completely digested, and quite disappeared in the short period of an hour and

a-half, showing the difference of the result in point of time, between natural and artificial digestion.

That alcohol, even in a diluted form, has the peculiar power of interfering with the ordinary process of digestion, I cannot have a doubt. That alcohol, in a diluted form, has the power of precipitating the *whole* of the pepsine used in these and other experiments, I will not affirm; but that it has the power of diminishing the efficacy of the gastric juice as a solvent for food, is sufficiently illustrated in my experiments.

On reference to the experiments performed in the human stomach, I may observe that Dr. Beaumont caused St. Martin, after a dinner of wholesome food, which was digesting happily, to take a glass of gin. The digestive process was immediately arrested and suspended, until the spirit had been absorbed into the general system, when the digestion of the food was slowly renewed and tardily completed.

Many years ago, after I had been an abstainer from all alcoholic drinks for six months, I partook of half-a-pint of Allsopp's bitter ale at dinner. In less than an hour afterwards I felt tired, exhausted in mind and body, inactive, rather feverish, and inclined to sleep, with increased fulness at the pit of the stomach, such as I had not experienced during my term of alcoholic abstinence. This fulness of the stomach, accompanied with slight difficulty of breathing, from pressure on the diaphragm, did not abate for three or four hours after taking the meal; nor did I again feel hungry that day, although I took my usual amount of exercise. For four days I continued to take the ale at dinner, but always experienced afterwards the same fulness at the pit of the stomach, and other symptoms of arrested digestion. At the end of the fifth day, happily for me, a fit of the gout supervening, caused my experiment to be suspended, and negatived the idea that Allsopp's bitter ale was, in any way, a promoter of digestion.

After a similar abstinence, I partook, daily, of half an ounce of spirits of wine diluted with three ounces of water at dinner. I again experienced the same feelings of distress at the pit of the stomach, inactivity, sleepiness, &c., arising, I have no doubt, from suspended digestion.

After a similar abstinence, I partook of three or four glasses of wine at dinner, and suffered again from the same symptoms. I have also partaken of almost every variety of food, accompanied with half to a pint of bitter ale, and have always experienced the same feelings of arrested digestion. For eight years I suffered from repeated attacks of gout, arising, I believe, from indigestion occasioned by the use of alcoholic beverages; but for some years, under total and persistent abstinence, I have never suffered from any symptoms of imperfect digestion, and my old enemy—the gout—has entirely retreated.—*British Med. Journal.*, *Dec.* 9, 1865, *p.* 618.

123.—THE COURSE OF A DOSE OF ALCOHOL THROUGH THE SYSTEM.

By Dr. H. Bence Jones, F. R. S.

It passes through the stomach, and in going through is to a small extent oxidised. The greater part escapes, and enters the circulation, acting there on the oxygen, fibrin, albumen, and blood globules to a small degree. In a few minutes a part passes, still being oxidised, into every secretion, as, for example, into the lungs, kidneys, liver; whilst another part passes into every other texture; and on each substance in each texture the alcohol produces its chemical action, still continually undergoing oxidation, so that in a few hours one part has entirely escaped, and another part has been entirely oxidised in the blood and textures. What has the alcohol during this time done? It has acted chemically on oxygen and on the textures with which it has been in contact. The first action has little to do with the strength of the alcohol. Weak or strong, it combines with oxygen; increased oxidation, increased circulation, increased secretion, increased effusion of lymph, altered nutri-

tion result. The second action depends on the strength of the alcohol. Strong alcohol acts chemically on albumen, fibrin, cellular tissue, &c. It has a powerful attraction for water, and it shrivels up cellular tissue, hardens fibrin, and precipitates albumen by its chemical properties.

To no organs of the body is more alcohol taken than to the kidneys and liver, and in no organs are the mechanical results of its chemical actions more manifest and the consequences of the altered structure more apparent; and the increased oxidation, the increased flow of blood, the altered nutrition are identical in kind with the first actions of inflammation; and when the series of actions are repeated over and over again for years, the result is the same as might have been produced in a shorter time by an ordinary inflammation when no alcohol whatever had been taken.

Dr. Christison states that three-fourths of all the cases of Bright's disease which he saw were produced by the habitual long-continued abuse of drink. Very great occasional excess did not act so strongly as long-continued smaller excess.

No doubt the alcohol acts on the vessels and ducts; but not so powerfully as on the interstitial texture.—*Med. Times and Gazette, Jan.* 6, 1866, *p.* 1.

124.—ON A NEW AND READY MODE OF PRODUCING LOCAL ANÆSTHESIA.

By Dr. Benjamin W. Richardson, M. A., Senior Physician to the Royal Infirmary for Diseases of the Chest.

[Some years ago Dr. Richardson published a paper on "Voltaic Narcotism." He had the idea that local anæsthesia could be produced by voltaic electricity, and succeeded in the case of feeble subjects; but in persons of average strength this actually increases the molecular motion of the part experimented upon. Anæsthesia really means the temporary death of a part influenced, i. e., inertia in the molecules of the part. Dr. Arnott's plan of using extreme cold was certainly the first step towards the production of local anæsthesia, but a more ready and easier plan of application is desirable.]

When the toy for diffusing eau de cologne in fine vapour over the skin, in the form of spray—which some time ago found its way into our drawing-rooms—first came before me, it struck me at once that it might possibly be applied to the production of local anæsthesia; and I set to work to try its applicability in this respect. I was soon afterwards assisted largely in my labours by taking advantage of Siegle's apparatus, with the hand-ball spray-producer invented by my valued friend Dr. Andrew Clarke, and supplied by the manufacturers, Messrs. Krohne and Sesemann, of Whitechapel-road.

With this apparatus I set myself to determine the degree of cold that could be produced by the vaporisation of all the known volatile liquids, and I determined the fact that the intensity of the cold produced held a definite relationship to the boiling-point of the fluid used; the rule being that the lower the boiling-point the greater was the amount of cold exhibited. In these inquiries I employed a very delicate thermometer, directing the spray upon the bulb from half an inch to an inch and a-half from the point of the jet. By these means I learnt that with rectified sulphuric ether I could bring down the thermometer within 10 degrees Fahr. of zero, and that by directing the jet on the skin I could produce a certain definite and marked degree of local insensibility, but not sufficient for surgical purposes.

I next got Mr. Krohne to construct for me a hollow cylinder of thin metal, six inches long and three inches in diameter. In the circumference of this cylinder was a chamber one-eighth of an inch in diameter for containing ether. The ether communicated with a tube which was joined to an air-tube, as in Siegle's apparatus, and the centre of the cylinder was filled with ice and salt mixture. In this way the ether was reduced to zero, and when vaporised gave spray which brought down the thermometer six degrees below

zero, and produced on the skin such entire insensibility that I could pass a needle through the part without sensation. On the 11th of December, 1865, I applied this process for the first time on the human subject for an operation. The patient was a lady, who required to have five front teeth extracted. I had previously administered chloroform to this lady for a tooth extraction, but the inhalation had produced so much irregularity in the action of the heart and other disagreeable symptoms, that I considered it inadvisable to repeat chloroform, and she herself was only too ready to give the local measure a trial. The extraction was performed by my friend Mr. Peter Matthews. On directing the ether spray first at a distance and then closely upon the gum over the first central incisor on the left side, we observed, at the end of fifty seconds, that the gum had become as white as the tooth itself, and quite insensible. I then directed the vapor upon the tooth for twenty or thirty seconds more, and on the patient intimating that she did not feel, I suggested to Mr. Matthews to proceed. He extracted a very firm tooth without the slightest expression of pain. The process being continued in the same manner, he extracted three other teeth with the forceps. The fourth gave way, and had to be removed by the lever; but in all cases the result was equally good. Not a drop of blood was lost; there was no painful reaction; and the healing process proceeded perfectly. Our patient, who was exceedingly intelligent, was specially requested to note every step of the operation, such as the applying of the forceps, the insertion of the blades beneath the gum, the loosening process, and the removal. She told us that in two of the extractions she felt nothing; that in one it seemed as though the jaw altogether were being pulled downwards, but without pain; that in another she was conscious of a kind of wrench or loosening but without pain, and that the introduction of the lever was attended with a momentary dull ache, just perceptible. On the whole, the process was quite as painless as when she took chloroform.

On December 13th I applied the local anæsthetic to the same lady for the further extraction of nine teeth, Mr. Peter Matthews again operating. The results were equally good with the first seven, at which point, unfortunately, the apparatus partly ceased play. At the eighth tooth pain was felt, and at the ninth, the apparatus being out of play, the operation caused great pain. We regretted this much, although it gave us the information of the perfect action of the process when no mechanical obstacle interfered with it. The reason why the apparatus stopped play was very singular, and could hardly have been foreseen. It arose from the condensation of water derived from the air in the air tube, and from the blocking up of the fine jet with a little portion of ice.

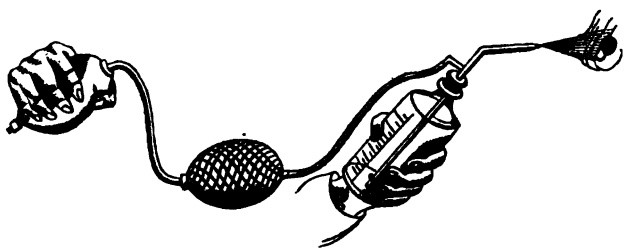
In the next step of research I got Mr. Krohne to make for me an apparatus with two spiral tubes, one the air tube, the other a tube for ether; and I immersed these spirals in a closed chamber filled with ice and salt. The degree of anæsthesia at first produced was most intense; and Mr. Spencer Wells was good enough to allow me the opportunity of applying the process in a case where an operation was required for closing a perineal rupture. Unhappily the apparatus, from the very same cause as before, ceased to yield a current; water condensed and became frozen in the air tube. The apparatus itself was also found to be too cumbersome for practical purposes. I therefore, in this trial, failed to obtain any result.

By this time I had been led, very reluctantly, to the fact that the use of ice and salt for reducing the ether was a failure when the plan came to be tried in practice, nor could I see any ready way of preventing the difficulties that were brought before me. Added to these difficulties there was another, which has always attended my friend Dr. Arnott's plan, viz., that of getting the ice and salt ready for operation. To succeed, therefore, it was requisite to dispense with ice and salt altogether.

In considering how this object could be achieved, it occurred to me that if a larger body of ether than is supplied by Siegle's apparatus could be brought through the same jet, by mechanical force, in the same interval of time, and with the same volume of air, a proportionate increase of cold must necessarily

be produced. The theory was one of pure physics, admitting even of arithmetical demonstration, and running parallel with the lessons which had been taught me with respect to the cold produced by liquids having different degrees of boiling point. The theory was put to the test at once, and proved correct to the letter. By driving over the ether under atmospheric pressure, instead of trusting simply to capillary action—or to suction, as in Seigle's apparatus—the spray evolved brought the thermometer within thirty seconds to four degrees below zero—the result that was desired.

Ascertaining this truth, I instructed Messrs. Krohne and Sesemann to construct the very simple apparatus depicted below.



The apparatus consists simply of a graduated bottle for holding ether; through a perforated cork a double tube is inserted, one extremity of the inner part of which goes to the bottom of the bottle. Above the cork a little tube, connected with a hand bellows, pierces the outer part of the double tube, and communicates by means of the outer part, by a small aperture, with the interior of the bottle. The inner tube for delivering the ether runs upwards nearly to the extremity of the outer tube. Now, when the bellows are worked, a double current of air is produced, one current descending and pressing upon the ether, forcing it along the inner tube, and the other ascending through the outer tube and playing upon the column of ether as it escapes through the fine jet. By having a series of jets to fit on the lower part of the inner tube, the volume of ether can be moderated at pleasure; and by having a double tube for the admission of air, and two pairs of hand bellows, the volume of ether and of air can be equally increased at pleasure, and with the production of a degree of cold six below zero.

By this simple apparatus, at any temperature of the day and at any season, the surgeon has thus in his hands a means for producing cold even six degrees below zero; and by directing the spray upon a half-inch test-tube containing water, he can produce a column of ice in two minutes at most. Further, by this modification of Seigle's apparatus, he can distribute fluids in the form of spray into any of the cavities of the body—into the bladder, for instance, by means of a spray catheter, or into the uterus by an uterine spray catheter.

When the ether spray thus produced is directed upon the outer skin, the skin is rendered insensible within a minute; but the effects do not end here. So soon as the skin is divided, the ether begins to exert on the nervous filaments the double action of cold and of etherisation; so that the narcotism can be extended deeply to any desired extent. Pure rectified ether used in this manner is entirely negative; it causes no irritation, and may be applied to a deep wound, as I shall show, without any danger. I have applied it direct to the mucous membrane of my own eye, after first chilling the ball with the lid closed.

I have now employed this mode of producing local anæsthesia in four cases on the human subject. The first case was the extraction of a tooth from a lady, the operation being performed by my friend and neighbor Dr. Sedgwick, on January 24th of this year. On the 29th of the same month I used it again on the same lady for the extraction of three very difficult teeth, Dr.

Sedgwick again operating. The results were as satisfactory as in the previous case, where the ice and salt ether apparatus was used.

I have used the apparatus also in connection with my friend Mr. Adams, who had a case at the Great Northern Hospital of deep dissecting abscess in the thigh of a young woman. In the abscess there was a small opening, which just admitted the director. I first narcotised around this opening, and the director being introduced, Mr. Adams carried his bistoury nearly an inch deep and one inch in the line of the direction. I then narcotised the deep-seated parts, and enabled him to cut for another inch and a half in the same direction. The director was then placed in the upper line of the abscess, the process was repeated, and the incision was carried two and a half inches in that direction. The patient was entirely unconscious of pain, and after narcotising the whole of the deep surface, Mr. Adams inserted his fingers and cleared out the wound, without creating the slightest evidence of pain.

Afterwards, in the case of a lacerated wound, six inches long, in the arm of a boy, who had been injured with machinery, I narcotised while six sutures were introduced by Mr. Adams. The first needle was carried through without the anæsthetic, and caused expression of acute pain; the remaining eleven needles, after a few second's administration of the ether spray, were passed through painlessly. The twisting of the wire sutures gave no pain.

These results are so interesting that I make no apology for bringing them at once before my Medical brethren. I wish it to be distinctly understood that at the present moment I only introduce the method here described for the production of superficial local anæsthesia. It is, I believe, applicable to a large number of minor operations, for which the more dangerous agent chloroform is now commonly employed—I mean such operations as tooth-extraction, tying nævi, tying piles, incising carbuncles, opening abscesses, putting in sutures, removing small tumours, removing the toe-nail, dividing tendons, operating for fistula, removing cancer of the lip, and other similar minor operations which I need not mention. The process may also be applied to reduce local inflammation.

In course of time, and guided by experience and the advancement of science, we may, however, expect more. If an anæsthetic fluid of negative qualities, as regards irritation of nerve, and which has a boiling point of 75° or 80°, can be obtained from the hydro-carbon series, the deepest anæsthesia may be produced, and even a limb may be amputated by this method. It may also turn out that certain anæsthetics may be added to the etherial solution with advantage, such as small quantities of chloroform, or some of the narcotic alkaloids, if they could be made soluble in ether. A solution of morphia and atropia combined, if they could be diffused through ether, which at present seems impossible, could thus be brought into action so as to cause deep insensibility. In operating on the extremities, it would be good practice to stop the current of warm blood by making pressure above on the main artery. Reaction from the anæsthesia is in no degree painful, and hemorrhage is almost entirely controlled during the anæsthesia.

One or two precautions are necessary. It is essential, in the first place, to use pure rectified ether; methylated ether causes irritation, and chloroform, unless largely diluted with ether—say one part in eight—does the same.

The *modus operandi* of this process is exceedingly simple. It acts at first merely by extracting force, and afterwards, when the nervous filaments are exposed, by preventing the conveyance of force through them. To be plain, sensation means the conveyance of force or motion from the extreme parts to the brain. The motion is communicated by the blood in the form of heat; it is communicated to the nervous filaments, and by them is conveyed to the sensorium. This is passive sensibility. When we irritate a nervous fibre, as by a cut, we communicate more motion rapidly along that fibre and cause pain. This is active or exalted sensibility. To remove sensibility, therefore, we must adopt one of three processes; we must remove or render inert the sensorium; we must stop the evolution of force generally by arresting oxidation of blood; or we must rob the body locally of its force beyond that with which it is constantly being renewed. We see the first of these processes in

action in cases of pressure on the brain, as from injury or effusion of blood; we see the second whenever we produce general anæsthesia by charging the blood with chloroform or other analagous anæsthetic; and we see the third when, by means of extreme cold, we rob the local part of the force that has been brought to it by the blood.

The problem of local anæsthesia will consequently be quite solved when by a rapid process we can exhaust the natural force of a part as fast as such force is evolved in the local structure; and especially when with this we can combine the action of a substance which for the moment controls, as by compression, the conducting power of nerve matter. These two latter objects are to a large extent carried out by the method I have described above.

[The following paper contains one or two observations of importance made since the publication of the preceding article.]

I. I find that all the ordinary ethers of the shops contain alcohol, the presence of which substance materially interferes with the success of the process; it prevents perfect anæsthesia, and it causes tingling and burning sensation at the beginning of the process and during the brief period of reaction. I would, therefore, point out that before Practitioners resort to the process they test the purity of the ether they are about to use, by the following simple experiments:—

Tests for Ether.—(A) Take the specific gravity. The specific gravity should not exceed 0·723.

(B) Try the boiling point. Warm the hands by gently blowing into them the warm breath. When the hands feel as warm as the breath, make the palm of one hand into a cup and pour in one or two drachms of ether. The ether ought immediately to boil briskly without giving any pain.

(C) Test the effect on mucous membrane. Put one or two drachms of the ether in the palm of the hand and quickly take up the ether into the mouth with the tongue. The ether should at once pass off, leaving neither smarting nor burning, nor any sensation except a slight coldness.

(D) Pour a little of the ether on a piece of clean white blotting-paper and lay the blotting-paper on the warm hand. The paper should dry within a minute, leaving no moisture and no smell whatever. If the paper, while drying, yield an odour like eau-de-cologne, there is some alcohol present. If it give a smell slightly pungent, and which hangs about for a time, there is some methylated compound present. Perfectly pure ether, in a word, leaves no persistent odour.

(E) Try the degree of cold producible by the ether. Charge the bottle connected with the spray producer, and direct the spray on the bulb of the thermometer. The mercury ought to fall rapidly to six degrees below zero, Fahr., and the falling of the mercury should continue until there is a deposit of snow on the bulb of the thermometer from condensation of water in the air.

(F) Test the effect on the skin. Direct the spray, at a distance varying from half an inch to an inch and a-half from the jet, on the back of the hand. In a space of time, extending from thirty seconds to two minutes, a slight hoar-frost deposit should form on the skin, followed immediately by a diffuse blanching. The skin is at this moment altogether insensible.

(G) Test the reaction of the ether by litmus. The reaction should be neutral.

These are the ready and necessary tests. A pure ether answers to all of them, and no other ether ought to be used.

For a little time the Profession may experience a difficulty in obtaining pure ether, because it is not commonly vended. At the same time, as there is nothing more required than repeated rectification to render ether quite pure, it will be produced so soon as there is a demand; and although the prime cost will be increased, it will be saved in the end, because the quantity of ether required for each operation is reduced in proportion to the purity of the fluid. Already, indeed Mr. Robbins, of the firm of Garden & Robbins, of Oxford-street, has, with his usual promptitude, produced for me an ether which bears all the tests I have described above.

II. I have observed in producing local anæsthesia by this new process that the peculiar hardness of the skin which occurs when the freezing mixture of ice and salt is applied, does not occur. The tissues, on the contrary are comparatively lax, so that difficult dissections may be carried on with ease and nicety.

III. I have found during the last few days that the application of the narcotic spray to the parts of the body subject to neuralgic pain gives immediate relief.

I have only to add, in conclusion, the gratifying intelligence that I have now applied the local anæsthetic in forty-three minor operations with a result of complete success in thirty-six cases, and with more than partial success in the remainder. In those cases where the success has not been absolute, the result has been due to imperfection either in the ether or apparatus, or to deficient experience (unavoidable in all preliminary inquiries) in the mode of application. The reaction has been rapid, painless, and satisfactory.

[The most important element in the success of the plan of producing local anæsthesia introduced by Dr. Richardson is, that it supplies a ready means of removing pain arising from surgical operations or other causes, without the risk of sacrificing life. For convenience, Dr. Richardson divides the following short paper under four heads.]

1. *The Principle of the Process.*—The principle of the new anæsthetic process consists in directing on a part of the body a volatile liquid having a boiling point at or below blood heat, in a state of fine subdivision or spray, such subdivision being produced by the action of air or other gaseous substance on the volatile liquid to be dispersed.

When the volatile fluid, dispersed in the form of spray, falls on the human body, it comes with force into the most minute contact with the surface upon which it strikes. As a result there is rapid evaporation of the volatile fluid, and so great an evolution of heat force from the surface of the body struck, that the blood cannot supply the equivalent loss. The part consequently dies for the moment, and is insensible, as in death ; but as the *vis a tergo* of the body is unaffected, the blood, so soon as the external reducing agency is withdrawn, quickly makes its way again through the dead parts, and restoration is immediate. The extreme rapidity of the action of this deadening process is the cause of its safety. The process can suspend life without causing disorganisation ; if I may use the expression, it produces syncope of the part—temporary death—but not necessarily destruction. When we produce general anæsthesia we virtually extend this mere local action to the body altogether—*i.e.*, we check the evolution of force at the centre, and produce an approach to temporary death of the whole of the organism.

2. *The Instrument.—Improvements.*—The instrument by which the volatile fluid is dispersed is described at length in my paper published in this journal on February 3d [given above]. It consists of a spray-tube and bottle worked by Dr. Andrew Clark's hand bellows. The tube differs from all other spray-tubes in that the volatile fluid is brought up for dispersion by air pressure produced by the same motion as that which causes the dispersion. The instrument also provides a means for regulating the current of fluid ; it allows the ether to be carried along tubes of any convenient length and curve ; and, lastly, it enables us to construct a *compound* instrument by which the effects may be multiplied to any reasonable extent for large operations.

In my original paper, referred to above, I described simply the single dispersion-tube. Since then I have made a large number of tubes to answer various powers and purposes. I have a tube in which there is a bulb enlargement at the end with perforated side, or side and central jets. This tube is exceedingly useful for the cavities of the body, such as the vagina or rectum. It distributes the fluid in the same manner as a syringe with several perforations at its point. In practice, I find that the dispersion of the fluid delivered from one fine tube by a series of jets is not so efficient, proportionately, as when it is delivered by one jet : the fluid, that is to say, requires a certain degree of concentration to ensure success.

In order to multiply the anæsthetic producing power, I have other instru

ments constructed which may be called compound. In these cases the bottle holding the volatile fluid either receives a common central tube of large size communicating with a number of terminal jets, or each terminal jet has a separate jet running into the fluid. By this means I have a brush of jets, which may be circular, or long, or flat, as required. For this compound tube a six-ounce containing bottle for the fluid is necessary, and additional bellows power. The present small hand bellows will only work a compound jet of two elements with efficiency. I have tried to meet this difficulty by using a bellows worked by the foot, but not as yet to my full satisfaction. To get the air pressure, I have tried various plans so as to do away with the hand bellows. I have used carbonic acid compressed into an iron bottle, and have applied the gas in its escape so as to act in the same manner as the air from the bellows. The apparatus complicates, and the pressure of gas cannot be nicely regulated. I have modified this plan also by trying to get force by generating carbonic acid gas at the time; also by generating hydrogen from zinc and dilute sulphuric acid, and using the pressure of the gas as the distributing agency.

Again, I have tried water pressure, as in the common gas holder; and I think in the dentists' room this plan would succeed well, if the preliminary expense were no obstacle. But taking all in all, the hand ball bellows are as yet the most practical and most ready; they carry in the pocket, and one can go with them to the patient and commence anæsthesia at once—a great consideration. In many small operations, requiring only one or two strokes of the knife, the whole may be done painlessly, while the patient thinks that the preparations are merely being made—much, of course, to his gratification.

Several suggestions for the improvement of the jets offer themselves for consideration; the fish-tail gas-burner, the Argand burner, the conservatory water engine, and many other mechanical contrivances similar in kind will occur to every one as worthy of imitation, and as quickly as they can be made they will be produced and tested.

3. *The Fluid to be Used.*—I still continue to use absolute ether, for operations, and now, as Mr. Robbins has produced an ether of specific gravity 0·720 of negative effect on the tissues, and having a boiling point of 92° Fahr., a better fluid can hardly be demanded. Many other fluids have been suggested by the various readers of my original paper—viz., methylic ether, amylene, monochloretted chloride of ethyle, pure chloric ether, nitrite of ethyle, a volatile hydrocarbon derived from the manufacture of portable gas, chloroform, rectified turpentine, and numerous compounds and mixtures. As regards these I may state that they have all been under my careful consideration, but that as yet none of them, taking their qualities all in all, are equal to absolute ether. Some are open to rejection at once from their boiling point being too high; others are unpleasant, and would not admit of being used in operations on the mouth or teeth; others cause irritation of skin; others in their pure state are so extremely volatile that they could not be kept in the surgery for any length of time—this is specially the case with methylic ether and nitrite of ethyl, both promising substitutes for ether; lastly, a body too volatile would somewhat affect the operator during the operation if it were diffused in the pure state. Nitrite of ethyl is open to this objection not so much as the nitrite of amyl would be, but to some extent.

In time we may, perhaps, by experiment get a compound volatile mixture which, being as negative as absolute ether in its effects on the body, shall boil a few degrees lower.

Before leaving this topic, let me state that the mere alteration or change of the volatile fluid used is no change of the principle of the present anæsthetic process. Ether itself is only a local anæsthetic on being employed according to this principle. This is important to be borne in mind, otherwise a principle may become confounded with a detail, and every fluid with a low boiling-point and the other necessary physical qualities, as I have described them, for producing insensibility will be dubbed a local anæsthetic.

By a slip of the pen, indeed, this error was committed in the Medical Times and Gazette of last week, a short leading article having been headed "Kerosolene a local Anæsthetic." Kerosolene—a body very impure, unpleasant, and of unsteady but low boiling-point—when applied by my method acts like ether, because it resembles ether physically. But kerosolene is no anæsthetic *per se*, although it would serve as a substitution agent for one part of the anæsthetic process, in the same manner, and in none other, as an earthenware bottle would take the place of the graduated glass bottle in which the volatile liquid is commonly retained.

Dr. F. D. Fletcher, of Southport, has suggested to me the employment of liquid carbonic gas, and, as will be seen by last week's Medical Times and Gazette, Sir James Simpson has had carbonic in view for some years. I believe the first physiologist who actually tried to apply the reducing agency of carbonic acid for the production of anæsthesia was the late Dr. Snow. He went to great trouble and expense to experiment on the gas in the solid state, and he applied it in that state to his own skin. Singularly enough, the insensibility produced was slow and imperfect, but the worst feature was that a slough was always produced on the part where the acid had been applied. Snow, therefore gave up the effort, convinced that carbonic acid in the solid form could never be made applicable in actual practice, and that if it could it would not be a safe agent.

I myself moved for a time in a similar direction by using carbonic acid in the liquid form. Mr. Robbins supplied me with the gas reduced in a three pint iron bottle under pressure. When the stop-cock was opened and the carbonic was liberated through a fine jet, an intense cold was produced; but I utterly failed in attaching a conveying tube that would be applicable for operations. The pressure, in a word, was unmanageable, and, for ordinary practice, dangerous. In one experiment, while the jet was being directed on the back of my hand, the nozzle of the tube became set free, and, being projected with violence, injured me severely. I, therefore, like Snow, gave up carbonic acid as a body that would not submit to guidance, and as impossible to use in surgical practice.

We need not, fortunately, trouble ourselves on this subject. I have shown that by the dispersion of fluids of low boiling-points we can get a degree of cold which answers the required purpose without employing fluids or gases under pressure. If we want more than absolute ether, chemistry can furnish us with fluids which boil even at below 70° Fahrenheit, which fluids, dispersed as vapour, would fill the purpose of carbonic acid, with only one disadvantage—that of being difficult to keep in store during many months of the year.

4. *The Practice.*—[The surgeon may produce either entire blanching of the surface to be operated on, or he may stop short of that extreme result, and only induce a superficial anæsthesia. The extreme action should be reserved for cases where deep-seated parts have to be divided.]

For producing the deep anæsthesia with superficial whiteness it is necessary to use absolute ether, and to direct the spray in brisk current at a distance of about an inch from the part. To induce the less determinate condition the ether may be diluted. This may be done by mixing alcohol with the ether, or better still chloroform. Two mixtures of this kind are very useful; one contains six parts of ether and two of chloroform, the other seven of ether and one of chloroform. In using pure ether, or the mixture, differences of time are required. To cause insensibility with the simple fluid—ether—from fifteen to fifty seconds only are necessary. To produce insensibility by the mixture of ether and alcohol, or of ether and chloroform, from four to five minutes are demanded. The sensation felt by the patient also differs. When pure ether is used little if anything is felt until the moment when the part becomes white; then there is a sharp, pricking, burning sensation. When the compound or mixture is used, the sensation prolonged by comparison, is that of numbness and aching. On the whole, I have found patients generally prefer the more rapid procedure.

The nature of the operation will, to a large extent, determine the

method to be resorted to. For opening an abscess, for incising a small carbuncle, for tying a nævus, for removing very small tumours, for applying nitric acid, and for operations of a similar kind, the mixing of ether and alcohol, or of ether and chloroform, answers every requirement. I should myself also use the mixture in an operation for hernia, because the tissues would not be rendered hard, and the dissections could be carried on with delicacy. But for deeper operations, such as removal of the nail, of portions of bone, of fingers and the like, the complete action of the anæsthesia requires to be brought into play. For teeth extraction the pure ether also answers best—it acts rapidly and deeply, and there is no great accumulation of fluid in the mouth. By practice the two degrees of action I have named may be obtained by the employment of ether alone: I mean, the degree of anæsthesia from the spray of absolute ether can be determined by the distance from the part at which the spray is directed; by removing the jet three inches from the part a moderate effect is produced, nearly equivalent to the dilution of seven parts of ether with one of chloroform. The condition of the patient generally ought likewise to be considered. Aged and weak people become anæsthetic very readily, and for them the milder process is most applicable.

My researches on the production of local anæsthesia by means of ether spray have led me to invent a few new compounds of ether which cannot, I think, but prove useful in practice, and to which I would direct attention.

Hæmostatic Ethers.—In observing the influence of the cold produced by the dispersion of absolute ether during operations, nothing has struck me more than the effect of the cold in immediately stopping the flow of blood. For a time, cold alone, when carried to its fullest degree, prevents all venous and capillary hemorrhage, and even the hemorrhage from small arterial trunks. After a time, however, as reaction returns, and the vessels relax under the influence of heat derived from the renewed circulation, there is bleeding, which, if a wound be closed too quickly, is a cause of after trouble. The observation of the immediate effects of cold led me to think that if they could be supplemented by a styptic which would spray evenly with ether, and which would take up the constringing action when the vessels commenced to relax, an important desideratum in both Medical and Surgical practice would be supplied.

Xylo-Styptic Ether Spray.—With this object before me, I requested Mr. Robbins to make for me a solution consisting of absolute ether, having a boiling point of 92° Fahr., charged to saturation at a low temperature with tannin, and afterwards treated with xyloidine, a little short of saturation. The compound, made with much care, came out well. It ran easily through the spray tube without blocking; it produced good local anæsthesia, and it possessed an agreeable odour.

In order to test to the extreme the effects of this preparation as a styptic, I took sheep's blood, removed all the fibrine previous to coagulation by whipping, and then let the blood remain exposed to the air for two days to ensure partial decomposition. In this way the blood was rendered nearly as fluid as port wine, and in the most unfavourable condition for being transformed into clot. A few drachms of this blood were now placed in a saucer, the saucer having been warmed to the temperature of the body. The spray of the styptic ether was then directed upon the blood from a full-sized spray tube, and in five seconds the whole mass of blood was so thoroughly solidified that the saucer could be turned upside down without any escape of fluid. The blood, which had previously presented the odour of putrefaction, was also deodorised, and remains quite inodorous at this date—ten days after experiment. The blood sets in a firm leathery consistence, covered on its upper surface with a fine whitish layer, with a bright vermilion colour beneath.

These are the effects of the styptic either on blood, the spontaneous coagulability of which has been lost, and I had the pleasure of showing these effects at the College of Physicians on Friday last, during a lecture on heat and cold in the treatment of disease; but these effects are trifling when compared with what takes place on blood newly drawn, and which con-

tains fibrine. In this case the process of coagulation under the influence of the spray is the work, I had almost said, of a second.

When this spray is directed on an open bleeding living surface, the primary effects are those produced by the cold—namely, the condensation and whitening of the tissues. If blood be flowing, it solidifies, and when the parts relax, new blood that may ooze up enters the solid blood as though it were a sponge, quickly solidifying by coagulation and stopping further flow.

The applicability of this process for the arrest of hemorrhage will occur to the mind of every practitioner. The substances used in the compound are innocuous, and the combined influence of the cold and the styptic are immediate, and so decisive that I can scarcely imagine any hemorrhage they would not control. I have not had an opportunity of testing the point, but I have no doubt from the influence of the styptic on the decomposing albumen of defibrinated blood, that even in those cases of hemorrhage where the blood is preternaturally fluid the styptic spray would arrest the hemorrhage entirely. Where the blood contains fibrine in a natural condition, I cannot imagine a case in which the fluid would not prevent exudation.

The essential elements of this process are three in number:

1. The immediate constringent effects of cold on the blood-vessels.
2. The chemical action of the solution on the fibrine and albumen of the blood.
3. The extreme mechanical fineness of distribution of the fluid on the bleeding surface.

The styptic ether can not only be applied to open wounds on the skin, but to hemorrhage after the extraction of teeth, and, by means of a uterine tube, to hemorrhage arising from cancerous disease of the uterus or other cause. It might also be applied to the rectum in cases of hemorrhage from piles.

The apparatus required for this styptic ether is mechanically the same as for ordinary ether—that is to say, my spray tube with Dr. Clark's hand bellows. The tube, however, requires to be made of different metal from that ordinarily in use for local anæsthesia; and I have therefore instructed Messrs. Krohne and Sesemann to construct a special tube for the purpose.

Ferro-Styptic Ether.—I have tried other experiments with the persalts of iron, which are more or less soluble in ether, especially the perchloride, and these one and all produced, as a styptic ether, rapid coagulation of blood. Solutions of iron salts in ether are not, however, more effective than the ether I have already described; and as they destroy the tube rapidly, act upon clothing injuriously, and do not so thoroughly deodorise, I do not think they are in the main so practical.

The styptic ether, containing xyloidine and tannin, will keep ready for use any length of time, as there is nothing in it to undergo decomposition; and as very small quantities of it are required, it will become, I trust, of standard service to the medical practitioner. It would be of great use also to surgeons on board ship, and particularly to army surgeons. In case of warfare, it would be exceedingly useful on the battle-field, as under the instruction of the surgeon it could be used by an orderly, so as to prevent hemorrhage instantaneously in the case of flesh wounds. It would also form a useful addition to the medical cabinet of travellers, who by necessity are removed from the direct succour afforded by medical art.

I have invented some ether compounds—namely, a caustic ether, an iodised ethereal oil, and an ozonised ether, to which I will refer in a future number. —*Medical Times and Gazette, Feb.* 3, 17, *March* 10, 17, *and April* 28, 1866, *pp*, 115, 169, 249, 277, 439.

125.—RHIGOLENE, A PETROLEUM NAPHTHA FOR PRODUCING ANÆSTHESIA BY FREEZING.

By Dr. Henry J. Bigelow, Professor of Surgery in the Massachusetts Medical College.

The above name is proposed as convenient to designate a petroleum naph-

tha boiling at 70° F., one of the most volatile liquids obtained by the distillation of petroleum, and which has been applied to the production of cold by evaporation. It is a hydrocarbon, wholly destitute of oxygen, and is the lightest of all known liquids, having a specific gravity of 0.625. It has been shown that petroleum, vaporized and carefully condensed at different temperatures, offers a regular series of products which present more material differences than that of their degree of volatility, and that the present product is probably a combination of some of the known products of petroleum with those volatile and gaseous ones not yet fully examined, and to which this fluid owes its great volatility. A few of these combinations are already known in trade as benzolene, kerosene, kerosolene, gasolene, &c., all of them naphthas, but varying with different manufacturers. I procured, in 1861, a quantity of kerosolene of four different densities, and found the lightest of them, the boiling point of which was about 90°, to be an efficient anæsthetic by inhalation. When it was learned here that Dr. Richardson, of London, had produced a useful anæsthesia by freezing through the agency of ether vapour, reducing the temperature to 6° below zero, F., it occurred to me that a very volatile product of petroleum might be more sure to congeal the tissues, besides being far less expensive than ether. Mr. Merrill having, at my request, manufactured a liquid of which the boiling point was 70° F., it proved that the mercury was easily depressed by this agent to 19° below zero, and that the skin could be with certainty frozen hard in five or ten seconds. A lower temperature might doubtless be produced, were it not for the ice which surrounds the bulb of the thermometer. This result may be approximately effected by the common and familiar "spray producer," the concentric tubes of Dr. Richardson not being absolutely necessary to congeal the tissues with the rhigolene, as in his experiments with common ether. I have for convenience used a glass phial, through the cork of which passes a metal tube for the fluid, the air-tube being outside, and bent at its extremity so as to meet the fluid-tube at right angles, at some distance from the neck of the bottle. Air is not admitted to the bottle, as in Dr. Richardson's apparatus, the vapor of the rhigolene generated by the warmth of the hand applied externally being sufficient to prevent a vacuum and to ensure its free delivery; 15° below zero is easily produced by this apparatus. The bottle, when not in use, should be kept tightly corked, a precaution by no means superfluous, as the liquid readily loses its more volatile parts by evaporation, leaving a denser and consequently less efficient residue. In this, and in several more expensive forms of apparatus in metal, both with and without the concentric tubes, I have found the sizes of 72 and 78 of Stub's steel wire gauge to work well for the air and fluid orifices respectively; and it may be added that metal points reduced to sharp edges are preferable to glass, which, by its non-conducting properties, allows the orifices to become obstructed by frozen aqueous vapor.

Freezing by rhigolene is far more sure than by ether, as suggested by Dr. Richardson, inasmuch as common ether, boiling only at about 96°, instead of 70°, often fails to produce an adequate degree of cold. The rhigolene is more convenient and more easily controlled than the freezing mixtures hitherto employed. Being quick in its action, inexpensive, and comparatively odorless, it will supersede general or local anæsthesia by ether or chloroform for small operations and in private houses. The opening of felons and other abscesses, the removal of small tumours, small incisions, excisions, and evulsions, and perhaps the extraction of teeth, may be thus effected with admirable ease and certainty; and for these purposes surgeons will use it, as also, perhaps, for the relief of neuralgia, chronic rheumatism, &c., and as a styptic and for the destruction by freezing of erectile and other growths. But for large operations it is obviously less convenient than general anæsthesia, and will never supersede it. Applied to the skin, a first degree of congelation is evanescent; if protracted longer, it is followed by redness and desquamation, which may be possibly averted by the local bleeding of an incision; but if continued or used on a large scale, the dangers of frost-bite and mortification must be imminent.

It may be superfluous to add that both the liquid and the vapour of rhigolene are highly inflammable.—*Boston Medical and Surgical Journal, April 19, 1866, p. 1.*

126.—ON ANÆSTHESIA BY MIXED VAPOURS.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

While engaged in obstetric practice, it has long occurred to me how great would be the advantage of possessing a substance safe in the use, and at the same time capable of producing insensibility to pain. Such an agent is yet to be discovered. But I think I have found a method by which the same result may be obtained by a combination, in a certain manner, of those substances with which we are already familiar. Reflecting on the commoner causes of fatal chloroform accidents, the conviction is impressed on me that we commence our anæsthetic induction with the wrong agency. Chloroform, however diluted, is unsuitable for the *early stages* of inhalation, and it is in these especially that fatal results have been most frequent. At this period the emotional causes of danger are most active, the resistance greatest, and the danger also greatest. But even when judiciously administered, chloroform, in my judgment, is the wrong thing to begin with. Its physiological action as a stimulant is brief, while the narcosis which follows is prolonged. Also, notwithstanding many assertions to the contrary, I think there is evidence to prove that occasionally chloroform vapour acts as a depressant from the first. What it seems to me we require, therefore, is to commence the anæsthetic operation with some of those substances which will give a gentle stimulus to the patient, sustaining the heart-power, tranquillizing the emotional condition, and thus gradually introducing to the stage of first partial, and then complete insensibility. In the method I would now introduce, this is effected by a means so simple and secure, that I have good hope it may commend itself to the judgment of many of our profession.

I take a drachm of *pure and strong alcohol*, sp. gr. 810 to 815, and pour into one part of the instrument described below; this will last for four or five minutes. Into another division I pour a drachm of *pure ether*, sp. gr. 725, which lasts, perhaps, a couple of minutes; and I keep supplying this in small quantities as required. The patient begins to inhale, and receives into his lungs a mixed vapour of alcohol and ether, the evaporating surfaces being so adjusted as to keep a tolerable equilibrium between these two ingredients. By the simple act of rotating the apparatus, the quantity inhaled is increased, and is measured off on a scale to which the index points. The operator thus sees at a glance what amount is passing into the lungs, and adjusts it to the requirements of the patient. There is no coughing, or struggling, or resistance at this stage. Very gradually the pulse begins to rise, the surface becomes warm (it is often cold from mere fright), and the patient comes under the full influence of the mixed vapours; this I have called the stimulo-sedative stage of the operation. *Then*, when the heart and the nervous system have fully experienced the power of this excitant, a little turn further brings in the chloroform vapour, by such gentle gradations that the patient passes insensibly from the milder influence of the one agent into the anæsthetic power of the other; and by still further rotating the instrument, he is made to breathe only chloroform vapour just at the time when he is ready for the further proceeding of the surgeon. I think that in this method of inducing anæsthesia are combined the conditions of the greatest security with those of complete abolition of pain. I would urge upon my professional brethren to avail themselves of the principle which I here seek to bring before them, so simple in its nature that it has probably occurred to many of them before this. I would also help those who wish to put it, as I have done, into actual practice, by describing the instrument I have made and used with success, and an engraving of which is appended. It is really a *working* instrument, without complication of parts, easily understood, quickly cleansed, and not very difficult to manufacture, my own having

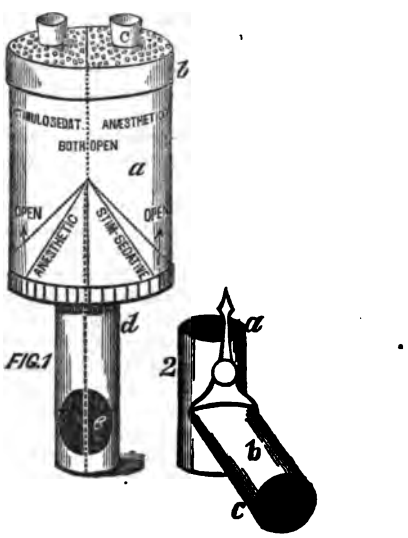
been made by myself out of a few bits of brass tubing, and then electro-plated.

For prolonged anæsthesia, as in obstetric practice, I think this apparatus admirably adapted. After a partial anæsthetic effect is produced by the mixed vapours, it is then easy to turn on a little more chloroform with each wave of uterine effort, and to turn it off to the safer vapour back again as the "pain" subsides. By this means we obtain all the advantages claimed by the Chloroform Committee for their anæsthetic mixtures, with the additional one of having the potent agent most completely under our control. So far as I am able to judge, the use of the mixed vapour is followed by much less depression than sometimes ensues after chloroform. It has also this advantage, that, whereas the use of chloroform alone (probably from its sweet, sickly odour) often leaves behind on the mind of the patient an excessive repugnance to the future use, or even the smell, of this substance, the same effect will not follow the employment of the vapours here recommended. In actual practice, I find it a slight good to dissolve a small quantity of some aromatic substances in the alcohol or ether, and none better can be had than the aromatics used in sal volatile—namely, the oil of lemons and the volatile oil of nutmegs. It is useful also to colour the fluids slightly, so that they may be at once recognised, and mistakes for chloroform prevented. But I must very carefully recommend the operator to make sure that he is using *pure absolute* ether and *absolute* alcohol, perfectly free from the abominable contamination of methyl and its compounds. By care in this respect he will secure constant uniformity in his operations, and avoid the ill effects of those adulterations to which alcohol and ether are too subject. It is also important to observe that the ether and alcohol are really absolute. For this purpose the ether should be washed in distilled water, and then drawn off by a water-bath. The strongest commercial alcohol should also be procured and redistilled from chloride of calcium, drawing off three-fourths of the amount held by the still. The advantages are these, that definite fluids of known strength are employed, the results of the use of which will always be comparable with each other, and the inhaler does not become charged with water or the heavier parts of the spirit. By proceeding thus, a highly stimulating, easily vapourisable, and a very agreeably smelling spirit is obtained—very different from the ether and alcohol of the shops.

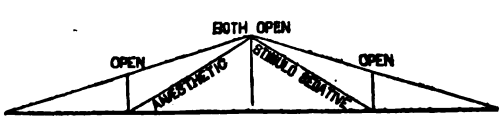
The question will be asked as to the value of using these anæsthetics in a separate form, when all can be had combined in the mixtures so strongly recommended by the Chloroform Committee. The principal and most important advantages are these: that we thus isolate and have under our control a perfectly safe stimulant and mild anæsthetic, while in the same instrument we have at command a most valued and powerful agent for the abolition of pain. And these are capable of being so united and blended together in any desired quantities as to give us such control over the individual case operated upon as that may seem to require. We are able to produce any degree of anæsthesia by this means, from the slight dreamy unconsciousness to the deepest coma; and by aid of the index we can at any moment observe what is the precise amount of either or of both agents which is being employed. Anæsthesia may, in fact, be procured more quickly than by any of the anæsthetic mixtures, and it may then be graduated to the varying condition of the individual—in the paroxysm of labour pain and in the interval of comparative freedom, in the division of skin and sensitive structures, and in the dissection of less nerve-endowed parts. Another and most important advantage is, that whereas anæsthetic mixtures all vary in composition by prolonged employment, and clog the instrument with unevaporated heavy fluid, no such effect occurs in this method.

For the mouth-piece I employ Snow's, or any other with a similar valve arrangement. The part of my instrument which is peculiar is the receptacle for chloroform and the corresponding one for the alcohol and ether. This part consists of two separate pieces, an upper and a lower (Figs. 1 and 2); the upper fitting as in a socket into the vertical part of the lower tube. The upper part consists of a tube (*a*) of a little more than two inches diameter,

and is three in height. It is fitted with a perforated lid (*b*), at each side of which is a small funnel (*c*). This tube is divided through its whole length by a metal diaphragm (*e*), which also runs down into the smaller tube fitted into it at the bottom. The whole forms one compact piece from top to bottom, and enclosing two entirely distinct spaces, one of which is reserved for the chloroform, and the other for the alcohol and ether. These have no communication with each other, but have a common opening of an elliptical figure at the bottom (*e*) of the smaller tube. This smaller tube (*d*) is accurately fitted, so as to work easily round in the vertical piece of the other part (Fig. 2) of the instrument. In so doing it necessarily exposes, according to the direction in which the larger tube is turned, first one side of the opening (say that leading to the alcohol and ether compartment), and then the other, leading to the chloroform. It is thus seen that we can perfectly control the nature of the vapour allowed to pass into the other part of this instrument, according as we cause it to turn to the right or to the left.



The second part (Fig. 2) of my instrument forms the receptacle for the socket (*a*) of the first, and carries the vapour up to the mouth-piece. But in order thoroughly to mix the separate vapours before they are breathed, a piece (*c*) of perforated gilt metal is fixed inside the horizontal arm (*b*) of this piece, and the diffusion thus caused effects a complete mixture before the vapours rise to the mouth. This same horizontal arm carries the index-finger, which points to the scale engraved at the upper part of the receptacle, and by its means (the scale having been carefully adjusted by experiment to the openings below) the quantity and the quality of the vapour are at once read off. What is told by this scale is represented below in the diagram. The rise of the lines



exhibits the gradual increase of aperture. The graduated marks answer the double purpose of an index and a safeguard; for the index-finger fits into them with a spring, and holds the apparatus at the required degree until it be

desirable to shift it farther. The instrument may thus be consigned to a nurse for a time, and the operator feel secure that no excess of chloroform can be accidentally administered.

[In a second paper published by Mr. Ellis on this subject, he says that during the three months which have elapsed since the first article was written, he has used the apparatus in a number of instances, and has added to it several improvements.]

The fluids I employ are—1. *Anæsthetic alcohol.* This has a specific gravity of 800 at 60° Fahr., is free from methylated adulterations, and may be prepared by the process indicated in the British Pharmacopœia. 2. *Anæsthetic ether.* This is perfectly free from methyl, should have a specific gravity of 720, and may be prepared by well washing the best sulphuric ether in distilled water, and subsequently drawing it off by a gentle heat into a receiver. It should be used fresh, and possess an agreeable aromatic odour, free from any acrid or pungent character. 3. *Pure chloroform.* This should have a specific gravity of 1495, and be absolutely pure and free from all contamination, whether of methyl or chlorine. The Edinburgh chloroform is that which alone I have used. The alcohol and ether are slightly aromatised, and tinged with colour to distinguish them from each other and the chloroform.*

The principles of my system of anæsthesia are to make provision for—

1st. Entire security against the excessive action of either of the anæsthetics.

2nd. The production of a modified anæsthesia, *varying at the will of the operator*, from a mere feeling of exhilaration to the deep unconsciousness requisite for abolishing acute pain.

3rd. The power of maintaining this state of anæsthesia at the same degree for any requisite period, or of modifying it to arising exigencies.

4th. The reduction of the dose of chloroform to its lowest practicable point.

5th. The partial substitution for it of a vaporous basis of mixed alcohol and ether, whereby its properties are enhanced and sustained, and its dose diminished without abatement of its value as an anæsthetic.

6th. The counteraction of the heart-depressing power of chloroform, by combining it with a heart-stimulant, and thus obviating some of the most frequent causes of danger in chloroformization.

The apparatus by which I have perfectly succeeded in fulfilling these objects is identical in principle with that described in my original article, but is different in its detail and method of construction. It has been in its improved form extremely well manufactured from my designs by Messrs. Savigny, of St. James's-street, from whom it can be obtained.

My object having been chiefly to get a good arrangement for use in the lying-in chamber, I have devised the following instrument, which has proved really valuable. It consists of a metallic cylinder about three inches in height by two inches and three-quarters in diameter. On one side, near the upper part, the tube emerges which conveys the vapourised air from the chambers contained in the cylinder. This elbow tube has attached to it a valve for admitting air at the commencement of the process, and it is movable so as to adapt itself to the different positions of the patient. Penetrating the cylinder, it communicates with a small circular chamber, at the bottom of which are two apertures, one connected with the chloroform side, and the other with the alcohol and ether. The different currents of air charged with vapours are thus caused to come together at the bottom of this chamber, and the openings in it are covered by a circular valve perforated in one half, so as to correspond with these openings, and the other half being left blank. A little consideration will show the reader that if this flat plate be turned by a pin passing through its centre, it will, according to the direction of the rotation given to it by the pin, uncover either one or other of these openings, and at one point both are wide open. After a few respirations the air-valve is gradually closed, and the index-finger is advanced by single degrees up to ten. This is the

* Messrs. Savory and Moore, of New Bond-street, have given much attention to the preparation of these fluids, and the products of their establishment may be thoroughly depended upon for excellence and purity.

point where chloroform influence faintly commences. The index is now moved still more gradually until it marks seventeen or eighteen degrees on the scale. Of these degrees, ten represent the mixed vapour of alcohol and ether, and the remainder that of chloroform. I very seldom find it necessary to advance the index further than this point in a confinement; but for a surgical operation it will perhaps be necessary to go beyond it, and to give the vapour uninterruptedly from both sides of the apparatus; or if very deep anæsthesia be demanded, then the index must be taken a full half turn onward, so as to give the predominance to the chloroform, and render the alcohol and ether mere auxiliaries. It is satisfactory to know that it is but seldom that the full power of the instrument is really required, while at no point is it capable of giving a dangerous dose to the patient. Opposite to the elbow is a small space marked "off," indicating that when the finger is at that point, no vapour whatever can leave the apparatus. Thus it can be turned off in the intermission of the inspiration, and the waste of the fluids prevented.

Two perfectly novel and remarkable features of this arrangement are illustrated in the apparatus: first, the method of supplying the chloroform and ether, which by a simple adaptation of the laws of capillary attraction, *cannot* escape in any dose exceeding about 2½ per cent.; and, secondly, the method of charging the air with alcohol vapour by an arrangement of cambric folds resembling the gills of a fish, and, indeed, designed from a consideration of those organs. By this means I have been enabled to give to the patient a larger proportion of alcohol in vapour than has ever been attempted hitherto, and with most excellent results.

Lastly, the economy of my method is most extraordinary. As a rule, in midwifery I have not consumed more than a *single drachm* of chloroform, and the same quantity of alcohol and ether, *per hour* of inhalation—intermittent, be it remembered, as it always is in obstetrics; and I believe from a drachm and a half to two drachms would be the extremest quantity necessary for any ordinary purpose. This may be well contrasted with the ounces of chloroform and the pints of ether hitherto deemed essential.

As I do not practise the art of chloroformization beyond the circle of my own professional domain, I have only the wish to make known a valuable addition to our art in the publication of this and the former article. I shall therefore feel honoured and happy if anyone with more leisure than myself will take up my views and pursue them to their widest application.—*Lancet, Feb.* 10, *and May* 12, 1866, *pp.* 145, 509.

SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—The germs of the disease lie scattered far beyond the apparent limits of the tumour, and hence, the apparent return of the disease is but the continued development of germs which were not included in the extirpation of the tumour. This is not merely theory, for on examining the tissues in the neighbourhood of such a tumour, little deposits of the morbid structure may frequently be seen, quite distinct from one another, and varying in size from the smallest pin's head to a large grain of wheat. In one instance some of these deposits were at least 3½ inches from the original seat of the mass. Probably the reason why an operation appears to intensify the disease is, that during it germs are set free and are carried by the lymphatics or veins, or in the areolar tissue, and becoming arrested here and there, take root, and are developed into new tumours. If this view, that cancer is a local disease, which becomes disseminated from the point of its first invasion is correct, the practice of early extirpation cannot be too much insisted on. After removal of such tumours by the knife, a solution of chloride of zinc, twenty to forty grains to the ounce, should be applied to the wound and thoroughly pressed in with a sponge; the surface will soon feel soft and creamy, and any germs of the disease will be destroyed. Chloride of zinc produces very little irritation beyond the point to which it is applied; it may therefore be worked into the inner surface of the flap till the tissue is softened to within a few lines of the surface. (Mr. C. De Morgan and Mr. Moore, pp. 147, 150.)

Citric Acid.—In cases of ulcerating and painful cancer, as of the mouth, breast, or uterus, a solution of citric acid locally applied will give complete relief. (Mr. C. J. Denny, p. 44.)

Citric, Acetic, and Carbolic Acids.—Citric, acetic, and carbolic acids, when applied in a diluted state to cancerous tumours, have a powerful effect in removing pain. The carbolic acid has a powerful effect in correcting the offensive fœtor of cancerous discharges. All three have a solvent effect on cancerous tissue, the citric acid least, the acetic acid next in degree, and the carbolic acid most. When a weak solution of carbolic acid is applied to cancer cells under the microscope, they are dissolved and the nucleus even disappears almost entirely. In a case of mammary tumour, to which it was applied, the thick, serrated, and everted edges disappeared, and cicatrization of many of the sores occurred. The following is the best formula:—Acidi carbolici ℥iss.; spiritûs vini rectificati ℥j.; aquae ad ℥ij. (Dr. J. Barclay, p. 329.)

CONVALESCENCE FROM INFLAMMATION.—Nitric acid is the most suitable medicine known for the state of convalescence from inflammation, and in the various cachetic states following on acute disease, or on habits of intemperance. Its beneficial influences may be referred to its possessing a large proportion of oxygen in loose combination, which promotes a more free circulation through diseased parts, or through effused solids, by further oxygenating them. (Dr. C. J. B. Williams, p. 128.)

FEVER IN CHILDREN.—*Diagnosis of, by Thermometer.*—The average temperature of the body in children from three to ten years of age is 99°. In cases of real fever it rises rapidly to 102°, 103°, or 104°, whereas it remains at or below the normal temperature in cases where real fever is not present, although there is foul tongue, quick pulse, and other symptoms of continued fever. Here we have a ready and accurate means of diagnosis. (Dr. G. S. Smith. p. 23.)

GOUT.—*Inhalation of Oxygen.*—The formation of urea instead of uric acid may be determined by supplying oxygen. Oxygen may be directly inhaled, or the binoxide of hydrogen exhibited in one-drachm doses diluted in about two ounces of water. This has a marked influence on the biliary secretion, which it increases in quantity and improves in quality, often producing excessive biliary dejections, thus relieving congested livers and secondary bronchial congestion. The inhalation of a mixture of oxygen and air, in the proportion of one to four, pretty uniformly clears the urine of lithates. (Dr. R. H. Goolden, p. 48.)

TYPHOID FEVER.—The most successful plan of treatment of typhoid fever (only one case in forty proving fatal) is the following: Strong beef tea and milk every two hours, together fourteen pints every twenty-four hours, and twenty drops of dilute nitro-muriatic acid every two hours. The late Dr. Henderson, of Shanghai, stated that by its adoption the mortality from continued fever was diminished from 28 per cent. to 7 per cent. (Dr. T. K. Chambers, p. 43.)

Temperature in Typhoid Fever.—Typhoid fever is not present, if, on the evening of the first or second day of the disease, the temperature be 104°; if the evening temperature do not rise to 103·3° between the fourth and sixth days; if, in the second half of the first week, the evening temperature decrease considerably; lastly, if, between the eighth and eleventh days, the temperature be below 103°. (Dr. F. W. Gibson, p. 30.)

TYPHUS FEVER.—*Amount of Urea Excreted.*—During the second week of typhus fever the quantity of urea excreted daily is decidedly below the standard of health, notwithstanding the presence of a state of high fever. This is contrary to the generally received doctrine that fever consists essentially in an increased metamorphosis of tissue. The intensity of the head symptoms coincides with the lowest urea excretion, and their improvement coincides with or follows on an increase in that secretion. (Dr. K. Anderson, p. 38.)

Temperature in Typhus and Typhoid Fevers.—The temperature of the body affords more reliable information respecting the severity of a case of typhoid fever than either the tongue or the pulse. It is also of great use in diagnosis. Thus, if on the fourth or fifth day the maximum temperature attained during the twenty-four hours be not 103·5°, the disease is most probably not typhoid fever, whilst, on the other hand, if on the first or second day the maximum temperature reaches 104° the disease is some other than typhoid fever. During the second week the thermometer is of great use as a means of prognosis. The greater the fluctuation between the morning temperature (lower) and the evening temperature (higher), the more favourable is the attack, and the shorter will be its duration. In cases of persistent elevation of the temperature, the earliest sign of improvement is a fall of the morning temperature. (Dr. Ringer, p. 21.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—*Calabar Bean.*—The calabar bean promises to be an effectual remedy for chorea. It should be given in half-drachm doses of the tincture (℥j. of the bean to ℥i. spirit of wine) three times a day, in water, and gradually increased if necessary. In one case which had gained no ground for more than two months, this plan of treatment produced most marked improvement in a fortnight, and cure in four weeks. In some cases no benefit seems to follow its use, but this may be said of any medicine. (Dr. J. W. Ogle, p. 71.)

EPILEPSY.—*Turpentine.*—Turpentine is a very valuable remedy in epilepsy. It may be given in half-drachm doses three times a day, or in a single dose of three drachms at occasional intervals, combined with an equal quantity of castor oil. (Dr. Headland, p. 250.)

Epilepsy, Hysteria, Neuralgia.—In cases with a distinct aura, a narrow circular blister applied all round a limb, a toe, or a finger, or a circular cauterization with a white-hot iron may cure the case, or at any rate be productive of benefit. (Dr. Brown-Séquard, p. 66.)

FUNCTIONAL NERVOUS DISORDERS.—*Mental Occupation in.*—In cases of hypochondria, of hysteria, of chorea, and even of epilepsy, a great benefit can be derived by inducing the patient to have some serious aim, whereby the mental faculties may find healthy employment. (Dr. C. E. Brown-Séquard, p. 64.)

Bromide of Ammonium.—It is of the greatest importance to procure sleep in patients attacked with a morbid increase of the reflex excitability. In cases in which there is sleeplessness owing to some cause of cerebral excitement, sleep is almost invariably induced by giving to adults a dose of thirty grains of bromide of ammonium a quarter of an hour before the last meal, and a second dose of from thirty to fifty grains at bedtime. (Dr. C. E. Brown-Séquard, p. 65)

LUMBAGO —*Ether Spray.*—If there is no gouty or rheumatic condition, instant relief is sometimes attainable by the use of Richardson's ether spray. (Mr. J. B. Walker, *Lancet*, March 17, 1866, p. 299.)

NEURALGIA.—The application of the ether spray of Richardson's apparatus for producing local anæsthesia gives immediate relief in cases of neuralgia. (Dr. B. W. Richardson. p. 261.)

Subcutaneous Injection of Atropine.—In cases of neuralgia not only may morphia be injected subcutaneously but aconitine in doses of $\frac{1}{10}$ of a grain, and atropia in doses of $\frac{1}{30}$ of a grain, may be similarly employed, and sometimes with benefit, where morphia has failed. (Dr. J. W. Ogle, p. 72)

PAIN.—*Subcutaneous Injection of Morphia and Atropine.*—Inject together half a grain of the sulphate of morphia with one-sixtieth of a grain of sulphate of atropine, and the good effects of both against pain are obtained, without the bad effects. (Dr. C. E. Brown-Séquard, p. 252.)

PROGRESSIVE LOCOMOTOR ATAXY, *or Wasting of the Posterior Columns of the Spinal Cord.*—In a well-marked case of tabes dorsalis (so called by Hippocrates) there is an actual waste of nerve fibres of the posterior columns of the spinal cord. The principal symptoms are a want of co-ordination of voluntary movements, and a tendency on the part of the patient to lose his balance, but without much actual loss of power. The only remedy which seems to be of any avail is the nitrate of silver, given in doses of one-tenth to half a grain two or three times a day ; it may be continued for four or six weeks consecutively, and then discontinued for a fortnight or three weeks. The gums must be examined from time to time, as the peculiar coloration which silver produces in the long run first appears in the mucous membrane, and only afterwards in the skin. It acts better when combined with the hypophosphite of soda. The disease was supposed by Hippocrates to arise from excesses in sexual intercourse. It, however, also arises from exposure to cold, exhaustion, and rheumatism. (Dr. J. Althaus, p. 50.)

Until recently this disease has been included under the class paraplegia. The symptoms however do not consist in actual paralysis of the muscles, but a loss of power to guide them. If the patient shuts his eyes he stops and sometimes falls. The microscope shows disease of the posterior columns of the spinal cord. There is no loss of sexual power. (Dr Johnson, p. 55.)

SPINE—*Effects of Cold and Heat to the.*—Cold and heat applied to the spine produce definite and constant effects, exactly the opposite of one another. Ice applied to the spine increases the general circulation, stops the cramp of voluntary and involuntary muscles, proves an effective remedy in epilepsy and other convulsive affections, cures sea-sickness, restrains the sickness of pregnancy, arrests diarrhœa, recovers patients from the cold stage of cholera, and finally promotes menstruation. On the other hand heat along

the spine lessens the general circulation, overcomes congestion in all parts of the body, lessens fever, restrains hemorrhage, and lessens or arrests the menstrual flow. (Dr. J. Chapman, p. 69.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANEURISM.—In tying the femoral artery the greatest nicety is required, and this will ensure almost perfect safety. First expose the sheath of the vessels carefully, next close every little vessel that discharges enough to obscure the parts; then seize the sheath with dissecting forceps, and gently raising it, make a small opening by means of a straight narrow sharp-pointed knife. Elevate the cellular and fatty substances which envelop the vessels until the external coat of the artery appears quite distinct and *white*, when the needle may be passed with ease and safety. (Prof. Syme, p. 139.)

Galvano-puncture.—In cases of aneurism, especially those which cannot be reached by surgical means, as compression, the only successful plan of treatment open is that by galvano-puncture. Introduce from the same side two needles into the sac, parallel to each other, and from one to two inches apart. The needles should be carefully insulated. They should be extremely fine, but may be multiplied according to the size of the aneurism. They should be made of steel, coated with gold, as the steel is eaten away with great rapidity, and the surface acting is thus diminished. A battery of medium strength, and with a continuous current, should be connected with the needles. The action may be continued till pulsation stops, or gas can be clearly detected by percussion. In a case of aortic aneurism related, the current was passed two hours and ten minutes. (Dr. J. Duncan, p. 133.)

Popliteal Aneurism.—Let the leg be bandaged and elevated, the patient confined to bed, and a grain of opium be given at bedtime if restless. Bend the knee at an acute angle so as entirely to stop pulsation in the aneurism. A pad may also be strapped over the tumour, and, if necessary, a double tourniquet applied over the superficial femoral artery. By simple flexion at an acute angle, all pulsation through the tumour is arrested, and this the more perfectly the higher the tumour is situated in the popliteal space. A tumour situated in the middle of the space, that is, at that spot which is opposite the interval between the tibia and femur, is not curable by this plan of treatment. (Mr. J. Hilton, p. 140.)

CONGESTION.—*Internal Use of Chloroform.*—Chloroform given by the stomach and rectum does not produce any symptoms of anæsthesia, but has the effect of equalizing the circulation of the blood and so relieving congestion. It may be thus used in all cases of congestion whether sthenic or asthenic. (Dr. Merril, *Lancet, Nov.* 25, 1865, *p.* 605.)

HEMORRHAGE.—*Xylo Styptic Ether Spray.*—A solution consisting of absolute ether, having a boiling point of 92° Fahr., charged to saturation at a low temperature with tannin, and, afterwards treated with xylodine, a little short of saturation, produces good local anæsthesia, and has the effect of producing complete coagulation of the blood in the surface of the wound exposed to the spray. The primary effects are of course those produced by the cold, namely, the condensation and whitening of the tissues. If blood be flowing it solidifies, and when the parts relax, new blood that may ooze up enters the solid blood as though it were a sponge, quickly solidifying by coagulation and stopping further flow. The effect is so powerful that, even when the spray is directed on partially defibrinated and decomposing blood, solidification ensues. (Dr. B. W. Richardson, p. 264.)

Hemorrhage from Wounds of the Hand.—In cases of hemorrhage from the hand occurring from several points simultaneously, flex the forearm forcibly upon the arm, and the hemorrhage will cease, owing to the acute angle at which the radial and ulnar arteries are placed. (Mr. J. Hilton, p. 142.)

PYÆMIA AND SEPTICÆMIA.—In pyæmia and septicæmia, and all the class of fevers with putridity and purulent absorption, such as hospital fever,

puerperal fever, anatomical inoculation, and the like, give sulphite of magnesia so as to saturate the organism with it rapidly, and cause the fluids of the body—urine, saliva, sweat—to contain it. It will rapidly check the effects of putrefactive absorption. (Dr. G. Polli, p. 233.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

CHRONIC BRONCHITIS OF THE AGED.—When, in chronic bronchitis in old people there is great dyspnœa, accompanied by a dry state of tubes, the tincture of stramonium given in ten minim doses along with other expectorants is very useful. (Dr. Headland, p. 250.)

CROUP AND DIPHTHERIA.—*Tracheotomy.*—Whatever may be the primary affection, whether sthenic croup, diphtheritic croup, or genuine diphtheria, death occurs in fatal cases from one or two causes, viz., exhaustion or suffocation. At an early period of those cases, the type of which tends to death by suffocation, before the energies of the patient have been wasted by the progress of the disease, perform tracheotomy. If you wait longer it will be too late. (Dr. G. Buchanan, p. 94.)

DIPHTHERIA.—*Hyposulphites.*—The incipient exudation of diphtheria may frequently be removed in forty-eight hours by the application twice a day of a strong solution of hyposulphite of soda, viz., 3iij of the hyposulphite, glycerine 3ij., with 3vj. of water. A weak gargle may be used every hour. (Mr. J. C. Maynard, p. 93.)

LARYNX SPECULUM.—An instrument has been invented by Dr. Labordette, of Lisieux, in appearance like a vaginal speculum, but adapted to the throat, and having a mirror so placed that when introduced into the mouth the larynx is reflected. A great deal of light is economised by reflection from the polished interior of the speculum tube. It may be obtained of Maw and Son, of Aldersgate-street. (p. 81.)

TUBERCULOSIS.—Tuberculosis is owing to imperfect or abnormal pancreatic secretion, whereby the fatty part of the food is imperfectly prepared for absorption. This is frequently aided by an unhealthy condition of the intestinal mucous membrane. When the case is so far advanced that it is considered hopeless to aim at restoring the function of the pancreas and the absorbing power of the intestine, we must give pancreatic emulsions of solid fat or of oil, in the hope that some absorbing power for such matter may yet remain in the digestive tract. The proportion of oxygen in the air presented to the lungs must be diminished in order to preserve the albuminoid tissues from disintegration to the greatest possible extent, and for the same reason artificial warmth must be supplied. If the symptoms, however, are only premonitory, the principles of treatment must be quite the opposite of those detailed: the function of the pancreas must be restored as quickly as possible by placing the patient under those conditions which call for pancreatic action. These are an atmosphere rich in oxygen, a climate at once cold and dry, exercise, cheerfulness, and a good mixed diet. The bulk of cases, however, are intermediate between these two, and require a nice adjustment of the plans of treatment. (Dr. H. Dobell, p. 73.)

Diagnosis of Tubercular Diseases by the Thermometer.—The temperature is always preternaturally raised when tubercle is being deposited in any of the organs of the body; this elevation often continues for many months. The temperature reached in tuberculosis varies from any point above that of health to 105° or 106° Fahr. The usual maximum temperature of the day varies between 102° and 103° Fahr. An appeal to the thermometer will not unfrequently decide the diagnosis in a case of suspected tubercular deposit in the meninges of the brain. We cannot decide by it between tubercular and non-tubercular inflammation, but we can between tubercular and non-tubercular tumours. (Ed. *Medical Times and Gazette, p.* 82.)

PHTHISIS.—*Inhalation of Carbolic Acid.*—The inhalation of carbolic acid in fine spray has sometimes a very favourable effect in cases of phthisis, allay-

ing irritation and checking hæmoptysis. Fifteen drops of the pure acid must be dissolved in ʒij. of spirits, and the solution mixed with ℥xxxij. of water. There are various instruments now sold for reducing fluid to a fine spray. (Dr. J. R. Wolfe, p. 87.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

CHOLERA.—Death in cholera results from the loss of the serum of the blood. After, therefore, the feculent discharges have ceased, and the rice-water evacuations commence, prevent the exit of the fluid from the rectum by means of a towel very tightly rolled up and placed longitudinally beneath the buttocks. The towel should be sprinkled with a solution of chloride of lime. The rectum may even be plugged in some cases. A flannel binder encircling the body exerts a wholesome pressure, and it may be sprinkled with chloroform to allay the spasms which are sometimes so excruciating. (Dr. John Gason, *Lancet, Dec.* 23. 1865, *p.* 697.)

Dr. Bowerbank, who has had experience of the effects of various modes of treatment of cholera at Jamaica, states that the administration of opiates and stimulants always proved injurious. He saw more good arise from the administration of Stevens' saline powders than from any other mode of treatment. He treated seventeen patients towards the close of the epidemic with iced sea-water; the patients took it greedily and all recovered. In almost every case after the fourth or sixth dose, the alvine dejections and the vomited matters became tinged with bile. (*British Medical Journal, May* 5, 1866, *p.* 470.)

Choleraic Diarrhœa.—When abruptly checked by opium, choleraic diarrhœa often returns immediately the effects of the opium passes off, the patient meanwhile having a hot skin, quick pulse, and headache, or the patient may pass at once into the stage of collapse. It should be treated by emetics, castor-oil, or some other mild laxative, and cold water; not one case out of a large number so treated passed into the stage of collapse. The effect of the opium is highly prejudicial, for it arrests the elimination of the poison through the mucous membrane of the bowels. (Dr. Johnson, *British Medical Journal, May* 12, 1866, *p.* 483.)

CONSTIPATION.—*Atropia.*—Atropia when it has been taken internally for a few days produces slight relaxation of the bowels, and if constipation existed it is removed. The stools, however, are but little altered in character, and the intestinal secretion but slightly increased. This peculiar action of atropia arises from its depriving the mucous membrane of the bowel of its mucous covering, whereby the fæces more readily excite muscular action. The action of atropia differs from that of common purgatives in that its relaxing power is not followed by a disposition to constipation. In cases of simple constipation the following plan of treatment is the best: ℞. Magnesiæ sulphatis ʒj; acidi sulphurici aromat. ♏x; tinct. aurantii ʒj.; aquæ ad ℥j. M. Let the above draught be given the first thing in the morning, and last thing at night, on an empty stomach, and let $\frac{1}{60}$ of a grain of atropia be added to the draught at bedtime. (Dr. A. Fleming, p. 88.)

Obstinate Constipation.—Electricity.—When electricity is employed for the relief of obstinate constipation it must be conducted through the rectum, care being taken to employ it gently, but repeatedly, our object being rather to restore the action of the bowel by small but successive doses of the stimulus, instead of attempting to dislodge the impacted contents by one powerful application of the galvanic current. It may be easily applied by passing a copper wire with a button on the distal extremity, through a rectum tube, the excitor of the other wire being applied to the abdominal walls. Duchenne's galvano-magnetic machine is the best for this purpose. (Dr. A. Fleming, p. 90.)

CHRONIC ENLARGEMENT OF THE LIVER.—Mercury is useless and indeed actually injurious in cases of chronic enlargement of the liver, such as occurs amongst Europeans in the East Indies. The nitro-muriatic acid bath

is of great use. An ounce and a half of strong acid must be added to each gallon of water to form a bath. The patient before going into the bath should be covered over with blankets until a gentle perspiration is induced, and when in the bath a covering blanket should be drawn over the head and shoulders to confine the steam and enable the patient to inspire it. The external application of acids act far more quickly and surely than their administration by the mouth. Even in irremediable cases the bath proves useful in cleaning the tongue, improving the appetite, abating thirst, and in many cases in retarding the progress of disease. It is necessary that the bath be made of wood, and it must be no larger than is absolutely needful to contain the person. When the patient is in so weakened a condition as to be unable to bear the immersion or sponging, a swathe damped in the solution must be worn round the body and covered with oiled silk. This may be continued for any length of time, and great advantage is often the result. (Sir J. Ranald Martin, p. 97.)

DIGESTION.—*The influence of dilute Alcohol on.*—Even the most dilute alcohol arrests digestion of animal food until it has been absorbed; digestion then goes on slowly and less perfectly than natural. A little of Allsopp's bitter ale completely arrests the artificial digestion of beef in a solution of gastric juice, which would otherwise completely digest it in eight hours. (Dr. Munroe, p. 254.)

OBSTRUCTION OF THE BOWELS.—Inject by means of the long tube of a stomach-pump a warm fluid as a solvent or diluent, allow it to remain two or three hours, then reverse the action of the pump and eject, or rather draw out. A very interesting case illustrating the success of this plan of treatment is related. (Dr. E. Fleming, p. 248.)

PHAGEDENIC ULCERATION OF THE MOUTH, OR SOFT PALATE.—Let the patient inhale a mixture of oxygen and atmospheric air, in the proportion of one part of the former to four of the latter. Rapid improvement generally results. A vulcanised india-rubber bag may be used, with a stop-cock and mouth-piece. (Dr. R. H. Goolden, p. 48.)

TÆNIA.—*Turpentine.*—Oil of turpentine, combined with castor oil, three drachms of each, is a better remedy for tænia than oil of male-fern. (Dr. Headland, p. 250.)

AFFECTIONS OF THE URINARY ORGANS.

ACUTE ORCHITIS.—*Puncture and Compression by Strapping.*—In cases of acute orchitis after puncturing the testicle through the front or lower part, so as at the same time to let off the serum in the tunica vaginalis, and relieve the tension of the tunica albuginea, so soon as the bleeding has ceased apply a firm case of strapping, put on so tightly as the patient can bear it. A full dose of opium should be given at the same time, and repeated twice or three times. (Dr. W. Spencer Watson, p. 145.)

AMMONIACAL URINE.—Ammoniacal urine depends upon decomposition of the urea, owing to inflammatory action in the mucous membrane of the bladder, and increases the inflammatory mischief. The urine is usually acid when it first enters the bladder. In the treatment, the main object to be attained is the stoppage of the formation of carbonate of ammonia in the bladder. For this purpose the bladder must be kept as empty as possible by the more or less frequent introduction of the catheter, when this does not produce much irritation. Vegetable acids run off far more freely by the urine than do mineral ones and hence are most suitable to these cases. Citric acid or lemon juice should be given in sufficient doses to keep the urine feebly acid. By washing the bladder with small quantities of hydrochloric or acetic acid and water, after the urine is drawn off with the catheter, the ammonia may be easily neutralised, and a very small quantity of feebly acid fluid may be left in the bladder to counteract the decomposition of the urea, and to make the surface of the bladder itself less foul. (Dr. Bence Jones, p. 102.)

BILE IN THE URINE.—*Detection by Chloroform.*—Place in the test tube forty or fifty grammes of the urine to be examined, and add to it four or five grammes of chloroform, and then shake the mixture. If the urine contains bile it immediately assumes a fine yellow colour, and on allowing it to rest, the chloroform falls to the bottom of the tube, drawing with it the fatty matters of the urine coloured by the yellow biliary substance. (M. Cunisset, p. 253.)

BRIGHT'S DISEASE.—The bowels, skin, and lungs, are more powerful auxiliaries in the elimination of urinary products than is generally supposed. The cutaneous perspiration not only carries off water but many of the organic as well as inorganic urinary salts. Thus in health, it has been found that the sweat contains urea, uric acid, phosphates, and chlorides, while in disease it contains even the insoluble oxalate of lime. Urea and urates are also contained in the pulmonary exhalation. The vicarious action of the bowels is to be induced by the internal administration of mild or drastic purgatives. When there is much dropsy, elaterium combined with hyoscyamus is a favourite remedy. (Dr. G. Harley, p. 110.)

LITHOTRITY.—Under certain restrictions with regard to the manner of operating, lithotrity is an eminently successful operation, and is attended with very little risk. The following rules should be observed: Let the natural efforts be trusted to as much as possible for the removal of the débris. Do not inject the bladder before operating. Introduce the lithotrite only once at each sitting. Let the lithotrite remain in the bladder only from one minute to a minute and a half at each sitting. It is rarely necessary to use the fenestrated lithotrite more than once at the outset of a case, in dealing even with a very large stone. For subsequent sittings, and for smaller stones, at the outset, the flat-bladed instrument, which is much less irritating to the bladder and pulverizes more completely, should be alone employed. (Mr. H. Thompson, p. 143.)

LITHOTOMY.—*New Instrument for.*—A new instrument has been devised by Mr. Henry Lee for facilitating the operation of lithotomy. It consists of two blades like an ordinary lithotrite, but the lower one is deeply grooved on the right side. It is passed and the stone seized and held between the blades as near the neck of the bladder as possible. The operation is now proceeded with, the groove serving as a guide to the knife. As the groove terminates half an inch from the commencement of the curve, the incision is not carried through the prostate, which must be dilated with the finger and pressed back over the stone at the same time that this is pulled forward and removed. The instrument is thus seized before any incision is made, and all delay in the seizure of the stone by the forceps after an opening has been made is avoided. Moreover the instrument itself determines the extent of incision necessary for the size of the stone, for a large stone separates the blades more, and so causes the groove on the under blade to guide the knife deeper. (Mr. H. Lee, p. 145.)

POLYDIPSIA.—(*Diabetes Insipidus.*)—Valerian administered in large and rapidly increasing doses is a most serviceable remedy in diabetes insipidus. A case is related in which the specific gravity of the urine rapidly rose from 1000 to 1006 under large doses of valerian in powder. (Dr. Reith, p. 48.)

TUBULAR NEPHRITIS.—Tartarized antimony has no strong action on the kidneys, and has a very decided action on the skin, hence its applicability to cases of tubular nephritis which generally results from stoppage of the action of the skin from cold, or is accompanied by that condition as following scarlet fever. Digitaline is the only diuretic suitable. As the disease becomes more chronic, iodide of potassium may be used in diuretic doses. (Dr. Bence Jones, p. 105.)

AFFECTIONS OF BONES, JOINTS, ETC.

COMPLETE ANCHYLOSIS OF THE KNEE-JOINT.—*Forcible Flexion.*—In a very interesting case of complete anchylosis of the knee-joint from rheu-

matic arthritis, in which forcible flexion was successfully performed by Dr. P. C. Smyly of Dublin, the points worth noting were as follows: Two strong iron splints, well padded with cotton wool, were bandaged to the thigh—one in front and one behind—in order to avoid the danger of breaking the thigh bone instead of the knee. Dr. Smyly remarked that he had been present on two occasions when the femur gave way in the middle. After the operation the limb was placed in a long-jointed splint with a screw behind, and at an angle of about forty-five degrees. The knee was kept constantly iced for forty-eight hours and the foot kept warm. Every day the position was slightly changed, so that towards the end of a week the limb was again straight. For about six weeks a gradual process of flexion and extension was persevered in, the splint was then removed. (Dr. P. C. Smyly, p. 121.)

Dislocation of the Shoulder-joint.—*To Reduce*—Kneel behind the patient and place the palm of one hand firmly on the scapula and the fingers in the axilla under the head of the humerus. With the other hand grasp the condyles and press the arm to the side, then draw it backwards, and thus get a better hold with the fingers engaged at the head of the bone. By the next movement slide the arm forward and depress the elbow, at the same time rotating outwards and lifting the head of the bone into its place. By this plan many cases may be successfully reduced; but when the patient is very muscular, it is better to adopt the usual plan, with the heel in the axilla. (Mr. W. E. Porter, p. 123.)

Fractures of the Leg.—*New Form of Cradle.*—A new form of cradle for suspending the splinted leg in cases of severe fracture, invented by Mr. Henry Greenway, of Plymouth, is described at p. 128. It appears likely to conduce greatly to the comfort of the patient.

Fracture of the Radius at the Styloid Process.—The ordinary straight splints are not satisfactory for the treatment of this fracture, owing to the radius not being a straight bone. Its concavity at the wrist should be maintained by any splint adapted to it. The only splint which answers this end is that known as Gordon's splint, a description of which will be found at page 124. (Mr. L. Tait, p. 123.)

Injuries to the Head and Neck of the Humerus.—Injuries to the head and neck of the humerus are divisible into those *below* the tuberosities and those *through* the tuberosities. In the latter case there are all the symptoms of dislocation into the axilla *a few months after the accident*, for the upper fragment is dragged upwards with the fractured surface pointing outwards, owing to the action of the muscles inserted into the tuberosities; and the form of the neck being thus altered, the head is gradually displaced downwards in the axilla. It is necessary to forewarn a patient of this. The only plan likely to prevent this is to carry the patient's arm out, at right angles to the body. This would prove very inconvenient in practice, but may be tried if the use of a long straight splint, well padded, and pressed into the axilla, does not sufficiently prevent displacement. (Mr. J. Hutchinson, p. 126.)

Splints.—*Gutta-Percha.*—No matter in what form the gutta-percha is, nor in how many small threads or large balls, place it in a vessel of water, and boil it till it is perfectly softened throughout; now collect it into one ball; dip it in cold water to enable it to be handled without burning, and knead it into a uniform mass with the hands. To do this, if it is very good and tough, will require some strength, and also warming two or three times; but eventually it will form a uniform plastic mass not much harder than stiff putty. When in this state, place it on any flat surface which is wet, and with a roller—a common glass rolling-pin dipped in water is the best—roll it into a sheet of the desired thickness, let the roller slide upon it in all directions, and continue this action till the surface is uniform; it will now be fit for cutting into shape. This can readily be done, either when hot or cold, with a pair of scissors, and no force is required to mould it into the form of splint required, the only precaution requisite being to hold it steadily in that shape till it is cold; after which it will not alter, unless again made hot. It may sometimes be requisite to join two pieces of gutta-percha together.

This can be readily done by warming the two surfaces to be united by dry heat, with a hot iron, not burning, but merely softening the surface. Two such hot, dry, soft, clean surfaces will unite most perfectly when brought into contact with each other, if held together till they are cold. It may sometimes also be useful to know how two pieces of this material may be moulded on to each other without sticking. If one be cold and the other hot, they will not unite, especially if the cold surface be anointed with soap-suds. (Mr. E. Truman, p. 131.)

Leather-Felt Splints.—These simple and admirable splints consist of stout felt, lined on one side with wash-leather. All that is necessary for their application is to place a splint (with the leather side downwards) upon a table, then paint the felt side with a liquid which is supplied by the inventor (Mr. J. G. Hides, of Mortimer-street), then apply the splints to the injured limb, and bandage with an ordinary roller, leaving a little interval between each turn of the bandage. The splint hardens like a piece of wood in about thirty-five minutes. These splints are extremely light, and are capable of adaptation to any part without padding. They are about the same price as wood splints, and can be used again and again by merely painting the felt side afresh with a weaker solution of the liquid. (p. 130.)

Subacute Synovitis.—*Effusion from.*—After all inflammatory symptoms have diminished, if the amount of effusion shows no tendency to diminish, apply a large blister to the joint, and dress with mercurial ointment, or if the blistered surface shows a tendency to heal rapidly, with equal parts of mercurial and savin ointment. (Mr. W. Adams, p. 122.)

AFFECTIONS OF THE SKIN, ETC.

Chilblains.—Moisten a small piece of linen with the following, and apply it to the part, or it may be rubbed in occasionally: Balsam of Peru 3ss.; rectified spirit ℥iss. Dissolve, and add hydrochloric acid 3ss.; compound tincture of benzoin ℥ss., mix. When the chilblain is broken, calamine ointment is a very good dressing. (Dr. H. S. Purdon, p. 147.)

Eczema Infantum.—Give minute doses of arsenic, and one grain of calomel twice a week, at bedtime, unless active diarrhœa be present. Apply lint very thickly spread with zinc ointment, and allow the ointment to remain on for a week, when it may be removed with a spatula; a nearly healthy surface will then be exposed. (Mr. Erasmus Wilson, p. 148.)

Erysipelas.—*Warm versus Cold Applications.*—Never use cold applications in cases of erysipelas. Warm fomentations medicated with poppy or hemlock should be used. The vitality of the part in which erysipelas occurs is always lowered. (Mr. J. Hilton, p. 46.)

Gangrene, Suppurations, Sloughing, &c.—*External use of Sulphites.*—In all external wounds, sloughing, gangrene, and ulcerations, the application of the sulphites or hyposulphites of soda or potass are useful in destroying any putrefactive tendency, and exciting healthy action. One part of sulphite of soda may be dissolved in five or ten of water, and frequently used as a lotion, or on compresses of wet lint. (Dr. Polli, p. 233.)

Local Pains of the Walls of the Chest.—Chlorocarbon, whilst it possesses the anæsthetic properties of chloroform, has less of the local irritating effects of the latter. Ten or twenty drops injected subcutaneously will relieve pains of the walls of the chest or abdomen, without producing the nausea so frequently the result of the hypodermic injection of preparations of opium and morphia. (Sir J. Y. Simpson, *Medical Times and Gazette, Dec.* 16, 1865, *p.* 652.)

Pruritus Senilis.—A case is related in which the subcutaneous injection of one-fourth of a grain of morphia, repeated in two days, completely cured the intolerable itching. In another case one injection produced the same result. (Dr. H. Beigel, p. 148.)

Wounds.—*Chloride of Zinc Lotion.*—The discharge from a wound, whether

the result of accident or surgical operation, usually decomposes whenever covered for even a few hours, as evidenced by the peculiar sickly smell. If, however, the wound is well bathed with a lotion of twenty grains of chloride of zinc to the ounce of water, this is prevented, and the wound will frequently heal by the first intention, without swelling, pain, or discoloration. This plan has been tried in a variety of operations, the removal of tumours, amputations, even with extensive and thin flaps, operations about the rectum, involving the mucous membrane, and in the perineum. In many cases the wounds have healed in twenty-four hours, without fulness or swelling, and leaving a line of cicatrix which after a short time could hardly be seen or felt. (Mr. C. de Morgan, p. 112.)

VENEREAL AFFECTIONS.

GONORRHŒA AND GLEET.—*Soluble Bougies.*—Soluble bougies, two or three or more inches in length and made of cacao butter may be impregnated with nitrate of silver (a quarter of a grain), or tannin, or acetate of lead, and being introduced into the urethra in cases of chronic gonorrhœa or gleet, form an excellent means of thoroughly applying remedies to the mucous membrane. They may also be made sedative by impregnating them with opium or belladonna. It is probable that many of these cases are so troublesome because the remedies applied are not allowed to remain sufficiently long in contact with the mucous membrane. (Mr. Henry Thompson, *Lancet*, *May* 12, 1866, *p.* 513.)

SYPHILIS.—*Calomel Vapour Bath v. Syphilization.*—The treatment of a case of constitutional syphilis by syphilization takes on an average nineteen weeks, that by the calomel vapour bath takes five weeks and a half. It cannot be urged against the use of the calomel vapour bath that it is unsuccessful, for no case occurs of severe complication of the deeper structures where the calomel bath has been properly used for primary syphilis. The time required for the treatment of syphilis by syphilization is so long, and the inconveniencies attending it are so great that it is not likely to be adopted in private practice in England. The calomel vapour bath does not act effectually, unless a hot-air or vapour bath be first taken so as to ensure a supple and absorbing surface. (Mr. H. Lee, p. 165.)

AFFECTIONS OF THE EYE AND EAR.

ATROPINE PENCILS.—Pencils or points impregnated with atropine and adaptable into wooden holders, which may be carried about in the pocket are by far the most convenient mode of using atropine to dilate the pupil. If the atropine point is lightly passed, once or twice, over the conjunctiva of the lower eyelid a sufficient quantity of atropine is washed off to dilate the pupil for opthalmoscopic or other purposes. These points may be obtained from Mr. Curtis, of Baker-street, London. (Mr. J. Z. Laurence, p. 154.)

BLACK CATARACT.—There is a form of cataract in which to the naked eye all appears healthy, but by the opthalmoscope it is seen that the lens is of a deep brown or black colour. The sight is lost. These cases are rare. It is of course impossible to illuminate the interior of the eye. (Mr. Haynes Walton, p. 152.)

BLUE GLASSES.—Blue tinted glasses are on the authority of Von Graefe and Professor Böhm superior to neutral tint as a means of modifying the intensity of light. (p. 155.)

FUNDUS OCULI.—*To Examine without the Mirror of Ophthalmoscope.*—Place the lamp on a table in a darkened room and let the patient sit on a chair two or three feet from the table, and facing the light. If the pupil is well dilated with atropine the fundus is seen to be illuminated, the Surgeon sitting with his back to the light. A lens of about two inches focus is neces-

sary to see the details of the posterior internal surface of the eye. (Dr. Roseburgh, p. 149.)

GONORRHŒAL OPHTHALMIA IN CHILDREN.—Wash away the discharge every half-hour with lukewarm water, and apply within the lids a solution of alum of the strength of three grains to the ounce. The diluted nitrate of silver stick is also very useful. The nitrate is diluted with nitrate of potash. (Mr. Ernest Hart, p. 151.)

GRANULAR LIDS.—*Pressure.*—If pressure is applied to the granular lids without pressing on the globe of the eye, we frequently gain much benefit in these cases. This may readily be done by means of an instrument consisting of two ivory plates, one fitting into the concavity of the under surface of the lid and the other upon the convex upper surface: the upper plate can be approximated to the under by a screw, and the amount of pressure delicately adjusted. The lid compressor may at first be applied for one hour daily, and if well borne, for two, three, or four hours. One great advantage which this treatment has over the ordinary methods of scarification and subsequent application of various caustics, is that there is nothing in pressure which can tend to the production of entropium with trichiasis. (Dr. W. Stokes, Jun., p. 149.)

IRITIS.—*Treatment of by Atropine.*—One half of cases of iritis, whether syphilitic or not, can be cured by the application of atropine alone. A little solution of the strength of two grains to the ounce should be dropped into the eye six times, at intervals of five minutes in the morning, and six times also in the evening. On the following day, if the pain is lessened and the pupil is beginning to dilate the case is slight, and atropine alone will cure it. If the pupil is affected, and the symptoms are unabated, mercurial treatment must be commenced without delay. Mercury acts much better on the system when it is introduced through the skin than by the digestive organs. The patient should be confined to bed, and a broad piece of flannel well smeared with mercurial ointment wrapped around each arm. The gums generally become affected in from three to four days. It is not necessary to rub in the ointment. Atropine should be used during the whole period of treatment, except where it causes great pain or increases conjunctival irritation, in which case it may be temporarily suspended. (Mr. T. Pridgin Teale, Jun., *Ophthalmic Hospital Reports, Vol.* v., *Part* 2, *p.* 156.)

MALIGNANT AND RECURRENT TUMOURS OF THE ORBIT.—After removing the tumour with all the tissues as far back as the optic foramen, apply chloride of zinc paste spread on strips of lint to the bottom and sides of the orbit, so as to effectually destroy any germ of the disease which may have escaped the knife. The actual cautery may be applied to the periosteum, in spots where there appear to be small adherent particles of the disease which it is difficult to get away with the knife. (Mr. De Morgan, Mr. Hulke, Mr. Lawson, p. 155.)

SPECULUM AURIS.—(*Dr. Brunton's.*)—In examining the membrana tympana with the ordinary speculum there is frequently great difficulty in seeing owing to the observer's head obstructing the light, and the eye cannot be brought near enough the object to permit of minute examination. All this may be obviated by the use of an instrument on the principle of the endoscope, the light being admitted at the side. The following simple instrument is well adapted to the purpose. The instrument consists of a brass tube, two inches long and three quarters of an inch in diameter, to one end of which is made to fit on, by sliding, an ear-piece similar to Toynbee's aural speculum. At the other end is an eye-piece with a lens of moderately magnifying power; the eye piece slides so as to admit of focal arrangement to suit the eye of the observer. In the body of the instrument, near the ear-piece end, and set at an angle of 45°, is a concave mirror with a hole in the centre; this aperture in the mirror is in the line of the axis of the tube and ear-piece. At a right angle to the body of the instrument, and opposite to this mirror, is adapted a sliding funnel-shaped polished silver reflector for collecting and concentrating the rays of light, so that the rays are let in at the side, and, falling on the mirror are reflected and concentrated into the ear, and carried back to the eye of the

observer through the hole in the mirror, and are magnified by the lens of the eye-piece. There is also a handle attached to the light-reflector, which works on a sliding band, and can be turned about to suit either hand of the observer as he may wish, when looking at the right or left ear. With this instrument the smallest change of structure may be noted, and the movement of the membrane readily observed. The blood-vessels that traverse the membrane can be seen in the healthy ear. (Dr. J. Brunton, p. 156.)

TYMPANUM.—*Application of Pulverised Fluids to.*—By means of a specially contrived instrument pulverised fluids can be applied directly to the interior of the tympanic cavity, and the same instrument serves for the application of such agents as carbolic acid and creosote to nasal cavities in cases of ozœna. The apparatus consists of a small graduated glass syringe similar to that used for subcutaneous injection. The solution is gently forced guttatim into a cylinder, and at the point where it leaves the nozzle of the syringe it is caught by a current of air sent by a pump worked with a proper degree of force by the hand. This drives the fluid forward in a pulverised state. This small apparatus is then attached to an ordinary Eustachian catheter previously introduced, and suspended by a suitable apparatus. To ensure complete pulverisation, the end of the catheter is covered by fine gauze wire. (Dr. E. Bishop, p. 157.)

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

CHLOROCARBON IN MIDWIFERY.—Chlorocarbon will probably prove especially useful in midwifery, for it abolishes pain without affecting consciousness. It is during its early stage of action a powerful stimulant to the circulatory system. (Dr. Sânsom, *Lancet. March* 17, 1866, *p.* 288.)

CHLOROSIS.—*Iron.*—Iron is useful in chlorosis only because of a peculiar influence which causes the food to be transformed more easily into blood. Other metals which are not normal constituents of blood globules, such as manganese and silver, will sometimes act even better than iron. There is immense value in a purely hygienic means in the treatment of chlorosis. (Dr. Brown-Séquard. p. 221.)

DIFFICULT TURNING.—By the use of the following instrument for applying a running noose of tape round the ankle of the child when very high up, turning may be accomplished in cases where embryotomy apparently was the only method practicable. The instrument is a staff of German silver, with a horse-shoe shaped bi-valvular tube, with open bevelled extremities fixed to its top. At the handle is a spring, by means of which the horse-shoe shaped tube can be opened into two semi-circular grooved valves. A loop of the running noose of tape is put into, and fastened in, the bi-valvular tube. The instrument is then introduced, with the horse-shoe along the arm and hand of the accoucheur, up to the foot, which is then slipped into the circle formed by the tube and the free part of the loop, between the two extremities of the horse-shoe. By pressing on the spring the tube readily opens, and lets the loop of the running noose free, surrounding the child's ankle. It is then tightened by pulling on the free ends of the noose. (Dr. Hyernaux, p. 177.)

DISPLACEMENT OF THE UTERUS.—Take a circle of gutta-percha and enclose it in a copper wire of No. 12 thickness. A stem of the same wire, a continuation of that enclosed in the gutta-percha, and bent to the curve of the vagina, must be brought forwards to the upper edge of the mons veneris. The os and cervix must be placed in the circle, which must be deep enough to contain them. At the upper edge of the mons veneris the wire must be bent into two semi-circular horns, one to run along the lower part of the abdomen on either side, immediately above the groins. These horns should be fastened to the abdominal bandage by tapes or stout pins. The instrument is now secure, and invariably affords relief without giving rise to any pain or irritation. It requires a little modification in the shape of the gutta-percha cup according as the case is one of anteversion or retroversion. Its cost is not more than threepence. (Dr. Main, p. 195.)

EXTREME FLEXIONS OF THE UTERUS.—A description of a new instrument for the relief of extreme flexions of the uterus, invented by Dr. Wright, will be found at p. 198.

GLADULAR TUMOUR OF MAMMA, WITH CYSTS.—*Injection with Iodine.*—The usual practice in these cases is to remove the tumour or to lay it open and stuff with lint. Instead of that introduce the needle of a subcutaneous injection syringe and withdraw the fluid through it, then inject with tincture of iodine. The indurated gland around the cysts disappears when the cysts are destroyed. (Mr. W. Martin Coates, p. 221.)

HEMORRHAGE AFTER LABOUR.—*Injection of the Uterus with Perchloride of Iron.*—After clearing out the uterine cavity of placental remains and clots, inject a solution of perchloride of iron into the uterus. (Dr. R. Barnes, p. 175.)

Hemorrhage from Sub-involution of the Uterus after Abortion or Labour. There is not the least danger in injecting the uterus with styptic solutions such as a solution of perchloride of iron, or in swabbing the interior by means of a piece of sponge. It is necessary, however, that the internal os should be first freely dilated. We must rely greatly upon the improvement of the general health which is always more or less impaired. (Dr. R. Barnes, p. 180.)

HYSTERALGIA.—*Chlorocarbon Vapour in.*—In cases of hysteralgia, the injection of the vapour of chlorocarbon sometimes relieves the pain. The first patient in whose case this was tried said that she experienced so much relief that she slept the following night "far better than she had done for weeks." The best apparatus is a common enema syringe with the nozzle introduced into the vagina, and the other extremity placed in a phial containing the chlorocarbon. (Sir J. Y. Simpson, *Med. Times and Gazette, Dec.* 11, 1865, *p.* 651.)

LIEBIG'S FOOD FOR CHILDREN.—In the preparation of this food the malt should be well freed from husk and finely ground, and the wheat flour should be slightly baked, in order to partially cook the starch entering into its composition. A thermometer should also be employed in the preparation of the food, as a temperature ranging between 140° to 148° is amply sufficient to effect the complete transformation and solution of the starch corpuscles, and, indeed, to cook the food sufficiently. (Dr. A. H. Hassall, p. 228.)

MECHANICAL DYSMENORRHŒA.—The seat of obstruction is almost invariably at the os externum. The obstruction is due chiefly to the small round os itself; partly to the pointed elongated form of the lower part of the vaginal portion, and partly to an unusual rigidity of structure. The vaginal portion is in fact in excess, and is subject to flexions. These flexions, which may be either anterior, posterior, or lateral, are flexions of the cervix and not of the body, and the angulation sometimes produces stricture. Any plan of treatment which is not adapted to remove the mechanical cause is not likely to do more than mitigate the symptoms. Neither Simpson nor Marion Sims have found dilatation either safe or successful, and it is liable to be followed by ill consequences, such as pyæmia and peritonitis. The instrument used by Dr. Barnes is contrived to cut like a pair of scissors. One blade, strong, and somewhat probe-shaped, passes through the narrow os into the cervix, whilst the other blade, slightly concave on the cutting edge, so as better to seize the rounded surface of the cervix, is applied at any part of the vaginal portion the operator chooses. Then the part to be divided being exactly included between the blades is cut through. Thus we are sure of cutting as much as we want, and no more. There is this great advantage in cutting from without inwards, instead of from within outwards, that we are cutting towards safety and not towards danger. (Dr. R. Barnes, p. 212.)

OVARIAN TUMOURS.—*Chlorate of Potash.*—Four cases of ovarian disease are related in which the internal administration of chlorate of potash produced a most marked effect in causing lessening of the tumour; two were quite cured, the tumour altogether disappearing. A dessert-spoonful of a saturated solution of the salt was given thrice a day. (Mr. W. Craig, p. 209.)

OVARIOTOMY—In a case of ovariotomy related by Dr. Aveling, of Shef-

field, the pedicle which was short and about four inches broad, was transfixed with a needle carrying a double thread. The two ends of each ligature were then brought up together on either side, and drawn through two iron wire coils; each coil had fitted upon its end a cross-bar, and round these cross-bars the ligatures were secured after they had been drawn sufficiently tight to compress the vessels. The pedicle was then placed in the hook of Dr. Aveling's polyptrite and crushed through. As there was no hemorrhage it was returned at once in situ, and the wound closed with iron wire sutures. The heads of the two coil clamps were left protruding from the lower angle of the incision. In two days the ligatures were unwrapped from the cross-bars, the coils removed, and the ligatures drawn out; a good recovery followed. The coil clamp will especially be found of use where the pedicle is short. (Dr. J. H. Aveling, p. 207.)

Polypus Uteri.—It is not correct to say that there is no danger of hemorrhage in cases of excision of a uterine polypus, for some are fed by an artery which may be as large as the radial. It is better in all cases where it can be applied, to remove a polypus by the ecraseur, rather than by the knife or scissors. If, however, from the large size of the polypus, and the contracted state of the vagina this is impossible, the ligature with excision, or the ligature alone must be employed. (Dr. J. G. Swayne, p 192.)

Procidentia Uteri.—A descent of the uterus is invariably preceded by a prolapse of the anterior wall of the vagina, which draws down the cervix. The ordinary perineal operation has no effect in providing support where it is wanted, namely, anteriorly, at the connection of the cervix uteri with the base of the bladder. The redundant tissue of the anterior wall must be removed. Raise the hypertrophied vaginal tissue by a tenaculum and remove it, afterwards bringing the edges together by silver wire sutures passed transversely. (Dr. Marion Sims, p. 182.)

Prolapsus Uteri.—*Plastic Operation.*—Most of the failures which have occurred in operating for the permanent cure of prolapsus uteri have arisen from denuding the vagina of the mucous membrane in a pyramidal form, of which the apex is above. Just the opposite should be done. *The base should be above*, because here should occur the greatest resistance to the descent of the womb. It is always well to lay bare an amount of surface somewhat in excess of that which we really wish to adhere together, because union by the first intention never takes place on the edges adjoining the mucous membrane, however carefully they are brought together. The denuded surfaces are better kept together by flat ribbons of silver, one-sixteenth of an inch in breadth, and very thin and soft; of course the needle to carry this requires a transverse slit. Instead of the two pieces of quill or bougie ordinarily used, two pieces of stout silver wire must be soldered together at the ends; when the end of the flat ribbon is passed through the slit between these wires, it can be fastened at once by giving it two or three twists. One of these double wires of course is required for either side. It is unnecessary as a rule to divide the sphincter. A firm bandage with a perineal pad should be worn for two or three months after the operation, until the parts are consolidated. (Mr. R. Ellis, p. 185.)

Caustic Applications.—In a case of prolapsus uteri in a patient 72 years of age, Dr. Routh painted the strong nitric acid over a portion of the external skin, which was very loose, and over the mucous membrane of the vagina, about one and a half inches upwards, and for the lower half of the external opening. The pain was considerable at first, but subsided entirely after four hours. The sloughs separated on the eleventh day. The granulations were well washed by a stream of warm water, the parts were brought together by clamps, and two deep sutures applied. A catheter was left in the bladder. The case did well. (Dr. C. H. F. Routh, p. 188.)

Puerperal Albuminuria.—Puerperal convulsions are not generally the result of the true albuminuria of pregnancy: in reality they are much more frequently the concomitants of true kidney disease assisted by the effects of the puerperal condition. While in the albuminuria of pregnancy, unless the symptoms be severe, the case may almost be left to nature, in pregnancy as-

sociated with true kidney disease energetic treatment is demanded, even to the induction of premature labour. Pregnancy in many cases ultimately induces true kidney disease by keeping up renal congestion. (Dr. G. Harley, p. 106.)

RANULA.—*Injection of Iodine.*—Inject fifteen minims of compound tincture of iodine into the interior of the tumour. (Mr. W. M. Coates, p. 222.)

RELAXATION OF THE PELVIC ARTICULATIONS.—At the close of pregnancy and after labour the pelvic articulations are sometimes so relaxed as to prevent the patient taking walking exercise without considerable pain in the back and pelvis. The gait produced is peculiar, being a species of waddling, each leg being advanced with difficulty. There is always great accompanying debility. This state is but an exaggeration of a physiological fact, the object of which is to facilitate the passage of the fœtus A laced jean belt must be constructed, to support the pelvis and hips. Before, however, this is done, it is well to try the effect of a circular bandage tightly applied round the hips. If the patient is able to walk with ease, the belt may be made for more permanent wear. (M. Trousseau, p. 224.)

SPINA BIFIDA.—*Injection of Iodine.*—In a case in which the opening into the spinal column was about half-an-inch long and a quarter wide, the fluid was withdrawn by means of a subcutaneous injection syringe, the needle of which was passed first through healthy parts before reaching the tumour; a solution of iodine ten grains, iodine of potassium twenty grains, and water an ounce, was then injected, and the tumour painted over at once, and daily afterwards, with contractile collodion. A cure resulted. (Mr. W. M. Coates, p. 223)

UTERINE DILATOR.—A new uterine dilator, intended to act more on the internal os and upper part of the canal than any instrument now in use, will be found described and figured at page 202. (Mr. J. W. Hembrough.)

UTERINE LEUCORRHŒA.—*Injection of Tincture of Iodine.*—Having by the introduction of a short-haired brush removed the layer of mucus adhering to the interior of the cervix and uterine cavity, inject by means of a gum elastic catheter a lotion consisting of one part of the compound tincture of iodine to two parts of water. (Dr. Murray. p. 211.)

VAGINAL RECTOCELE.—Vaginal rectocele is protrusion of the dilated rectum and posterior wall of the vagina through the relaxed vaginal outlet. There is no displacement of the entire rectum, the anterior part only is involved. It is remediable by a plastic operation, whereby the relaxed state of the vagina is remedied. The operation necessary is essentially similar to the one for ruptured perineum. (Mr. J. R. Lane, p. 203.)

MISCELLANEA.

ACIDS.—The rule is sulphuric acid to astringe, hydrochloric acid to promote digestion, and nitric acid to promote secretion. (Dr. Bence Jones, *Lancet. Dec.* 9, 1865, *p.* 641.)

ANÆSTHESIA.—*Anæsthesia by Mixed Vapours.*—Chloroform is the wrong thing to begin with; it is a depressant to the heart. We ought to commence the anæsthetic operation with a gentle cardiac stimulant. Let the patient at first inhale a mixture of ether and alcohol vapour, and when the heart and nervous system have fully experienced the power of this excitant, let chloroform vapour be gradually inhaled. This may be readily done by an inhaler specially contrived by Mr. Robert Ellis, of the Chelsea and Belgrave dispensary, for the purpose, described and figured at page 389. By it the relative proportion of these vapours can be regulated with the utmost nicety. Pure absolute ether and absolute alcohol must be used, perfectly free from the abominable contamination of methyl and its compounds. (Mr. R. Ellis p. 265.)

Chloroform.—In cases of threatened mischief during the administration of chloroform, nothing does more good than pulling forward the tongue, where-

by the epiglottis is likewise pulled forward and air admitted into the larynx. Chloroform should never be given to a patient in the sitting posture, or fatal fainting may ensue. (Prof. Spence, Dr. Gillespie, *Edinburgh Medical Journal, March.* 1866, *p.* 857.)

Ether and Chloroform.—If, in any case, chloroform seems to produce a depressing effect, it is better at once to substitute ether for it; and in prolonged operations, when anæsthesia has been fairly produced by chloroform, it is a good plan to keep it up by ether. The substitution is never attended with any return of sensation. There seems to be a natural antagonism between these agents. (Dr. Sanson, *Lancet, March* 17, 1866, *p.* 288.)

Local Anæsthesia.—Dr. Richardson's Ether-Spray Method.—If ether be forced through a jet by mechanical force, along with a certain amount of air, such intense cold is produced as readily to freeze water exposed to it in a test-tube. The apparatus used (figured at page 258) consists of a graduated bottle for holding ether, through the perforated cork of which a double tube is inserted, one extremity of the inner part of which reaches to the bottom of the bottle. The air is forced into the bottle by means of a compressible india-rubber bellows, and forces the ether through the inner tube, and in fine spray from the terminal jet; a current of air is also forced through the outer tube and terminal jet, which is of course a little larger than, and surrounding the inner, or ether jet. When the spray thus produced is directed upon the skin, insensibility is produced within a minute, and after the division of the skin the ether begins to exert on the nervous filaments the double action of cold and of etherisation, so that the narcotism can be extended deeply to any desired extent. The spray directed upon a half-inch test-tube containing water will produce a column of ice in two minutes at most. The spray may even be directed into the bladder, or into the uterus. It may readily be directed upon the gum previous to the extraction of a tooth, rendering the operation quite painless. It is likely to be useful also in a great number of minor operations, such as tying nævus, tying piles, incising carbuncles, opening abscesses, and removing small tumours. In the extreme rapidity of the action of this deadening process lies its safety, suspending life without causing destruction of vitality. Many other fluids have been used instead of absolute ether, of specific gravity 0·720, such as methylic ether, amylene, and kerosolene, but none answer so well; kerosolene is a very impure and unpleasant liquid. The peculiar hardness of the skin which occurs when the freezing mixture of ice and salt is applied does not occur under the use of the ether spray. Tests for the purity of ether will be found at p. 260. (Dr. B. W. Richardson, p. 256.)

According to the nature of the case the surgeon may either produce a superficial anæsthesia or entire blanching of the part: to produce the latter absolute ether must be used, and the spray directed in a brisk current at a distance of about an inch from the part; to induce the former, or superficial anæsthesia only, a mixture of ether and chloroform, in the proportions seven to one, or six to two, may be used. For opening an abscess, incising a small carbuncle, tying a nævus, or removing very small tumours, for applying nitric acid, and for operations of a similar kind, the mixture of ether and chloroform, answers every requirement. In an operation for hernia it would also be better to use the mixture, because the tissues would not be rendered hard, and the dissections could be carried on with delicacy. For teeth-extraction the pure ether answers best, it acts rapidly and deeply, and there is no great accumulation of fluid in the mouth. By practice, the two degrees of action may be obtained by ether alone, by regulating the distance from which the spray is directed. By removing the jet three inches from the part a moderate effect is produced. (Dr. B. W. Richardson, p. 263.)

Rhigolene.—It will probably be found that rhigolene, prepared from naphtha, will be the best means of freezing parts by Dr. Richardson's spray method. Common ether will boil at about 96°, while rhigolene boils at 70°. It will be both more effectual and much cheaper than ether. (Dr. H. J. Bigelow, Massachusetts, p. 265.)

Local Anæsthesia.—Certain substances such as atropine, strychnine, mor-

phine, and aconite, when mixed with chloroform and applied to the skin are absorbed very rapidly, while if alcohol is used absorption is very slow. If this by further experience proves correct, we may produce local anæsthesia without subcutaneous injection. (Dr. Brown-Sequard, p. 252.)

HASSALL'S FLOUR OF MEAT.—This food consists, in the concentrated form and with the exception of the water, of the whole of the constituents of the meat, previously freed from bone and visible fat. It is in a very fine powder, finer indeed than flour. A spoonful of it added to ordinary beef tea or soup greatly enhances its nutritive qualities. When taken as a sandwich, or sprinkled over or mixed up with vegetables it is often retained by the stomach when solid meat could not be taken. It is most serviceable in cases of dyspepsia. (Dr. A. H. Hassall, p. 235.)

MILK.—*To Prevent its turning Sour.*—Add to each gallon of milk a teaspoonful of a saturated solution of sulphite of soda. Even in hot weather the milk will remain sweet for a week or ten days, and the sulphite added is quite imperceptible. (Dr. J. Scoffern, p. 237.)

POISONING BY PRUSSIC ACID.—Give the patient one or two drachms of magnesia made into an emulsion with water, and then give, in water, a solution of sixteen minims of perchloride of iron and twelve grains and a half of green vitriol. A harmless Prussian blue results. (Messrs. T. and H. Smith, p. 253.)

PREPARATIONS OF IRON.—Preparations of iron promote the process of oxidation in the body, changing the oxygen taken into the blood by respiration into ozone. They therefore favour the nutrition and activity of the body, and are contraindicated when the process of oxidation is already too energetic, as in inflammatory diseases, or when an increased flow of blood is apprehended. (Dr. A. Sasse, p. 251.)

PURIFICATION OF WATER.—Black oxide of iron possesses a remarkable power of separating the organic impurities from water. The best way of preparing it for this purpose is by heating together hæmatite, or red oxide of iron ore, with sawdust. The oxide as made by this process contains a small percentage of carbon which, however, renders the substance harder and less brittle. If river water containing much organic impurity be made to percolate a layer of this substance a few inches thick, it will lose almost all organic impurity, and be perfectly pure both to taste and smell. It is an admirable substance to use in filters, and, moreover, does not lose its power in the least by time. (Mr. Thomas Spencer, *Med. Times and Gazette, April* 28, 1866, *p.* 447.)

THERMOMETER.—*Uses of, in Diagnosis.*—The thermometer is now in constant use as an aid in the diagnosis of certain cases. A set of two, specially graduated for the use of medical men, can be obtained. A persistent rise of the temperature of the body above 99·5° or depression below 97·3° is a sure sign of disease. The temperature is lowered after a full meal and the use of alcohol; it rises as digestion advances. An increase of temperature of one degree above 98° generally corresponds with an increase of ten beats of the pulse per minute. There are however many exceptions to this rule. The temperature is the only certain means of diagnosis between the frequency of pulse produced by weakness and that caused by fever, and by it we diagnose phthisis long before any physical signs can be detected, and can distinguish it from diseases which closely resemble it, as carcinoma of the lung, aortic aneurism, and dilated bronchi. We may also diagnose tubercular meningitis from cerebral tumour. (Dr. F. W. Gibson, p. 30.)

SICK ROOMS.—*Disinfection of.*—For a sick room charged with an organic poison, all disinfectants that may be used should belong to the class that assist oxidation. Ventilation and a regular temperature (60° to 63°) increase their action. The three agents suitable are chlorine, ozone, and iodine. Chlorine should not be exposed freely to the air, but the solution should be contained in small stoppered bottles which may be emptied into the room at regular intervals at as great a distance as possible from the patient. When a slow and continuous process of deodorising is required iodine maintains an excellent effect, and if required for more active service it may be evaporated

from a heated plate. Ozone, however, is extremely easy of production by means of phosphorus half covered with water, and maintains the air of a sick chamber perfectly sweet. (Dr. T. H. Barker, *British Med. Journal, Jan.* 6, 1866, *p.* 13.)

SINAPINE TISSUE PAPER.—Sinapine tissue paper is a thin glazed paper impregnated with something of a very pungent character, which, when wetted with cold water and applied to the skin, produces in about ten minutes a good deal of superficial irritation. Its effects are more persistent than those of an ordinary mustard poultice, and it is much more cleanly. (Mr. Cooper, p. 252.)

SULPHITES AND HYPOSULPHITES.—*Administration of.*—The salts of sulphurous and hyposulphurous acids with potass, soda, and magnesia, are those most suitable for medicinal use. They are not changed when taken into the stomach, but pass unchanged into the blood and appear in the urine, saliva, and sweat. Hence their use in arresting putrefactive zymotic changes in the body. Their beneficial action is checked by any of the vegetable acids; hence food or drinks containing them must be avoided at the same time. The sulphites are better than the hyposulphites when a rapid curative action is required, but the latter may be advantageously substituted for the former when prophylaxis is aimed at. Sulphite of magnesia has the least taste and is the most active, but being less soluble than the others should be administered in powder. (Dr. Polli, p. 232.)

WHEAT PHOSPHATES.—There is something essentially special in the organised phosphates, those, in fact, which have been found by passing through a living organism as compared with those artificially prepared. It is not the amount, but the kind exhibited which produces the good result. In infants' food, and in our bread and flour, the organised phosphates and cerealin (which has a somewhat similar action to pepsin) have been deliberately rejected. These may be administered medicinally to children and infants where the assimilative function is at fault. In eruptive diseases of the scalp (which are generally associated with faulty assimilation), in rickets, marasmus, chronic diarrhœa, and impaired nutrition of all kinds, the wheat phosphates act marvellously. Pallid children pick up tone, colour, and flesh, worms disappear, the secretions become healthy, and disease goes. They are prepared by carefully evaporating in a water bath a decoction of well-selected bran, mixing the residue with sugar, and reducing to powder. (Dr. Tilbury Fox, p. 101.)

INDEX TO VOL. LIII.

PAGE

Absorbing power for medicines of certain parts of the body . . . 252
Acetic acid, its use in cancer 231
Acids, Dr. Bence Jones on the action of 287
Actual cautery, Mr. Baker Brown on securing the pedicle by, in ovariotomy . 211
" cautery, Mr. Douglas on its use for nævi 146
Acupressure, Mr. Macgregor on the value of 44
Acute orchitis, Dr. Watson on the treatment of 145
" rheumatism, Mr. Macgregor on the efficacy of blistering in . . 44
" rheumatism, Mr. Moore on the efficacy of blistering in . . . 45
Adams, Mr. W., on subacute synovitis and contracted joints . . . 122
Albuminuria from imperfect digestion, Dr. Harley on 106
" from reflex irritation, Dr. Harley on 107
" of pregnancy, Dr. Harley on 107
Alcohol, Dr. Bence Jones on the course of a dose of, through the system . 255
" Dr. Munroe on its action on digestion 254
Alcoholic stimulants in typhus fever, Dr. Perry on the use of . . . 87
Alkaline urine, Dr. Bence Jones on the symptoms arising from . . 102
Althaus, Dr. J., on progressive locomotor ataxy 50
Ammoniacal urine, Dr. Bence Jones on the treatment of . . . 102
Anchylosis of the knee in the straight position, Dr. Smyly on forcible flexion in 121
Anderson, Dr. K., on the daily excretion of urea in typhus fever . . 38
Anæsthesia by mixed vapours, Mr. R. Ellis on 267
" local, Dr. Bigelow on rhigolene for inducing . . . 265
" local, Dr. Richardson's ether-spray method of inducing . . 256
Aneurism, Dr. Duncan on the galvano-puncture of 133
" Prof. Syme on ligature of the femoral artery . . . 139
" popliteal, Mr. Hilton on the treatment of 140
Apoplexy, Dr. Headland's treatment of the precursory symptoms of . . 250
Ataxy, progressive locomotor, Dr. Althaus on a case of . . . 51
" progressive locomotor, Dr. Jackson's case of 55
" progressive locomotor, Dr. Johnson's case of 54
Atropia, Dr. Fleming on its use in constipation 88
Atropine points, Mr. Laurence on 154
Aveling, Dr. J. H., his case of ovariotomy 207
Barclay, Dr. J., on citric, acetic, and carbolic acids in cancer . . . 230
Barnes, Dr. R., on dysmenorrhœa and its treatment 212
Beck, Dr. S., on enlargements of the uterus following abortions, &c. . 179
" on puerperal fever 173
Bed-guard and leg-suspender, Mr. Greenway's 128
Beigel, Dr. H., on the treatment of pruritus senilis 148
Belladonna and bromide of potassium in epilepsy 64
Bigelow, Dr. H. J., on rhigolene for procuring local anæsthesia . . 265
Bile in urine, M. Cunisset on chloroform as a means of detecting . . 253
Bishop, Dr. E., his instrument for applying remedial agents to cavity of tympanum 157
Black cataract, Mr. Walton on the extraction of 152

PAGE
Black, Dr. D. C., on an outbreak of typhoid fever 41
Blistering in acute rheumatism 45
Blue glasses, Prof. Bohm's preference for 155
Bowels, obstruction of, Dr. Fleming's case successfully treated by stomach pump 248
Broadbent, Dr., on use of belladonna and bromide of potassium in epilepsy . 64
Bright's disease, Dr. Harley on the treatment of 110
Bromide of ammonium in the sleeplessness of functional nervous diseases . 64
" of potassium in epilepsy 64
Bronchitis, chronic, of the aged, treatment of 250
Brown-Sequard, Dr. C. E., on functional nervous affections 60
" on absorbing power for medicines of certain parts of the body 252
" on the moral treatment of functional nervous diseases. . . 63
" on subcutaneous injection of morphia and atropine . . 252
" on the treatment of chlorosis 221
" on the treatment of epilepsy 66
" treatment of sleeplessness in nervous diseases. . . 64
" on the use of chloroform in epilepsy and hysteria . . 68
Brown, Mr. I. B., on the actual cautery for securing the pedicle in ovariotomy . 211
Brunton, Dr. J., his new otoscope, or speculum auris 156
Buchanan, Dr. G., on tracheotomy in croup and diphtheria . . . 94
Calabar bean, Dr. Ogle on its use in chorea 71
Cancer, Dr. Barclay on citric, acetic, and carbolic acids in . . . 230
" Mr. De Morgan on, and its treatment 112
" Mr. Denny on citric acid as an application in 45
" of the breast, Mr. Moore's case of 114
Cancerous tumour of the arm, Mr. Hilton's case of removal of . . . 46
Carbolic acid, Dr. Wolfe on its use in phthisis 87
" acid, its efficacy in the treatment of cancer 231
Cataract, black, Mr. Walton's case of extraction of 152
Caustic applications, Dr. Routh on the treatment of prolapsus uteri by . . 188
Chambers, Dr. T. K., on the treatment of typhoid fever 43
Chapman, Dr. J., on the use of ice-bags in convulsions and in cholera . . 69
Chilblains, Dr. Purdon on the nature and treatment of 147
Childbed, Dr. Matthews Duncan on the mortality of 226
Chlorate of potass in ovarian disease, Mr. Craig on 209
Chloride of zinc in surgical operations and injuries, Mr. De Morgan on . . 112
" of zinc lotion in the treatment of cancer, Mr. De Morgan on . . 115
Chlorocarbon, Dr. Sansom on its use in midwifery 284
Chloroform and ether, Dr. Sansom on 288
" Drs. Spence and Gillespie on prevention of danger from . . 287
" its use in epilepsy and hysteria 68
Chlorosis, Dr. Brown-Sequard on the treatment of 221
" with suppressed menses, Dr. Headland's treatment of . . 251
Cholera, Dr. Bowerbank on the saline treatment of 277
" Dr. Chapman on the use of ice-bags in 69
" Dr. Gason on the treatment of 277
Choleraic diarrhœa, Dr. Johnson on the treatment of 277
Chorea, Dr. Headland on the use of iron in 251
" Dr. Ogle's treatment of, by the Calabar bean 71
Chronic bronchitis of the aged, Dr. Headland's treatment of . . . 250
Citric acid as an application in cancer 45
" its use in cancer 230
Coates, Mr. W. M., his case of glandular tumour of mamma with cysts. . 221
" his case of large ranula treated by injection of iodine . 222
" his case of spina bifida treated by injection of iodide of potassium 223
Colles's fracture, on its treatment by Dr. Gordon's splint . . . 124
Congestion, Dr. Merrill on the internal use of chloroform in . . . 275
Constipation, Dr. Fleming on the use of atropia and galvanism in . . 88
Contracted joints, Mr. Adams on the treatment of 122
Convalescence from inflammation, use of nitric acid in 99
Convulsions, on the use of ice-bags in 69
Cotton, Dr. R. P., on hemoptysis unassociated with pulmonary tuberculosis . 85
Craig, Mr. W., on chlorate of potass in ovarian disease 209
Croup, Dr. Buchanan on tracheotomy in 94

PAGE
Cuvisset, M., on chloroform for detecting bile in urine 268
De Morgan, Mr. C., on chloride of zinc in surgical operations and injuries . 112
Denny, Mr. C. J., on citric acid as an application in cancer . . . 45
Detached placenta, Mr. Tubbs's case of hemorrhage from . . . 181
Diabetes insipidus, treated by large doses of valerian 48
Difficult turning, Dr. Heyderdahl on the use of wire-ribbon in . . . 174
Digestion, Dr. Munroe on the action of alcohol on 254
Dilatation of the uterus, Mr. Hembrough's new instrument for . . 202
Diphtheria, Dr. Buchanan on tracheotomy in 94
" Mr. Maynard on the use of hyposulphite of soda in . . 93
" complication with enteric fever 42
Disease, Dr. Tilbury Fox on the action of fungi in the production of . 239
Dislocation of the shoulder-joint, Mr. Porter's easy method of reducing . 123
Displacements of the uterus, Dr. Main's support for 195
" of the uterus, Dr. Wright on 198
Dobell, Dr. H., on the nature, cause, and treatment of tuberculosis . . 78
Douglas, Mr. J. R. A., on the actual cautery for nævi 146
Drowning, resuscitation after, Mr. Ellis's case of 238
Duncan, Dr. J. M., on mortality of childbed as affected by number of the labour 226
Duncan, Dr. J., on the galvano-puncture of aneurism 133
Dysmenorrhœa, Dr. Barnes on the treatment of 212
" Dr. Greenhalgh on the operation for 216
" Dr. Routh on the operation for 219
" Dr. Savage on 219
" Mr. Baker Brown on the operation for . . . 218
Dyspepsia associated with chronic affections, M. Trousseau on hydrochloric acid in 100
Eczema, acute, Mr. Johnston on the treatment of 149
" infantum, Mr. Wilson's treatment of 148
Ellis, Mr. R., on anæsthesia by mixed vapours 267
" on the radical cure of prolapsus uteri 185
Endemic or miasmatic fevers, use of the sulphites and hyposulphites in . 234
Enlargements of the uterus following abortions and confinements, Dr. Beck on 179
Epilepsy, Dr. Broadbent on use of belladonna and bromide of potassium in . 64
" Dr. Brown-Sequard on the treatment of 66
" Dr. Brown-Sequard on the use of chloroform in . . . 66
" Dr. Headland on the use of turpentine in 250
Erysipelas, Mr. Hilton on the treatment of 46
Ether, Dr. Richardson's tests for purity of 260
" spray apparatus for inducing local anæsthesia, Dr. Richardson's . 258
" " xylo-styptic, Dr. Richardson on 264
Femoral artery, Prof. Syme on ligature of the 139
Fergusson, Sir W., on recurring fibroid disease of the shoulder . . 131
Fever, Dr. Bence Jones on 17
" Editor of Lancet on the value of the thermometer in . . . 21
" enteric, complication of diphtheria with 42
" infantile, Mr. Smith's thermometrical observations in . . . 23
" mechanical disorders that arise out of the chemical errors in . 19
" puerperal, Dr. Snow Beck on 173
" typhoid, Dr. Black on an outbreak of 41
" typhoid, Dr. Chambers on the treatment of 43
" typhus, Dr. Anderson on the excretion of urea in 38
" typhus, Dr. Perry on the treatment of 36
" typhus, Mr. Randolph on 39
Fibroid disease of the shoulder, Sir W. Fergusson on recurring . . 131
Fleming, Dr. A., on the treatment of constipation and stoppage of the bowels 88
Fleming, Dr. E., his case of obstruction of the bowels 248
Flexions of the uterus, Dr. Wright on extreme 198
Flour of meat, Dr. Hassall's 236
Fœtus retained in abdomen 43 years, Mr. Watkins's case of . . . 178
Food, Liebig's, for infants and invalids 228
Fox, Dr. T., on the action of fungi in the production of disease . . 239
" on wheat phosphates 101
Fracture apparatus, Mr. Greenway's 129
" of the humerus, Mr. Hutchinson on 126
" of the radius at styloid process, Mr. Tait on use of Gordon's splint in 128

	PAGE
Functional nervous affections, Dr. Brown-Sequard on	60
" nervous diseases, Dr. Brown-Sequard on the moral treatment of	63
" nervous diseases, use of bromide of ammonium in the sleeplessness of	64
Fundus oculi, Dr. Roseburgh's method of examining	149
Fungi, Dr. Tilbury Fox on its action in the production of disease	239
Galvano-puncture of aneurism, Dr. Duncan on the	133
Gibson, Dr. F. W., on the thermometer in the diagnosis of pyrexial diseases	30
Glandular tumour of mamma with cysts, Mr. Coates on injection of iodine in	221
Gonorrhœa and gleet, Mr. Henry Thompson on soluble bougies in	282
Goolden, Dr. R. H., on the treatment of disease by oxygen	48
Granular ophthalmia, Dr. Stokes on the treatment of	149
Greenway, Mr. H., his new form of leg-suspender and bed-guard	128
Harley, Dr. G., on secondary albuminuria	106
" on the treatment of Bright's disease	110
Hart, Mr. R., on the treatment of gonorrhœal and purulent ophthalmia	151
Hassall, Dr. A. H., on the concentration and preservation of meat	236
Hassall's flour of meat	235
Headland, Dr., his therapeutical notes on various subjects	250
Hembrough, Mr. J. W., his new uterine dilator	202
Hemoptysis unassociated with pulmonary tuberculosis, Dr. Cotton on	85
Hemorrhage from detached placenta, Mr. Tubbs's case of	181
Hepatic disease in India, Sir Ranald Martin's treatment of	97
Heyderdahl, Dr. V., on the use of wire-ribbon in cares of difficult turning	176
Hilton, Mr., his case of wounded ulnar artery, sloughing and secondary hemorrhage	142
" on removal of a cancerous tumour of the arm	46
" on the treatmeat of popliteal aneurism	140
Holthouse, Mr. C., on the treatment of strabismus without operation	153
Humerus, Mr. Hutchinson on some injuries to the head and neck of	126
Hutchinson, Mr. J., on some injuries to the head and neck of the humerus	126
Hyposulphite of soda in diphtheria, Mr. Maynard on the use of	93
Hysteralgia, Sir J. Y. Simpson on chlorocarbon vapour in	285
Hysteria, Dr. Brown-Sequard on the use of chloroform in	68
Ice-bags, Dr. Chapman on their use in convulsions and in cholera	69
Infants' food, Dr. Fox on the desirable properties of wheat phosphates for	101
Inhaler for producing anæsthesia by mixed vapours, Mr. Ellis's	269
Interstitial nephritis, Dr. Bence Jones on	104
Intestinal obstruction, Dr. Fleming on the use of atropia in	88
Iodide of potassium, case of spina bifida cured by injection of	223
Iodine, case of glandular mammary tumour treated by injection of	221
" case of large ranula cured by injection of	222
Iritis, Mr. Teale, jr., on the use of atropine in	283
Iron, Dr. Sasse on the physiological operation of preparation of	251
Jackson, Dr. H., his case of progressive locomotor ataxy	55
Johnson, Dr. G., his case of progressive locomotor ataxy	54
Jones, Dr. C. H., on spasm	56
Jones, Dr. H. B., on interstitial nephritis	104
" on fevers	17
" on the course of a dose of alcohol through the system	255
" on the treatment of ammoniacal urine	102
" on tubular nephritis	105
" on the symptoms arising from alkaline urine	102
Knee-joint, Dr. Smyly on forcible flexion in anchylosis in the straight position	121
" Mr. Adams on the treatment of effusion into the	122
Labordette, Dr., his new bivalve speculum for the larynx	81
Lane, Mr. J. R., on vaginal rectocele	203
" on syphilization	171
Larynx, Dr. Labordette's new bivalve speculum for	81
Laurance, Mr. J. R., on atropine points	154
Leather-felt splints, Mr. Hide's	130
Lepra, Dr. Headland's treatment of	251
Lee, Mr. H., his new instrument for lithotomy	145
" on syphilitic inoculation in 1865	160
" on the treatment of syphilis	165
Leg-suspender and bed-guard, Mr. Greenway's	128

PAGE
Leucorrhœa, Dr. Murray on iodine in the treatment of 211
Liebig's food for infants and invalids 228
Ligature of the femoral artery, Prof. Syme on 139
Lithotomy, Mr. Lee's new instrument for 145
Lithotrity, Mr. Henry Thompson on 143
Liver, Sir R. Martin's treatment of diseases of, in India . . . 97
Local anæsthesia, Dr. Bigelow on the use of rhigolene for producing . . 265
" anæsthesia, Dr. Richardson's ready method of producing . . 256
" pains of the walls of the chest, Sir J. Y. Simpson on chlorocarbon for . 281
Macgregor, Mr. D., on acute rheumatism, and acupressure . . . 44
Main, Dr. W., on a new uterine support 195
Malignant and recurrent tumours of the orbit, treatment of . . . 155
Mammary tumours with cysts, Mr. Coates on injection of iodine in . . 221
Martin, Sir R., on hepatic disease amongst Europeans in India . . 97
Maynard, Mr. J. C., on hyposulphite of soda in diphtheria . . . 93
Meat, Dr. Hassall on the concentration and preservation of . . . 236
" Dr. Hassall's flour of 235
Medicines, Dr. Brown-Sequard on the administration of 253
Membrana tympani, Dr. Weber on perforation of the 158
Milk, Dr. Scoffern on prevention of sourness in 237
"Missed labour," Mr. Watkins on a remarkable case of . . . 178
Morphia, Dr. Ogle on the subcutaneous injection of, in neuralgia . . 72
" and atropine, Dr. Brown-Sequard on the subcutaneous injection of . 252
Mortality of childbed, Dr. Matthews Duncan on the 226
Munroe, Dr. H., on the action of alcohol on digestion 254
Murray, Dr., on iodine in the treatment of leucorrhœa 211
Mustard paper, Mr. Cooper's sinapine tissue or 252
Nævi, Mr. Douglas on the use of the actual cautery for 146
Narcotics, subcutaneous injection of, in neuralgia 72
Nephritis, interstitial, Dr. Bence Jones on 104
" tubular, Dr. Bence Jones on 105
Nervous affections, functional, Dr. Brown-Sequard on 60
" affections, functional, on the moral treatment of . . . 63
" affections, functional, on elimination of certain poisons in . . 221
" affections, functional, use of bromide of ammonia in sleeplessness of 64
Neuralgia, Dr. Ogle on the subcutaneous injection of narcotics for . . 71
" Dr. Richardson on its relief by ether-spray 260
Nitro-muriatic bath in hepatic disease, directions for 98
Obstructions of the bowels successfully treated by stomach-pump, Dr. Fleming on 248
Ogle, Dr. J. W., his treatment of chorea by the Calabar bean . . . 71
" on the subcutaneous injection of narcotics for neuralgia . 72
Orbit, treatment of malignant and recurrent tumours of the . . . 155
Orchitis, acute, Dr. Watson on the treatment of 145
Ophthalmia, granular, Dr. Stokes on its treatment by pressure . . . 149
" gonorrhœal, Mr. Hart on the treatment of 151
" purulent, Mr. Hart on the treatment of 151
Otoscope, or speculum auris, Dr. Brunton's new 156
Ovarian disease, Mr. Craig on chlorate of potass in 209
Ovariotomy, Dr. Aveling's case of 207
" Dr. Aveling's new coil-clamp for 208
" Mr. Baker Brown on the actual cautery in 211
Oxygen, Dr. Goolden on the treatment of disease by 48
Paralytic strabismus treated without operation 154
Pelvic articulations, M. Trousseau on loosening of the 224
Perforation of the membrane tympani, Dr. Weber on 158
Perineum, Mr. Lane's operation for suture of the 204
Perry, Dr. R., his observations of the present epidemic of typhus . . 34
Pessaries, uterine, Dr. Main's 195
" uterine, Dr. Wright's 201
Phagedenic ulceration, on inhalation of oxygen in a case of . . . 49
Phthisis, Dr. Wolfe on the use of carbolic acid in 87
Poisoning by prussic acid, Messrs. T. and H. Smith's antidote for . . 258
Polli, Dr. G., on the sulphites and hyposulphites in zymotic diseases . 232
Polydipsia, Dr. Reith's treatment by large doses of valerian . . . 48
Polypus uteri, Dr. Swayne's cases of 192

PAGE
Popliteal aneurism, Mr. Hilton on the treatment of 140
Porter, Mr. W. E., his method of reducing dislocations of the shoulder-joint . 123
Pressure, instrument for applying, in granular ophthalmia . . . 150
Procidentia uteri, Dr. Barnes on 184
" uteri, Dr. Marion Sims on the operation for 182
" uteri, Mr. Chambers on 184
Progressive locomotor ataxy, Dr. Althaus on 50
" locomotor ataxy, Dr. Jackson's case of 55
" locomotor ataxy, Dr. Johnson's case of 54
Prolapsus uteri, Dr. Routh on the radical cure by caustic applications . . 188
" uteri, Mr. Ellis on the radical cure of 185
Pruritus senilis, Dr. Beigel on the treatment of 148
Psoriasis, Dr. Headland's treatment of 251
Puerperal fever, Dr. Barnes on 175
" fever, Dr. Grailly Hewitt on 174
" fever, Dr. Snow Beck on 173
Pulse and temperature, correlation of 30
" indicator, M. Marey's sphygmograph, or 249
Purdon, Dr. H. S., on the nature and treatment of chilblains . . . 147
Pyæmia and septicæmia, use of the sulphites and hyposulphites in . . 233
Pyrexial diseases, Dr. Gibson on the use of the thermometer in the diagnosis of 30
Radius, fracture of, at the styloid process, use of Gordon's splint for . . 123
Randolph, Mr. H. W., on typhus fever 39
Ranula, Mr. Coates's case treated by injection of iodine 222
Rectocele, vaginal, Mr. Lane on 203
Reith, Dr., his case of polydipsia treated by valerian 48
Resuscitation after drowning by Drs. Silvester and Marshall Hall's methods combined 238
Rheumatism, acute, Mr. Macgregor on its treatment by blistering . . 44
" acute, Mr. Moore on its treatment by blistering 45
Rhigolene, a petroleum naphtha for producing local anæsthesia, Dr. Bigelow on 265
Richardson, Dr. B. W., on a new and ready mode of producing local anæsthesia 256
Roseburgh, Dr., his mode of examining the fundus oculi 149
Routh, Dr. C. H. F., on the radical cure of prolapsus uteri, by caustic applications 188
Sasse, Dr. A., on the physiological operation of the preparations of iron . 251
Scoffern, Dr. J., on the preservation of milk 237
Shoulder, Sir W. Fergusson on recurring fibroid disease of the . . 131
" joint, Mr. Porter's method of reducing ordinary dislocations of . 123
Sick-rooms, Dr. Barker on the disinfection of 289
Sims, Dr. J. M., on procidentia uteri 182
Sinapine tissue, or mustard paper, Mr. Cooper's 252
Sleeplessness of functional nervous diseases, use of bromide of ammonium in 64
Smith, Mr. G. S., thermometrical observations on the fevers of children . 23
Smyly, Dr. P. H., on forcible flexion of the knee for anchylosis in straight position 121
Spasm, Dr. C. H. Jones on 56
Spectacles, blue, preference of, at Berlin 155
Speculum auris, Dr. Brunton's new otoscope or 156
" for examining the larynx, Dr. Labordette's 81
Sphygmograph, M. Marey's, Editor of Lancet on 249
Spina bifida, Mr. Coates's case treated by injection of iodide of potassium . 223
Splints, gutta-percha, Mr. Truman's method of making 131
" leather-felt, Mr. Hidesis 130
Sterility, Dr. Barnes on the causes of 213
Stokes, Dr. W., jun., on the treatment of granular ophthalmia . . . 149
Stomach disorders, use of nitric and muriatic acids in certain forms of . 100
Strabismus, Mr. Holthouse on its treatment without operation . . 153
Sulphites and hyposulphites, Dr. Polli on the uses of, in zymotic diseases . 232
Swayne, Dr. J. G., his cases of polypus uteri, with remarks . . . 192
Syme, Prof., on ligature of the femoral artery 139
Syphilis, Dr. Lee on its treatment by the calomel vapour bath . . 169
Syphilitic inoculation in 1865, Mr. Lee on 160
Syphilization, Mr. Lee on 165
" Mr. Lane on 171
Synovitis, subacute, and contracted joints, Mr. Adams on 122
System, Dr. Bence Jones on the action of alcohol in the . . . 255

PAGE
Tabes dorsalis, or progressive locomotor ataxy, Dr. Althaus on . . 50
Tænia, Dr. Headland's treatment by turpentine and castor oil combined . 250
Tait, Mr. L., on the treatment of fracture of radius by Gordon's splint . 123
Temperature, fluctuations of, within the limits of health . . . 30
" of the body in disease 33
" range of, in disease 31
Thermometer, Dr. Gibson on its use in diagnosis of pyrexial diseases . 30
" a means of diagnosis between tubercular and non-tubercular diseases 82
Thermometer, in specific fever, Editor of Lancet on the . . . 21
Thermometrical observations on the fevers of children, Mr. Smith on . 23
Thompson, Mr. H., on lithotrity 143
Tracheotomy in croup and diphtheria, Dr. Buchanan on . . . 94
Trousseau, M., on loosening of the pelvic articulations . . . 224
Truman, Mr. E., how to make gutta-percha splints . . . 131
Tubbs, Mr. W. J., his case of hemorrhage from detached placenta . . 181
Tubercular and non-tubercular diseases, thermometer as a means of diagnosis 82
Tuberculosis, Dr. Dobell on the nature, cause, and treatment of . . 73
" on a certain form of hemoptysis not associated with . . 85
Tubular nephritis, Dr. Bence Jones on 105
Tumour, mammary, Mr. Coates on injection of iodine in . . . 221
Tumours of the orbit, treatment of malignant and recurrent . . . 155
Turning, Dr. Heyderdahl on the use of wire ribbon in difficult cases . 176
Tympanic membrane, Dr. Weber on perforation of the . . . 158
Tympanum, Dr. Bishop's instrument for applying remedial agents to cavity of 157
Typhoid fever, Dr. Black on an outbreak of 41
" fever, Dr. Chambers on the treatment of 43
Typhus fever, Dr. Anderson on the daily excretion of urea in . . 38
" fever, Dr. Perry's observations on the present epidemic of . . 34
" fever, Mr. Randolph on 39
" fever, daily record of the temperature, &c., in cases of . . 25
Urea, researches on the daily excretion of, in typhus fever . . . 38
Urine, M. Cunisset on chloroform for detecting bile in . . . 253
" alkaline, Dr. Bence Jones on 102
" ammoniacal, Dr. Bence on 102
Uterine dilator, Mr. Hembrough's 202
" leucorrhœa, Dr. Murray on the use of iodine in . . . 211
" pessaries, Dr. Main's 195
" support, Dr. Wright's 201
" polypi, Dr. Swayne on the operation for 194
Uterus, Dr. Wright on extreme flexions of the 198
" enlargement of, following abortions and confinements, Dr. Beck on . 179
Vaginal rectocele. Mr. Lane on 203
Valerian, its use in the treatment of diabetes insipidus . . . 48
Walton, Mr. H., on the extraction of black cataract 152
Water, Mr. Spencer on the purification of 289
Watkins, Mr. R. W., his remarkable case of "missed labour" . . 178
Watson, Dr. W. S., on the treatment of acute orchitis . . . 145
Weber, Dr. F. E., on difficulty of hearing in perforation of the membrana tympani 158
Wheat phosphates, Dr. Fox on 101
Wolfe, Dr. J. R., on the use of phenic (carbolic) acid in phthisis . . 87
Wounded ulnar artery, sloughing and secondary hemorrhage, Mr. Hilton on . 142
Wounds, surgical, Mr. De Morgan on the use of solution of chloride of zinc in 119
" Mr. Paget's cases treated with solution of chloride of zinc . . 120
Wright, Dr. H. G., on extreme flexions of the uterus 198
Xylo-styptic ether spray, Dr. Richardson on 264
Zymotic diseases, Dr. Polli on the sulphites and hyposulphites in . . 232

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Legal Liability in cases of Surgical Malpractice, in its Principles and Limits. By George C. Blackman, M. D., Prof. of Surgery, Med. Col., Ohio; and Stanley Mathews, Esq., of the Cincinnati Bar. 1 vol. 8vo.

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A Manual of the Principles of Surgery, Based on Pathology. For Students. By William Canniff, Licentiate of the Medical Board of Upper Canada, etc., etc., etc. Philadelphia. 1866. Price $4.50.

Diarrhœa and Cholera; their Origin, Cause, and Cure by Means of Ice. By John Chapman, M.D., etc., etc.

The Transactions of the American Medical Association. Vol. xvi. Philadelphia: Printed for the Association. Pp. 155.

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Asiatic Cholera; its Origin and Spread in Asia, Africa, and Europe, Introduction into America through Canada; Remote and Proximate Causes, Symptoms and Pathology, and the various Modes of Treatment analyzed. By R. Nelson, M. D. Pp. 206. $1.50.

Asiatic Cholera. By F. A. Burrall, M. D. New York. $1.50.

Recent Advances in Ophthalmic Science. The Boylston Prize Essay for 1865. By Henry W. Williams, M.D., Ophthalmic Surgeon to the City Hospital, Boston, University Lecturer on Ophthalmic Surgery in Harvard University, member of the American Ophthalmological Society, etc., etc.

McMillan & Co, London, are engaged in the publication of a great work entitled, **A System of Medicine**, edited by J. Russell Reynolds, M. D., F. R. C. P., London, Home Professor of Clinical Medicine in University College, etc. It is on the plan of Holmes' **System of Surgery**, and is to be completed in three volumes. The character of the work may be inferred from the table of contents of volume first, nearly ready, and advertised in the *London Lancet*, Feb. 24.

Proceedings of the American Pharmaceutical Association at the Thirteenth Annual Meeting, held in Boston, Mass., September, 1865; also, The Constitution and Roll of Members.

A Manual of the Principles of Surgery, based on Pathology; for Students. By William Canniff, Licenciate of the Medical Board of Upper Canada; M. D. of the University of New York; M. R. C. S., England; formerly House Surgeon to the Seamen's Hospital, New York; late Professor of General Pathology, and the Principles and Practice of Surgery, Univ. Victoria College, Toronto, C. W., etc. 1866.

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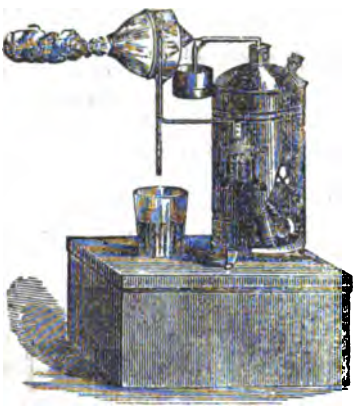

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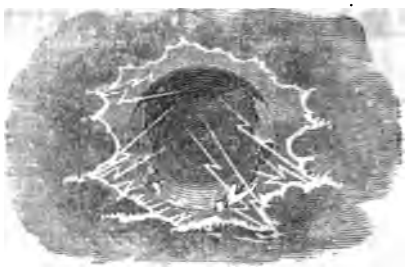

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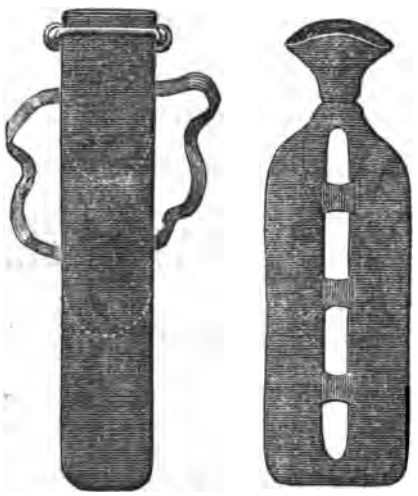

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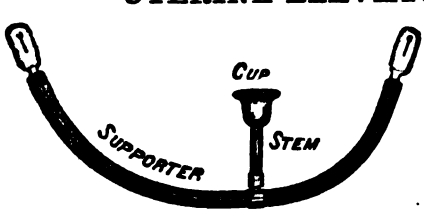

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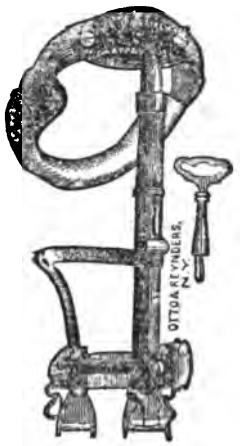

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Quiniæ et Ext. Belladon. [Quiniæ Sulph. 1 gr.; Ext. Belladon. ½ gr.]	1 90	9 25
Quinin et Ferri. [Quin. Sulph. 1 gr. Ferr. pr. Hydg. (Quevenne's) 1 gr.]	1 90	9 25
Quiniæ et Ferri et Strychniæ. [Quin. Sulph. 1 gr.; Ferri Carb. (Valett's) 2 grs.; Strych. Sulph. 1-60 gr.]	1 90	9 25
Quiniæ et Ferri et Valer. 2 grs.	3 50	17 25
Rhei. U. S. P.	75	3 50
Rhei Comp. U. S. P.	75	3 50
Rheumatic. Ext. Coloc. C. 1½ gr.; Ext. Colchici Acet. 1 gr.; Ext. Hyoscyam. ½ gr.; Hydg. Calor. Mit. ½ gr.]	90	4 25
Santonin, 1 gr.	1 00	4 75
Scillæ Comp. U. S. P.	50	2 25
Stomachic. (Lady Webster's Dinner Pills). 3 gr. [Pulv. Aloes Socot.; Gum Mastich; Flor. Rosæ]	50	2 25
Zinci Valerian. 1 gr.	1 00	4 75

GRANULES.

Acid. Arsenious, 1-20 gr.	40	1 75
Acid. Arsenious, 1-50 gr.	40	1 75
Aconitia, 1-60 gr.	75	3 50
Atropia, 1-60 gr.	75	3 50
Corrosive Sublimate, 1-12 gr.	40	1 75
Corrosive Sublimate, 1-20 gr.	40	1 75
Digitalin, 1-60 gr.	75	3 50
Elaterium (Clutterbuck's), 1-10 gr.	1 00	4 75
Extract Belladonna (English), ½ gr.	40	1 75
Extract Cannabis Indica, ½ gr.	60	2 75
Extract Hyoscyamus (English), ½ gr.	40	1 75
Extract Nux Vomica, ½ gr.	40	1 75
Mercury Iodide, ½ gr.	40	1 75
Mercury Iodide, Red, 1-16 gr.	85	4 00
Morphiæ Acet. ½ gr.	70	3 25
Morphiæ Sulphate, 1-10 gr.	85	4 00
Morphiæ Sulphate, ½ gr.	1 00	4 75
Morphiæ Sulphate 1-6 gr.	1 00	4 75
Morphiæ Valerianate, ½ gr.	40	1 75
Podophyllin, ½ gr.	50	2 25
Podophyllin, ½ gr.	50	2 25
Potass. Permangan. Cryst. ½ gr.	2 00	
Quinia Valerianate, ½ gr.		
Silver Nitrate, ½ gr.		
Strychnia, 1-20 gr.		
Strychnia, 1-40 gr.		
Strychnia, 1-60 gr.		

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This valuable and popular Medicine, prepared in conformity with the analysis of the water of the celebrated Seltzer Spring in Germany, in a most convenient and portable form, has universally received the most favorable recommendations of the medical profession and a discerning public, as the

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